CHEMICAL HERITAGE FOUNDATION

GEORGE A. OLAH

Transcript of an Interview
Conducted by

James G. Traynham and Arnold Thackray

at

Loker Hydrocarbon Research Institute
Los Angeles, California

on

3 February 2000

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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George A. Olah, interview by James G. Traynham and Arnold Thackray at Loker Hydrocarbon Research Institute, Los Angeles, California, 3 February 2000 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0190).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

GEORGE A. OLAH

1927	Born in Budapest, Hungary, on 22 May

Education

1945	B.S., organic chemistry, Technical University, Budapest
1949	Ph.D., organic chemistry, Technical University, Budapest

Professional Experience

	Technical University, Budapest, Hungary
1949-1954	Assistant Professor to Associate Professor of Organic Chemistry
	Hungarian Academy of Sciences
1954-1956	Head of Department of Organic Chemistry and Associate Scientific Director of Central Research Institute
	The Dow Chemical Company
1957-1964	Senior Research Scientist
	Case Western Reserve University
1965-1967	Professor and Chairman, Department of Chemistry
1967-1969	Chairman of Combined Departments of Chemistry (Case Institute and Western Reserve University)
1967-1977	C. F. Mabery Distinguished Professor of Research in Chemistry
	University of Southern California
1977-1977	Professor of Chemistry and Scientific Director, Hydrocarbon Research Institute
1980-present	Distinguished Professor of Chemistry
1983-present	Donald P. and Katherine B. Loker Distinguished Professor of Organic Chemistry
1991-present	Director, Loker Hydrocarbon Research Institute

Honors

1964	Award in Petroleum Chemistry, American Chemical Society
1967	Leo H. Baekeland Award
1970	Morley Medal

1972	Fellow, J. S. Guggenheim Foundation
1976	Member, U.S. National Academy of Sciences
1979	Award for Creative Work in Synthetic Organic Chemistry, American Chemical Society
1979	Alexander von Humbolt-Stiftung Award for Senior U.S. Scientist
1987	Michelson-Morley Award, Case Western Reserve University
1988	Fellow, J. S. Guggenheim Foundation
1989	California Scientist of the Year Award
1989	Roger Adams Award in Organic Chemistry, American Chemical Society
1990	Honorary Member, Hungarian Academy of Sciences
1992	Richard C. Tolman Award, American Chemical Society, Southern California Section
1993	Chemical Pioneers Award, American Institute of Chemists, Inc.
1993	William Lloyd Evans Award, Ohio State University
1994	Nobel Prize in Chemistry
1995	George Washington Award, American Hungarian Foundation
1996	Cotton Medal, American Chemical Society, Texas A&M University
1996	Kapista Medal, Russian Academy of Natural Sciences
1996	Inventor of the Year Ward, New York Intellectual Property Lawyers Assoc.
1996	Award in Petroleum Chemistry renamed “George A. Olah Award in Petroleum Chemistry,” American Chemical Society
1996	Golden Plate Award, American Academy of Achievement
1997	State Prize of the Republic of Hungary for Contributions to the “Fame of Hungary”
1999	Golden Medal of Charles University, Prague, Czechoslovakia
1999	Hanus Medal, Czechoslovak Chemical Society
2000	Cope Award, American Chemical Society

ABSTRACT

George A. Olah begins the interview with a description of his family and childhood years in Budapest, Hungary. Olah first developed an interest in chemistry after taking a chemistry course at Technical University of Budapest. While a laboratory assistant at the Zemplen Institute, Olah received his first patent on digoxin under the mentorship of Geza Zemplen, a carbohydrate chemist and former student of Hermann Emil Fischer. With Zemplen's approval, Olah began his work on organofluorine compounds. In 1949, Olah received his Ph.D. in organic chemistry from Technical University. That same year, Olah married Judith Lengyel. Olah joined the Hungarian Academy of Sciences in 1954 as the head of the department of organic chemistry and associate scientific director of the Central Research Institute. During a momentary collapse of the Iron Wall in 1956, Olah, his wife, and young son fled Hungary to take refuge with family members in London, England. Finally settling in Sarnia, Ontario, Canada in 1957, Olah became a senior research scientist at the Sarnia laboratory of Dow Chemical Company. Impressed by the work of Christopher Kelk Ingold, Olah turned his research towards Friedel-Crafts reactions, alkylations, and nitrations. After moving to a Dow facility in Massachusetts, Olah was offered the position of professor and chemistry department chair at Western Reserve University. Shortly after starting at Western Reserve, Olah aided in the coalescence of Western Reserve University and Case Institute of Technology, which now form Case Western Reserve University. Following twelve years of service at Case Western, Olah decided that he wanted to apply his chemistry to the broader area of hydrocarbons, so he accepted an offer from the University of Southern California in Los Angeles as a professor of chemistry and scientific director of the Hydrocarbon Research Institute, which was later named Loker Hydrocarbon Research Institute. Olah is currently the director of this institute. Olah concludes the interview with a discussion of the future of environmental chemistry, reflections on winning the 1994 Nobel Prize in chemistry, and thoughts on his family.

INTERVIEWERS

James G. Traynham is a professor of chemistry at Louisiana State University, Baton Rouge. He holds a Ph.D. in organic chemistry from Northwestern University. He joined Louisiana State University in 1963 and served as chemistry department chairperson from 1968 to 1973. He was chairman of the American Chemical Society's Division of the History of Chemistry in 1988 and is currently councilor of the Baton Rouge section of the American Chemical Society. He was a member of the American Chemical Society's Joint-Board Council on Chemistry and Public Affairs, as well as a member of the Society's Committees on Science, Chemical Education, and Organic Chemistry Nomenclature. He has written over ninety publications, including a book on organic nomenclature and a book on the history of organic chemistry.

Arnold Thackray is president of the Chemical Heritage Foundation. He majored in the physical sciences before turning to the history of science, receiving a Ph.D. from Cambridge University in 1966. He has held appointments at Oxford, Cambridge, Harvard, the Institute for Advanced Study, the Center for Advanced Study in the Behavioral Sciences, and the Hebrew University of Jerusalem. In 1983 he received the Dexter Award from the American Chemical Society for outstanding contributions to the history of chemistry. He served on the faculty of the University of Pennsylvania for more than a quarter of a century. There, he was the founding chairman of the Department of History and Sociology of Science, where he is the Joseph Priestley Professor Emeritus.

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INTERVIEWERS:	James G. Traynham and Arnold Thackray
LOCATION:	Loker Hydrocarbon Research Institute, Los Angeles, California
DATE:	3 February 2000

TRAYNHAM: Professor Olah, I know from things I've read that you were born in Budapest, Hungary, on 22 May 1927. I'd like for you to tell us something about your childhood—identify your parents and something about the home situation in which you grew up.

OLAH: Well, as you mentioned, I was born in Budapest, Hungary between the two world wars. My father was a lawyer. My mother was a homemaker. To the best of my knowledge, nobody in my family ever had any interest in science. I remember my childhood as being quite happy. I really appreciate that I got a very good education. As you know, Hungary was split off at the end of World War I from the Austro-Hungarian monarchy, and one thing that this little country inherited was a reasonably good educational system. I must emphasize that this wasn't homogenous across the country, but in Budapest there was a number of very good elementary, middle, and high schools. I really enjoyed getting a good general—what you would now call—liberal arts education. Science never played an important role in my education at this stage. I can't even remember who my chemistry teacher was in high school. I remember, however, my physics teacher, who was a very interesting man and had a substantial impact on his students. Growing up, I was primarily interested in history, languages, philosophy, and many other things. I have never regretted it. You know, once you get involved in the sciences or chemistry, the time pressures are such that you inevitably become focused and specialized. Obtaining a fairly broad-scope education with an interest in many things serves one very well. What else can I tell you about my childhood? Of course, these were not easy years in Central Europe.

THACKRAY: Were you an only child?

OLAH: No. I had a brother who died at the end of the War [World War II] in Russian captivity.

THACKRAY: An older brother or a younger brother?

OLAH: He was an older brother.

TRAYNHAM: What were the names of your parents?

OLAH: My father was Julius Olah and my mother was Magda.

TRAYNHAM: What was your father's occupation?

OLAH: He was a lawyer.

THACKRAY: In a practice or on his own?

OLAH: Yes, he was in practice on his own.

THACKRAY: So, can you talk a little bit about the social circle? You were living in the city.

OLAH: Well, you know, that's an interesting question, which I don't want to stretch out too much. Hungary is a small country with a current population of about ten million. At the time, it was substantially different, but it had a significant capital city. The population of Budapest was, I guess, a million. In a way it's similar to Austria, which has a total population of about seven million and a capital city of two million. This happened because the Austro-Hungarian monarchy was a large entity, and once the country shrunk to half, the capital city stayed there. So Budapest, not unlike Vienna, was not really characteristic for the rest of this small country. We lived in the capital city, but I didn't know much about the rest of Hungary. It was a major event to go into the countryside by train and so on. Very few people had cars. We never had a car. We really lived in a city that had a fairly high concentration of culture, schools, and so on. This environment was fairly westernized with substantial emphasis on cultural things. Franz Liszt, who was Hungarian by birth, could never speak Hungarian. [laughter] When he became successful, he gave a music conservatory [New Hungarian Academy of Music], which became an outstanding school, you may have heard of it, as a gift to his native land. A not insignificant number of later well-known Hungarian conductors, composers, and musicians attended it. I believe the reason, to a substantial degree, was that it was a very good music school, which also inspired its students. Most of them came out of this one school. I guess—and it's also frequently mentioned—there is also a significant number of Hungarian scientists, who in some way or another have achieved something. It is frequently questioned whether or not Hungarians have special talents in certain fields like science, music, and so on. I really don't believe that people are genetically different depending on one country or the other. I feel, however, that a significant impact on Hungarians, and particularly Hungarians who lived in the capital city—and of course, this was a significant proportion of the people—was the good fortune that there were some very good schools available. This made a major impact. Of course, you know, we are talking about this many years after my school years. When you are a school kid, you look at these things differently. In Hungary, at the time, we had a German-type school system. There were the schools called "gymnasia," which combined middle and high school into an eight-year program. For eight years, I went to a school of the Catholic order called Piarist Brothers. They ran some very good schools, top schools. We had to go to school six days a week from 8:00 am to 1:00 pm or 2:00 pm. On Saturdays, school ended may be an hour earlier. The rest of the days were taken up with homework and so on. This was a very strict regimen, but I think it served us well. I don't know whether I could fully appreciate reading the classics in Latin at that time. I took Latin for eight years. It was a very rigorous program. It gave me a foundation to learn other languages more easily—it's a very structured language. It's like mathematics. If you are exposed to a rigorous training in mathematics, it affects your thinking for the rest of your life. As I said, I have no memories of being particularly intrigued by chemistry, but I think my general education served me well. I was and still am an avid reader of various topics outside chemistry.

THACKRAY: Was there a competitive entry for this school?

OLAH: Yes, it was very competitive. I was a fairly good student all my life. My wife is constantly kidding me that I was what you would now call a "nerd." I think that it was mostly interest that drove me. I don't necessarily believe that you inherit abilities or habits, but I think a lot depends on your interest, and the effort you put in. Of course, there were fewer distractions at the time. There was no television, no other things, so I read a lot.

THACKRAY: Was your family at all musical? Did you have musical interests?

OLAH: No, nobody in my family was very musical.

THACKRAY: What was the family recreation or was your father usually working?

OLAH: Well, I remember that my father liked sports. He was involved in all kinds of sports. He skied and so on. I did some of that. I was also on the crew later on and played soccer, but nothing very serious.

THACKRAY: Did you live in a house or in an apartment?

OLAH: We lived in an apartment. Now, that's interesting. We lived in an apartment house next to the opera. You asked about music. I don't think there was a family tradition, but we happened to live in a house next to the Budapest Opera House and quite a few members of the opera company, from singers to musicians and so on, lived in the same apartment building. I was exposed to opera through friends. I'd gone frequently to the opera, including rehearsals. I remember from my childhood—things come back—one particular rehearsal. Some friends took me—there was this gentle, smallish conductor who was a very pleasant person <u>except</u> that when the rehearsal started he instantly transformed into a living terror, terrorizing musicians and singers alike. [laughter] I learned that his name was Arturo Toscanini! [laughter] I was exposed to music even if I had no musical ability. I never learned to play an instrument.

TRAYNHAM: Well, after you completed your work in the gymnasium, where did you go for further education?

OLAH: Well, you see, this was just at the end of the War, and with all my interest in areas that I really loved to read about, the realization came that I better start thinking about how I would make a living. Obviously philosophy, Hungarian history, and many other things were not promising to allow one to do much. Later on I read about another fellow Hungarian born scientist, Gene [Eugene J.] Wigner who was a physicist at Princeton [University]. He had, even in high school, an interest in physics, and one day his father, who was a businessman, asked him, "Now tell me what you would like to do with your life." Wigner answered that he would like to become a theoretical physicist. His father said to him, "That's nice, but do you have any idea how many jobs there are in Hungary for theoretical physicists?" The son supposedly replied, "Two, maybe three." At which the conversation ended, and he was persuaded to enroll to study chemical engineering. Eventually, however, he changed over to his original field of interest. My parents didn't put me under any pressure, but I realized that it would make more sense to try to study something that also had realistic possibilities for the future. When I took my first university class in chemistry, I was really sold on it. I fell in love with it. Don't ask me why, but maybe what intrigued me was that chemistry had a wide scope. There are so many things that with some knowledge of chemistry you can do, and of course, you never foresee, particularly in your younger days, where life will carry you. Anyhow, I got really involved in studying chemistry by more or less eliminating other fields. It interested me and also looked very practical to pursue. Once you fall in love, that's it. I'm still in love with chemistry.

THACKRAY: This first class in chemistry—at what age were you, and where was that?

OLAH: This was just at the end of the War, in 1945, at the Technical University in Budapest.

THACKRAY: Go back a minute if you would into the 1930s, which were not great times, and into the World War II sequence and how it affected you.

OLAH: I was lucky. I have gone through it. I was eighteen years old when the war ended. These were difficult times, but I survived and my parents survived, including the siege of Budapest, which destroyed much of the city. When I started at university, it was under difficult conditions. Our chemistry building was half destroyed. It had no windows and so on. It taught me that you can do things even under very harsh conditions, and that it will eventually serve you well.

THACKRAY: Did you and your parents stay in Budapest the whole time?

OLAH: Yes.

TRAYNHAM: When you went to the Technical University in Budapest, did you have financial aid, scholarship aid, or were you on your own?

OLAH: Well, you know, this was a very different time. I don't remember what tuition was, but it wasn't a significant factor. I lived at home, so the cost wasn't a major factor.

THACKRAY: How did your brother end up in a Russian prison?

OLAH: The Russians, near the end of the War, indiscriminately collected civilian people as prisoners. A lot of civilians were just picked up to fill up numbers, and he ended up in a prisoner camp where he perished.

THACKRAY: Weren't you at some risk yourself?

OLAH: Oh, everyone was. I mentioned, and this is funny, my connection with living next to the opera house. Friends got me a certificate saying that I was a member of the Budapest Opera House. The Russian soldiers respected few things, but being a member of the opera house was one thing that they did respect. Little did they know that my duty was to help clean rubbish and move around pianos. [laughter] For some months I was a member of the Budapest Opera House.

THACKRAY: Was your father able to maintain his employment through all these time sequences?

OLAH: After the War, he was employed as a lawyer for the National Railways with a modest salary. Going to the university wasn't financially a burden, and I don't think there was a scholarship system at the time. Later on, of course, this changed. I was lucky that I was quite young. I was accepted as an assistant in the institute of my professor, [Geza] Zemplen, who was a student of the famous [Hermann] Emil Fischer. Emil Fischer was one of the major organic chemists of the twentieth century.

TRAYNHAM: Was Professor Zemplen the one who taught you that first exciting chemistry course?

OLAH: No, not really, but you know, this was a different time. He was a very traditional person who taught chemistry in the vein of Fischer, which at the time was quite outdated. I'll come back to this in a minute. He brought back the Fischer tradition to Hungary. Fischer had many students of his who became professors, not only in Germany, but also in the rest of Europe. As you know, students, in some way, frequently imitate their professors, for better or worse. Thus Fischer's students spread Fischer's view of many things. Fischer, I understand, was quite a wealthy man. Part of it was due to the fact that he charged his graduate students very significant fees for the privilege to be able to work with him. Zemplen told us that Fischer always interviewed potential new doctoral students in the presence of their family, and he had on the wall a chart showing where his former students were. Many became professors or others in high positions in industry. Fischer told the family that it was the best investment they could make for their youngster, because if he accepted him—girls were not frequently considered—his career would be assured, even if they needed to sell or mortgage the farm. Anyhow, I am mentioning this because when I started to work as a young assistant for Zemplen, he must have been somewhat pleased with me because there was never any question of my getting anything. It was a great privilege to work for him and I was not obliged to pay him.

TRAYNHAM: Was that while you were still an undergraduate student that you started working with him?

OLAH: I got into the [Zemplen] Institute in 1947, when I was still an advanced undergraduate, then I started my thesis research. Occasionally, I have told my students, my graduate students, this story trying to impress them with the fact that in my time it was a great privilege to work for a known professor—I don't know how many long hours a week. I, however, have never considered chemistry to be work. This generally has no effect whatsoever on my students! [laughter] They thought it was a very strange thing. Anyhow, Zemplen did a lot of good for chemistry and his students in Hungary, yes. He was the first professor of organic chemistry at the Technical University. These, you probably know, in Germany or Europe are universities where, for example, chemistry was taught not very differently from other universities. In fact, chemical engineering was really the study of chemistry with very little engineering aspects, which is something entirely different from what it is in the United States. Zemplen was, in many ways, a product of his past, and therefore, the chemistry he taught at the end of World War II was the type of chemistry that Fischer had taught fifty years earlier. We never even heard anything about mechanism, atomic theory, or anything like this. I, therefore, was self-educated in all this. When I became a young assistant professor—I taught the course around 1952 or 1953 for the first time about theoretical organic chemistry—what was really mechanistic organic chemistry. We learned a lot of practical chemistry and natural product chemistry. Fischer, I think, can also be credited to have started biochemistry, single-handedly. Then, however, he also gave it away, because he believed that decent chemists should only deal with compounds, which they could isolate, crystallize, analyze, and so on. However, there were also practically no physical methods available at the time, or even when I started chemistry and my research. I left Hungary in 1956 and up until this time, for example, in the whole of Hungary there wasn't a single infrared spectrometer. It was rumored that the secret police had one, but fortunately I never found out about it. [laughter] We did microanalysis on our isolated products ourselves. All of Zemplen's graduate students and associates were taught to do elemental combustion analysis. Besides, you learned other things like it's very necessary to have clean, well-isolated, crystallized compounds and so on. It was a very strict regimen, but it was a basic Hungarian version of Fischer's school.

THACKRAY: How many people were there in the chemistry group?

OLAH: Well, when I started—I'm talking first of my undergraduate experience—in chemistry there were maybe eighty students. At the end of the first year, half of them were eliminated. The first year was considered a very cruel and tough way to weed out about forty, which was realized as a necessity because the lab facilities were so limited that they couldn't accommodate more. Organic lab was a Fischer type of lab. I don't know whether Gattermann preparations mean anything these days, but Gatterman's organic preparative text was the Bible for laboratory education in Germany and also in Hungary. We did, I think, forty or fifty Gattermann preparations. You learned very early that chemistry is an experimental science, and even with limited facilities we got a very good start.

THACKRAY: This was the system in which, even as an undergraduate, you were spending most of your time doing chemistry?

OLAH: Yes, in the Zemplen Institute, including doctoral students, there were maybe twenty-five people. It was financed in a very peculiar way. As mentioned, there was very little tuition, but there was also very little university money, so Zemplen more or less financed his chemistry laboratory by himself with his research related income. He also helped his associates and students. I remember instances of this—these were difficult years when people were really living on a shoestring. One of the products of Hungary, which was a famous export item, was salami, and it was manufactured in Szeged, the city where [Albert] Szent-Györgyi came from. After the war, they restarted production for export and they wanted a certificate from a well-known professor on the product, so they came to Professor Zemplen who asked for a "suitable sample." He asked for some hundreds or kilograms of salami, and the whole Institute lived on this sample for weeks. At the end of which, he wrote a credible certificate to the manufacturer that the salami was quite satisfactory! [laughter]

THACKRAY: You've got all sorts of things happening politically. How was he deriving the monies he needed?

OLAH: Well, you know, in Hungary he was the leading chemical authority, so he was consulting. This was, of course, before the communists completely took over, then things changed and they posted "State Controlled" on everything. Up to this point, he was a consultant for the pharmaceutical industry. He got paid for it, not only personally, but for example, they provided chemicals, equipment, and so on. He basically maintained his laboratory by all means, like a little private enterprise.

THACKRAY: George, just chronologically, the years from about 1943 to 1948 were very important, I would think, in your own intellectual formation. Those were just terrible years in which to be not only physically surviving, but also receiving intellectual information from around the world. I mean, what literature did you have? What textbooks? How did it work?

OLAH: Well, it was very difficult. I have recollections, for example, that Zemplen maintained his own library in the Institute, which may have had some influence on me because with my wife, we are managing to initiate a library [George and Judith Olah Library in The Loker Hydrocarbon Research Institute] here. I have always felt that it is essential for chemists to read a lot and also to have direct access to literature, journals, and etcetera. When I started in chemistry in small, isolated Hungary there was a very difficult problem of learning what was happening in the world. There were only a few chemistry journals available, mainly based on personal subscriptions. It was very difficult. When the communists took over in the period up to 1956, we were even more isolated because there was not even access from the Soviet Union, which may sound surprising, but it's a fact. If you could get hold of *Chemical Abstracts*, you learned from *Chemical Abstracts* what was happening in the Soviet chemical literature. We were really isolated. At the time, I met only a few foreign chemists. One contact I had early in Hungary and maintained until he passed away many years ago was with Costin Nenitzescu from Romania. He was a truly great chemist. On the few occasions he visited, we commiserated the fact that we had poor facilities, very limited access to literature, and were very much cut off from the rest of the world, which was bad enough in general, but even worse in science. This was one of the reasons that many young Hungarians, who when the opportunity arose in 1956 for a brief moment while the Iron Curtain was thrown open, felt that they had no future, not only because of political reasons and lack of freedom, but also because of this feeling of complete isolation, which is, of course, in the sciences and chemistry, a terrible thing. So when you ask me—most of my chemical education came from self-study and reading.

THACKRAY: But could you read enough?

OLAH: It is difficult to define what is enough. [laughter] I speak a number of languages, and this made things easier. German was basically a second language in Hungary, so I learned German, really, as a child. I also learned French and English, at least at a workable level. I spent much of my time in libraries. Concerning libraries, I remember I had a friend who was a librarian in the university library. From him I learned that when the communists took over, the first book they removed from the library was [Karl] Marx' *Das Kapital: Kritik der politischen Oekonomie* (1). The early editions of Marx were prohibited because it was republished so many times under the guidance of the Soviet regime, that what was the official version had not much relation to the original. [laughter] Spearing of books and literature, a small country like Hungary, besides musicians, conductors, or even scientists, produced many very fine writers and wonderful poets. The trouble is that with somebody writing in Hungarian, which is a language not related to any of the major language groups, but belonging to the Finn-Ugric family—a Hungarian writer or poet has a very difficult time because his work is not accessible. But on the other hand, much of the Western literature was translated into Hungarian. This, considering the small country with a small market, was quite amazing. Not only classics, but also novels and so on, were translated and easily available. Hungarian writers had great talent. One Hungarian poet, János Arany, who was also the secretary general of the Hungarian Academy of Sciences, was a wonderful poet. He also was an outstanding translator of [William] Shakespeare. His translations and those by other Hungarians, who translated poetry, were really remarkable. I am told that Arany, this <u>great</u> translator of Shakespeare, couldn't speak a word of English [laughter] because he had only self-educated himself phonetically to be able to translate.

TRAYNHAM: He could read, but not speak?

OLAH: Yes, that's right.

TRAYNHAM: I want to inquire about your association with Professor Zemplen. He was, I believe, a carbohydrate chemist.

OLAH: Yes.

TRAYNHAM: As an assistant in his laboratory, were you engaged in carbohydrate chemistry early on?

OLAH: Yes. As a matter of fact, I started as a *bona fide* natural-product chemist, and it served me very well. The Hungarian patent office, I am told, some years ago celebrated its hundredth anniversary and they had a commemorative booklet published, in which they reproduced a 1950 patent of mine with Zemplen relating to the isolation of heart glycosides. This involved a process that I worked out to make digoxin. As you know, this is still a major heart medication. The plant called "foxglove" grew well in Lake Balaton in Hungary. For some reason, it's an ideal place to cultivate this plant. One of Fischer's other students, Alfred Stull, who became a famous chemist and a major force behind the Swiss pharmaceutical industry, worked out processes to make digoxin and digitoxin from the leaves of foxglove, which they imported from Hungary. As part of my work in glycosides, I worked out a much-improved process, for which Zemplen and I obtained the patent, which was, I understand, used for many years quite successfully by the Hungarian pharmaceutical industry. But I had my own ideas, even in my early days, about chemistry. It was through my work in carbohydrates and glycosides that I got the idea to use fluorinated sugars in coupling reactions. I read a paper by the German carbohydrate chemist, Michel, who described that he could make the free fluorohydrides of simple sugars. Halohydrides of sugars always have protected OH groups, for example, acetyl bromoglucose. But with the fluorine derivative Michel made, he was able to deacetylate the system and obtain the free fluorohydrin. This gave me the idea that there may be a way to make either alpha or beta glycosides by using fluorinated sugars anticipating 100 percent Walden inversion, whether you deprotect before or after the coupling. My interest thus started in organofluorine compounds and their chemistry.

THACKRAY: When was that, George?

OLAH: This was around 1950. Professor Zemplen, let's put it mildly, thought little of my idea. He not only felt that any chemist who wanted to work with the strange fluorine compounds must be crazy, but, probably rightly, that Hungary, at the time, wasn't the place to try this at all, because we had nothing in the way of the needed chemicals or equipment. In order to start, I made my own HF [hydrogen fluoride] from feldspar and sulfuric acid. I also made my own fluorosulfuric acid, boron trifluoride, and many other things. Anyway, I started to change my interest from carbohydrate and glycoside chemistry.

Now, there's just one other thing that I'll mention about Hungarians. Obviously, my early patent was not shaking the world, but the Hungarian patent office, in its mentioned brochure, had other patents related to Hungarian-born inventors. In the 1920s there were two Hungarian patents issued for an improved refrigerator. One of the inventors was Leo Szilard, who was the most ingenious physicist. This patent was for a thermo-electric refrigerator, which never got off the ground, but many years later Electrolux [Company] in Sweden basically used the principle. But more interesting is the co-inventor on this patent. I don't think that you would have guessed that the co-inventor was Albert Einstein. I am told that they both had great hopes for this patent because they both always needed money! [laughter] Szilard never had a permanent job. He was struggling, and Einstein also had financial pressures. Regretfully, they never made any money off of their patent.

TRAYNHAM: What was the date of that patent?

OLAH: I think it was in the late 1920s. Now, as fate has it, Szilard, later in the 1930s, applied for a patent in England, which was kept—because of the approaching war situation—secret, but after the war, he obtained a broader U.S. patent with [Enrico] Fermi (2). This patent was not for the thermoelectric refrigerator, but for a thermonuclear device. [laughter] So Szilard has patents on both the thermo-electric refrigerator and the thermonuclear bomb. [laughter] I understand he tried to sue the U.S. government for infringement because the Manhattan Project supposedly infringed on his invention. However, the atomic energy law [Atomic Energy Act of 1946], which congress passed, exempted the U.S. government from any infringement or liability (3).

TRAYNHAM: At the time that your interest in the fluorinated carbohydrates emerged, were you reading about organic-reaction mechanism studies that were taking place in other countries?

OLAH: Yes. I was reading. I'll tell you, my mother-in-law, who was widowed and remarried after the War and lived in Canada, sent me, on occasion, some books. My two prized possessions, vintage 1950s, were Christopher [Kelk] Ingold's *Structure and Mechanism* (4) and Michael [J. S.] Dewar's book on organic mechanisms (5). I cherished these books. When we escaped in 1956, with the shirts on our backs and my little son [George John Olah] who was two-and-a-half years old, practically the only other possessions I brought out were these two books, which I still have.

THACKRAY: Go back to when you were twenty-two, twenty-three, twenty-four, George, how many other assistants like yourself were there in the Institute?

OLAH: Maybe three or four.

THACKRAY: All working on comparable areas?

OLAH: Well, Zemplen had a very strong personality, in the German tradition. The old Prussian, German professors were really running the institutes with an iron fist at the same time.

I also consider myself to be a reasonably strong person. The point is that if I made any impact on Zemplen, it may have been for the reason that at one point he—informally but—blessed the fact that I could pursue my own interests.

[END OF TAPE, SIDE 1]

OLAH: Doing carbohydrate chemistry, isolating compounds, purifying, crystallizing them, etcetera—a clean, well-developed organized way. When I got involved trying to make organofluorine compounds including my own HF and so on, this was not something that was easily tolerated. Around 1950, I got my first university appointment with a salary, which by the fifteenth of the month was all gone, but we were young, healthy, and motivated to pursue chemistry.

THACKRAY: You were appointed as what?

OLAH: I was the equivalent of an assistant professor including teaching duties. I was teaching my own courses, eventually on theoretical mechanistic organic chemistry. In the chemistry building, on the top floor, there was an open balcony looking out over the Danube River, which was used for some storage. I had the idea that it could be used for my research. Zemplen eventually let me have this balcony. With some of my early and enthusiastic co-workers, we enclosed it and converted it into a laboratory with a couple of primitive fume hoods and so on. We had our own little laboratory. It was soon called "the balcony laboratory." I visited the place some years ago, but unfortunately, it has been much improved. [laughter] It was an interesting place. And we did work.

TRAYNHAM: Was it just natural atmospheric conditions, or did you have heat back then?

OLAH: Well, you know, the heat of a single gas burner exhausted our fume hoods, which provided the draft. Not a very efficient operation, but that's what it was.

TRAYNHAM: I mean, out on the balcony?

OLAH: As I mentioned, we enclosed it. There practically was not much heating, except from Bunsen burners.

THACKRAY: What was the natural career expectation of someone in your position at that time? It's the Eugene Wigner question, as it were.

OLAH: Well, the career expectation was that eventually you might become a full university professor, which sounded better than it really was, because of the limitation of the working conditions. Now, in physics, particularly if you were a theoretician, it was all right. Other areas of physics, of course, were also very difficult in a small, poor country. Chemistry, of course, was better, because with some imagination you could find areas where with a little means you could pursue your work. I learned one thing early that has helped me in my life: never complain about work conditions. I always tried to adjust. My interests were always broad and I realistically adjusted to what could be done. I guess even now, if they locked me up in a simple lab, I would still find topics that I could work on with whatever was available in the lab and find some way to do my research. I was blessed that I was able to attract fine young people, who, for whatever reason, felt that working with me was interesting. One of my earliest young associates who joined me in the Budapest "balcony laboratory," was Atilla [E.] Pavlath, who was recently elected as President of ACS [American Chemical Society]. Limited facilities to a degree are like money, if everybody around you is very poor, you don't necessarily feel that you are so poor. If you have very poor working conditions, and everybody else has the same, you don't feel under-privileged. Of course when I got out into the free world, I soon realized the advantages of better working conditions, and I made full use of them. I'm still overwhelmed by my adopted new country, because the U.S. is still the country of unlimited possibilities.

THACKRAY: What did your father and mother think about what you were up to?

OLAH: My parents gave me complete freedom. I don't remember if they had an influence on my career selection. They felt that I should find my own way in life. You could argue whether over-protection—many parents trying to do everything for their children—is the best way, or leaving them to find their own way is. I feel that it worked out all right for me.

THACKRAY: But in, say, 1952, when you were twenty-five, George, did all this look as if it was going somewhere interesting from a worldly point of view?

OLAH: Oh, I don't know. Going back, I lost my mother early in life. She died in early 1950, and my father then remarried. So family wise, I was very much on my own. I was very, very lucky that early in life I found a wonderful wife [Judith Lengyel Olah] and we were married in 1949.

THACKRAY: Do you want to just say briefly something about Mrs. Olah and how you met?

OLAH: Well, as I said, last year we celebrated our golden wedding anniversary. We met in 1943, during a summer vacation. It was a long while ago. We got married in 1949. After we married, I convinced my wife that she should study chemistry, and she eventually did, but I am not sure if she has ever forgiven me for it. We may have had different ideas about it. Being a scientist is very time demanding. I think that if I achieved anything, it may have been because of the balance between my private and professional life, which was not easy. I have always felt that if my wife could better understand what I was doing and vice versa, this would ease the pressure. It worked out well for us, except for a period when our children were growing up. Judy worked with me for many years. She, however, is more sensible than I am. She retired ten years ago. We had a very good time together in research and she has helped me in many ways.

THACKRAY: So around about 1950 you found a little apartment for the two of you, or where did you live?

OLAH: We had the good fortune that we had a one-room apartment, and we were very happy. It was a tiny little place, but it was home.

THACKRAY: How far away from the university was this?

OLAH: Well, it depends on how you define it. [laughter] By streetcar, which in the winter was freezing cold, maybe forty minutes, but we were young and it did not bother us. Many things that you can't imagine doing now were quite reasonable to you at the time. I remember that during the Hungarian uprising, our laboratory was in a sublease in an industrial research place quite a distance away. We walked, both ways—I don't know how many miles—a lot of miles. So hardship, you see, when you are young, and everybody has the same hardship, is very different. My older son was born in Hungary and we had a very happy family life there.

THACKRAY: What year was he born?

OLAH: He was born in 1954.

THACKRAY: In 1954 what income did you have?

OLAH: Well, you know, I was a young assistant professor. I remember I was paid six hundred Hungarian forint, which by the official course was maybe fifty dollars, and by mid-month we had practically nothing, but everyone we knew was in the same boat. Quite frankly, it never crossed our minds how poor we were. It was in mid-life—I'm not very good about finances or economics, my wife has always handled this—but it took me many years to realize that having some financial stability is a necessity for a family. In my younger days, I was even worse, and it never crossed my mind whether or not there was a material future in what I was doing in my career. You asked me about job outlooks. I guess the outlook was always for a modest economic living, but it never crossed my mind as a significant aspect. Maybe I was foolish.

THACKRAY: In those years from 1950 to 1956, economically and socially with the communist takeover, was the country going forwards, backwards, or sideways? I mean, what was the mood? What was the reality of that?

OLAH: The mood was basically—we were locked in. It was a big prison. Maybe it wasn't a prison that had bars everywhere—the fact that there was practically no ability to communicate with the outside world. As we discussed, you were isolated in many ways. This was enormously depressing, but again, you see, everybody else was depressed. In addition, I was always preoccupied to such a degree pursuing my chemistry that this was my life, and outside of it there was very little else.

TRAYNHAM: A couple of years after the birth of your first child, the 1956 revolution took place. What, if any, was your involvement in that? What led up to your decision to leave Hungary at that time?

OLAH: This was an absolutely spontaneous event. I haven't lived it through from the perspective here, but it's quite obvious that it wasn't organized, influenced, or whatever. There was increasing criticism of the regime expressed, which Radio Free Europe and others encouraged. We were listening to these broadcasts, obviously. They tried to give encouragement, maybe foolish encouragement. We now know that there was never a prayer of a chance that the West would have helped, but it was out of the question that the revolution was started because of that. It was started because young people, the general population, were completely fed up with the regime. It reached the point where it exploded without any preparation, without any reality or thinking about it. We were caught up in this as young people, and foolishly, we really never thought about how this would end. When it was crushed, there was a realization that the future was bleak, even bleaker because very few people believed that in the foreseeable future there would be change. Therefore, a lot of people, particularly young people, decided that given the opportunity since the Iron Curtain was temporarily down they would take their chances and leave. Two hundred thousand Hungarians walked away in these few weeks to start a new life in the West.

TRAYNHAM: There was no barrier to your leaving at that particular time?

OLAH: Nobody knew. I personally feel, in retrospect, that the Russians, if they wanted to, they could have stopped us. But they realized that this, being an explosive situation, was the best thing to let the extra pressure out, in order to manage the situation better. I'll tell you, we hadn't preplanned anything. We were caught up in the events, and when we left, we left without any consideration. It's not like we had decided to move from one city to another and had carefully considered our possessions, our chances. We really walked off with nothing in the hope that we would be able to restart our life. For a small country all this was a very great loss.

TRAYNHAM: You mentioned earlier that you left with the shirt on your back, essentially. Was that because it was such a hurried-up departure, or were there barriers at the border that would have prevented you from carrying your worldly goods?

OLAH: No. First of all, we knew of nothing definite. You know, we really knew nothing. We still were very much isolated. The only thing was that occasionally you could listen to the radio. The radio wasn't giving us any practical facts or advice. Nothing was important. In the final analysis, you reach a crucial realization that it's the life of your family that is important. Nothing else. I mentioned that besides my family I came out with two scientific books. Completely irrational, no? I mean, I could tell you of other things that could have been much more useful. But basically, similar things happened to a lot of other people. The great thing was that most people, including us, were young. When you are young, you don't think much about material things and you are not really afraid to start all over.

I must tell you about my own experience. I think it's an experience many people went through. We really started <u>all</u> over. I thought that I already had some reasonable background and achievements in chemistry at the time. I was an early starter and I had—I don't know exactly, but maybe a hundred publications, and some of them were reasonably well known. I mentioned to you that I started work in organofluorine chemistry and then extended it to Friedel-Crafts chemistry. The fluorinated sugars I originally worked with are basically acryl-fluorides, so it crossed my mind that it would be interesting to look at how acryl fluorides and later alkyl fluorides could be applied in chemistry. Then I started to study acid catalyzed reactions in a more general way. This was really the beginning of my work on carbocations. We did all this work in Hungary under difficult conditions. I started to publish, but only in Hungarian journals, although one had English translations or summaries. One day out of the blue, I got a letter from Hans Meerwein, a great German chemist who was my idol in chemistry. To my amazement, he had somehow read some of my papers. I was a young unknown chemist working behind the Iron Curtain, but he felt that it was worthwhile to correspond with me. This was great encouragement. One day I got a present from Meerwein, an unbelievably valuable present that affected my work. The present was that somehow through friends in industry he had arranged a cylinder of boron trifluoride to be sent to me. This also reflected on the man. He was a world-renowned chemist, very established in his career, and he cared about an unknown, young chemist somewhere in the boondocks, to a degree, which I think reflected his humanity.

TRAYNHAM: Which border did you cross while leaving Hungary?

OLAH: Oh, there was only one border to the West.

TRAYNHAM: Through Austria?

OLAH: Yes, there was only one border. But when we got out, my wife had family in England who helped us. We got, before Christmas, to London, to my wife's relatives. While there, I visited and met, for example, Christopher Ingold, another of my idols in chemistry. Much was written about Ingold, the man, too. Many people felt that he was a very strong, maybe even arrogant person. He had used strong words about others in his writing. In my experience, however, he was a wonderful, caring human being. Again, I published in Hungary, just before we fled, some work on nitration with nitronium salts (6). I managed, in Hungary, to make nitronium tetra-fluoroborate as a stable salt. I knew, of course, about Ingold's work. Ingold reported in his classic series of papers in 1950 on nitronium perchlorate (7). We made, however, the tetra-fluoroborate as a very stable compound, and I started to do nitration with it. Some of this work was published in 1956, just before we escaped (8). Ingold knew about it, and he felt that it was worthwhile to try to help a young guy, which again, convinced me that there are decent people in life. We were lucky because we made the nitronium tetra-fluoroborate salt, which was very stable, and it's still used and is commercially available. Little did I know that Ingold never followed up on his work on the perchlorate because the perchlorate salt is unstable. He had a vial of it, which exploded one night, and Ingold felt it was not safe and worthwhile to pursue this. Talking about nitronium salts, the cation is perfectly all right. It's sometimes the anion that causes the problems. [laughter] Perchlorate has the ability to form covalent esters, so in Ingold's nitronium perchlorate, it was anion that caused the problem. But again, little did I know about it, but some of my early work got published and people knew about it (9). Anyhow, I think, immigrants all start from scratch and there is a justification for it. Coming from faraway places without an established background is quite difficult. Although, I consider myself the scientific grandson of Emil Fischer, but certainly in the field of preparative and physical organic chemistry, ionic chemistry, or whatever I was very much on my own.

THACKRAY: Before you came west, George, where had you traveled outside of Budapest?

OLAH: I had a chance in 1955, to give a few lectures in East Germany, and I was once in the West at the IUPAC [International Union of Pure and Applied Chemistry] Congress, I guess in 1955, in Zurich. I gave a lecture, and it was a great experience, because I met in real life some of the people I had only heard about. But this was it, nothing else.

THACKRAY: How did you experience London as a place to be?

OLAH: Of course, when we got out, after what we had gone through, we were in a state of mind shock. This affected us all at once. You really don't think in entirely real terms. We started a new life, we accepted it, and we never looked back. I think it's a fortunate thing in life that you can try to look ahead and not look back.

THACKRAY: My memory is that at that moment, countries like England were—today, with the particular case of England, you'd probably be turned back at Heathrow Airport, but at that time—

OLAH: Oh, for us it was paradise!

THACKRAY: No, but I mean just that you were allowed in, weren't you?

OLAH: They provided visas for Hungarian refugees. The whole world was very decent, really. There was a genuine feeling of trying to help, and in a way, I think our timing was fortunate. These were economically good years. We were welcomed. People were very decent and helpful.

THACKRAY: So how long then did you stay in England?

OLAH: We stayed until the spring, maybe three or four months, with my wife's relatives.

TRAYNHAM: Did you find employment during those three or four months?

OLAH: No. We never were really planning to stay in England.

THACKRAY: You knew you were heading to the Americas?

OLAH: We thought that when starting a new life, we should go where we intended to live permanently. There were many very decent people helping us. There was a Miss Esther Simpson who was running the society for refugee scientists. This goes back, I guess, to a previous generation of scientists who fled from [Adolf] Hitler in the 1930s. Ours was a new wave of refugees. She was a wonderful lady and she tried a lot, but we had always said that we wanted to move on. It was also necessary to ignore our limited language skills. I gave my first lecture in English, I think, in early January 1957, in Cambridge. Because of my fluorine work, I knew about Bob [Robert] Haszeldine, who was also doing fluorine work. He invited me to Cambridge University for a lecture. I lectured in English, but some people felt I was speaking a very strange language with some words, which had some resemblance to English. [laughter] This was really not original because some other Hungarian refugees had previously gone through the same experience. But it was a very interesting visit. I remember—not that it's really important—that Bob Haszeldine was picking me up at the train station in a very fancy car, which turned out to be a Bentley. Little did I fully appreciate the importance of this. [laughter] In late afternoon, another colleague dropped me off at the same station driving a very small, modest car, which tells you something about the different people. [laughter]

TRAYNHAM: You had a taste of both ends.

OLAH: We had an interesting stay, in London—I had gone frequently to the Royal Society of Chemistry Library at Burlington House. As I mentioned, I received much help in resettling, particularly from two people. I have already mentioned Ingold, with whom I had some connection, and Alex [Alexander] Todd, with whom I had no scientific connection whatsoever. I wasn't in his field, but Ingold introduced him to me, and being a very decent human being he also wanted to help me. However, he suggested that we should move to Western Canada. As I mentioned, my mother-in-law remarried in Canada, so our interest to go to Canada was primarily for that reason. When Todd heard about that, he told me that the place to go was Saskatoon, Saskatchewan. [laughter] Now I'll tell you, I had read about Saskatoon, Saskatchewan only once before. Having been a student of Zemplen, I was familiar with one of Zemplen's contributions to carbohydrate chemistry. There was a Swiss chemist called [Ame] Pictet, who published a paper in the early 1930s, in which he described the synthesis of saccharose (10). It's interesting that saccharose hadn't been made in the laboratory before. So this was an important paper. However, subsequently two groups, one in England and the other Zemplen's, showed that this paper was incorrect. I think it's a tragic story because one of Pictet's research students, in order to try to please his professor, put a little table sugar into the last three crystallizations and got a nice yield of the "product." This caused great upheaval, and I remember that some years later Pictet committed suicide. Why I mentioned this is because, in Hungary, I read, around 1954 or 1955, a paper by Ray [Raymond U.] Lemieux describing the *bona fide* synthesis of saccharose (11). I still remember that I was not only impressed by the paper, but it came from this lab, which was called the "Prairie Laboratory" in Saskatoon, Saskatchewan. Now imagine that you are locked in a faraway place in the world, there's no outside contacts, and you have this vision of what a place like this must have been like! [laughter] Todd's relation to Saskatoon was real and he told me that he had a nephew who was a professor in the medical school. He visited his nephew, with his wife, on two occasions, and felt that it was a wonderful place to do research, and the university there could use somebody like me. After the first time he mentioned it, I looked it up on the map and thought that it was, for all practical purposes, in the middle of nowhere. I couldn't see any mountains between it and the North Pole. The next time I saw Todd, I asked what time of the year he had visited it. It turns out that they were there in July [laughter], so probably that's the month when the prairie is livable. The winters are particularly harsh. We still haven't gone to Saskatoon, but I still remember with great gratitude his willingness to help.

TRAYNHAM: Well, when you decided to go to Canada, what was your destination?

OLAH: Well, we were going to my mother-in-law's.

TRAYNHAM: Where was that?

OLAH: In Montreal. Then I started to look around. Todd and Ingold also recommended me to a friend of theirs, Stacey, who was running the National Research Council in Ottawa at that time. I visited him and he offered me a possibility, but at this time, my major concern was to have a permanent job, to make a living, and to keep my family going. The best job offer I had was from The Dow Chemical Company. They had started, at the time, a small—what they called an exploratory research laboratory in Canada, this was a time when most major American companies were establishing European research laboratories: Union Carbide [now owned by Dow Chemical Company] in Brussels, Monsanto [Company] in Zurich, and Cyanamid in Geneva, and so on. Dow established its laboratory in Sarnia, Ontario, a major Canadian chemical center just across from Michigan and not far from their Midland, Michigan base.

TRAYNHAM: So was your work in the Friedel-Crafts-like reactions the lead in to the offer from Dow?

OLAH: Well, probably. Look, I was still a young chemist with some record, and some recommendations. I got a chance. When we left Hungary, most of my small research group came out with us and got scattered. Two of them ended up joining me at Dow. The Sarnia Dow laboratory was a small operation with maybe twenty people overall. It had a fair amount of freedom because it was not run by the Canadian operation, which was more limited, but was run and financed completed by the parent U.S. company.

THACKRAY: What was the rationale for this—they were doing that why?

OLAH: Primarily, for tax reasons. At the time it was difficult for American companies to bring out, without a big tax burden, earnings from Canada. Dow used some of it to do research, instead of going to Europe, and established this little lab. It gave me a chance, and I greatly appreciated it.

THACKRAY: How did you learn of the position? I mean, was it advertised?

OLAH: Oh, yes. I answered an advertisement. I didn't know much of what was really involved.

THACKRAY: What did they hire you to do?

OLAH: I was hired as junior research chemist. [laughter] Seemingly, I did reasonably well because I moved very rapidly up the "ladder." We had this parallel structure at Dow. If you stayed just as a researcher, you could become associate and then a research scientist, which, at least on paper, was equivalent with the research director. Of course, it wasn't in many ways. In my work, however, I did a fair amount of what interested Dow. Dow practiced the Friedel-Crafts chemistry, in which I had much interest, on a large scale. They made styrene monomer for their polystyrene operation on a very large scale. At the time, this was in early 1957, the Dow Chemical Company, of which the Canadian operation was a small part, as I remember, had their annual sales of a hundred million dollars. Now they are probably in excess of thirty billion dollars, so even if you adjust for inflation and so on, it has grown enormously.

Dow was, at the time, making ethylbenzene from benzene and ethylene, I think on a scale close to a million tons. It was, at the time, one of the largest scale chemical intermediates made. As a young chemist coming from a very different background, I, of course, was very impressed and I fully expected that the company knew everything about this reaction. As it turned out this wasn't necessarily the case, so I made some contributions. I learned how the process was done using benzene and significant amounts of anhydrous aluminum chloride in large reactors, while introducing ethylene. Of course, Friedel-Crafts reactions are supposed to be catalytic reactions. When I learned that they were using huge amounts, up to 25 percent, of aluminum chloride in the reactors, amounting to hundreds and hundreds of metric tons, I asked the simple question: "If this is a catalytic reaction, why the need to use the catalyst in such enormous amounts?" I was given the simple answer: "Young man, we are doing this because it's the way it works." You really can't argue with success. On the other hand, as a chemist, I was very intrigued as to why this really was needed. In connection with some other interests I already had in Hungary when I did Friedel-Crafts reactions, like alkylating aromatics with alkyl fluorides and using boron trifluoride as a catalyst, I was interested in finding out about the mechanism. I also tried to see what complexes were formed. My work in Hungary, as mentioned, was under very limited conditions. We had no spectroscopic means—it's difficult to believe, but it's a fact. I observed complex formations between alkyl fluorides and boron trifluoride, and even isolated these complexes at low temperatures, but we couldn't obtain spectra. So we measured conductivities. We observed that methyl fluoride and ethyl fluoride in boron trifluoride gave very limited conductivities, but isopropyl and particularly tertiary butyl fluoride, under similar conditions, gave high conductivities. It was tempting to say that maybe tertiary butyl fluoride was ionized. Of course, you can measure conductivities reasonably well, but you have no proof of what the conducting species are. Obviously, our conditions never could exclude some moisture and acid impurities. At least my interest was aroused.

As mentioned, when I came to Dow, I found more intriguing questions. Why were such large amounts of Lewis acid halide catalyst used in alkylation? I started to look into this. The work also resulted in some practical improvements, which my employer appreciated. If in a large-scale industrial operation, you achieve, say a 2 percent improvement—this was a big deal economically, not necessarily scientifically. I slowly untangled the problem, and the conclusion was really quite simple. Namely, it's well known that Friedel-Crafts systems usually form red oils, which goes back a long while. You carry out the reaction with the red oils present in the system, but nobody really figured out at the time what these red oils were. I was able to show that these red oils were really ionic complexes dissolved in hydrocarbons, in other words, organic ions, namely carbocations. From these aromatics come highly stable benzenium ions or sigma complexes as you call them. With each cation in solution you must have an anion, so you have in this red oil a high concentration of ions with an equivalent amount of anions. It turned out that the large amount of aluminum chloride in the reactors overwhelmingly was bound as the tetrachloraluminate anion, and this anion, of course, has no catalytic ability whatsoever, but this provides an ionic environment with aromatics, which act as a favorable medium for the alkylation reaction. You have this ionic liquid environment in which the Friedel-Crafts reaction takes place, but you need to continuously add a small amount of fresh catalyst to assure the presence of uncomplexed active catalyst. My industrial colleagues know this well because the plants operated by adding fresh catalyst. Now, this make-up catalyst was free aluminum chloride, catalyzing the reaction. The large amount of overall aluminum chloride was mainly to provide the anions for the system. This process was used for a long while—I think up to the late 1970s—and was state of the art in very large-scale chemistry. I spent seven-and-a-half years in industry, and I really enjoyed it. It gave me many opportunities and increasing freedom. Some of the knowledge we gained from some of my academic studies helped to improve some large-scale industrial processes.

THACKRAY: George, you identified what was going on in this particular case. You just said it went on into the late 1970s, so did the company not respond to what you were telling them?

OLAH: My contributions made some very useful improvements. What I am simply saying is basically that the technology to do liquid-phase aluminum-chloride-catalyzed alkylation stayed with the industry for a long while, even long after I left.

THACKRAY: What in fact—having discovered what was happening—did you recommend that they not use the red oils? I mean, what was the operating solution?

OLAH: I don't know how much of this is open knowledge or not. I can only reflect on what we published in the literature. [laughter]

[END OF TAPE, SIDE 2]

THACKRAY: We were hearing, among other things, that you've known each other for a number of years.

MRS. OLAH: Perhaps more than I want to remember. [laughter]

OLAH: She is the one who really deserves a gold medal. She's stuck it out with me.

THACKRAY: What we didn't hear, Mrs. Olah, was something about your own family. We heard you met in 1943, but tell us a little about your own life up to that point.

OLAH: She was a young girl.

MRS. OLAH: I was fourteen at the time—I don't have much to tell up until that point. We met when I was fourteen and he was nearly finishing high school.

THACKRAY: Please tell us about your parents.

MRS. OLAH: My father was in agriculture business.

THACKRAY: In Budapest?

MRS. OLAH: Yes, in Budapest. My mother was a housewife.

THACKRAY: And where was it that you two met?

MRS. OLAH: We were in a resort place. We were there with our parents.

OLAH: It was a real coincidence—you remember this. A real love story.

THACKRAY: So what did you notice about this young man? [laughter]

MRS. OLAH: Actually, not much. [laughter]

TRAYNHAM: Yet he stood out above all the others?

MRS. OLAH: He was tall and very skinny.

OLAH: Those were the good old days.

MRS. OLAH: Six years later we got married. As you may have heard he enrolled me to study chemistry, which I didn't appreciate at that time.

OLAH: Which I'm not so sure that she's forgiven me for!

TRAYNHAM: What were you interested in before he told you that you were going into chemistry?

MRS. OLAH: Well, I had a job. I had fun. I was a laboratory technician. That was enough for me. I really didn't want to start to study again after I had happily finished with gymnasium.

THACKRAY: You went to a single-sex school?

OLAH: Just boys. Oh, yes, very strict.

THACKRAY: Mrs. Olah, you too were at a similar school for girls? Is that right?

MRS. OLAH: I was in a girls' school, yes, but not so strict. Not a religious one.

THACKRAY: Your families, George, did they have any religious beliefs or practices?

MRS. OLAH: You mean the parents, or the children?

THACKRAY: Yes, the parents.

OLAH: Not particularly.

MRS. OLAH: I suppose we lived in a rather secular society.

OLAH: Concerning religion, this is a difficult topic because there are some concepts that are frequently misused. Take for example democracy. Everybody talks about democracy, but most people have different views about what it means. Another one is, is there really a God. I mean, people talk about God, but what they mean about it is enormously different. Frankly, it took me a long while to sort out where I stand. Religion and belief—these are different things. I really don't believe that religious rules, regulations, and so on are necessarily God's will, although, I have gone through eight years of a very strict way of upbringing in a religious school. Most of these rules and regulations, of course, are quite contemporary even as far as human history goes. I do not consider myself to be an atheist; I can't really give an answer one-way or the other. I am most probably an agnostic, which simply means that I don't know. If you accept some higher being, I don't think that all the rules and regulations that different religions attach to it are really so important. In a way, if you look at this in the business world, you may have all these organizations pretty much in the same field, with similar goals, and the differences are not fundamental differences.

TRAYNHAM: You indicated that your parents were not noticeably religious. Did they send you to Catholic school because of the school's reputation as a good place to study?

OLAH: That's right. Definitely. And because it was exclusive.

THACKRAY: When it came to bringing up your own children, what did you do in relation to religion?

MRS. OLAH: I tried to send them to Sunday school, but without much success, because they noticed that we sent them to Sunday school but we didn't go to church.

OLAH: We have two sons. I think that they turned out all right, mostly due to Judy's merit. But, all the same, they must find their way in life.

THACKRAY: Where are they? What do they do?

OLAH: They both live very close to here, which is very fortunate. One is a physician and the other has an MBA. Fortunately, neither of them went into science.

TRAYNHAM: Were they actually living in California at the time you made the move to California?

OLAH: We moved here, in part, because my younger son had just finished high school, and wanted to go to Stanford [University]. He felt it would be more convenient if the whole family came here to California. [laughter]

MRS. OLAH: Easier to do the laundry! [laughter]

THACKRAY: Your oldest son was born in Hungary; your younger son was born where?

MRS. OLAH: In Canada.

THACKRAY: When you got to Canada, you kind of paused, then got the job, George, I think you said earlier about starting over again—I mean, it was not Hungary.

OLAH: No, but you see, we traveled light. When I got this job and moved to Sarnia, Ontario, which is just north of Detroit, the company assured us that they were going to pay our moving expenses from Montreal, where we were staying at the time. So we splurged and checked in on the train with two cardboard boxes containing all of our earthly possessions. Subsequently, I submitted to the Dow Chemical Company my moving expenses as a receipt for checking in my two cardboard boxes! You acquire a lot of things in life, but at the time, we moved very easily.

THACKRAY: You worked for a very small unit, you said.

OLAH: Yes.

THACKRAY: Did you choose to work on the Friedel-Crafts out of your interests rather than Dow saying that this was what you should do?

OLAH: Basically, there was a fair degree of freedom but I think that there was also an implicit understanding that some of the work should be in areas where the company had at least some interest.

THACKRAY: When and how did you first meet Ted [Herbert D.] Doan?

OLAH: During my visits to Midland. As a matter of fact, in my research, in the late 1950s, for the first time we discovered persistent carbocations. We had solutions where we believed they were present, but we had no way to prove it ourselves. We had a local research director who was difficult to convince that something like NMR [Nuclear Magnetic Resonance] was important. In Midland, I discovered a wonderful researcher who later became a friend. His name was Ned Baker, a physicist and one of the pioneers of NMR techniques. He had a very advanced NMR lab, and became interested in my problems and generously agreed to help us. So early one morning, we packed our samples into dry ice and drove to Midland to spend the day running spectra. Although I never lived in Midland, I spent a lot of time there. Occasionally, there were research presentations to higher brass and so on. You met people, including the president, who was Ted Doan, in the cafeteria. This was America. The president was having lunch in the same cafeteria where all his employees sat. It was not like a European company, where the president of the corporation has a corporate dining room.

MRS. OLAH: Or USC [University of Southern California]! [laughter]

OLAH: But there were also—I am sure he would not remember this—other things. I received the 1964 ACS prize in petroleum chemistry. The same year, I finished a book on Friedel-Crafts chemistry, on which I worked very hard entirely on my own time (12). At home, at night, weekends, and so on with my wife's help. You remember, Judy, we both did it.

MRS. OLAH: In the basement, on an eighty-dollar Olivetti.

OLAH: Yes, we invested eighty dollars to buy an Olivetti typewriter.

MRS. OLAH: A small one.

OLAH: It was an enormous effort; it was a four-volume monograph.

MRS. OLAH: There was also a lot of money invested into our typewriter, too. [laughter]

OLAH: No, I mean the effort put into this book. The first volume came out about this time, and I had a lab director, a Canadian gentleman, who had the idea that as an employee of the company I was not entitled to any side income. He felt that the ACS prize money and whatever royalties my book would bring, obviously, should go to the company. Eventually, the question ended up with the president who felt that this was really something that made no sense, so I kept my ACS prize money, which, as you know, was not exactly a great fortune, and also whatever income there was from my book. I don't think it amounted to much and in all sincerity, I don't think it would have materially affected the bottom line of Dow Chemical Company. [laughter]

TRAYNHAM: No. Well, how did your research director, who may not have always favored your independent research, feel about your visits to Midland to spend the day doing NMR experiments?

OLAH: That was approved. We really cooperated very closely with Midland. Eventually, I moved to, first Framingham, and then Wayland, Massachusetts, where Fred McLarrerty established for Dow what was called at the time the Eastern Research Lab.

THACKRAY: Which was exploratory research?

OLAH: Yes.

THACKRAY: Did you do other things that had an equivalent impact on the early Friedel-Crafts work from a Dow economic point of view?

OLAH: That's difficult to say. I helped to establish a visitors' seminar program. *Interv alia* George Wittig came to visit. I took him to Midland also. It was a remarkable visit because Midland was still "dry" at the time, going back to Herbert [H.] Dow, who was a teetotaler. The company town remained dry into the late 1960s. The only decent place to stay in Midland was the country club. They had an annex where you could stay. So we went there and had dinner. It was a hot summer day, I remember. At the next table, people were drinking beer. Wittig said he would like to have a beer, however, I was not a member of the country club, and if you were not a member you could not get a drink. Try explaining this to a European. [laughter] Eventually, the people sitting next to us ordered a beer for him. When I was in Framingham, Ingold came to visit. I read a book—a former student [Kenneth T. Leffer] of his wrote a book about Ingold's life, maybe two years ago (13). He told me that Ingold came from Boston where he was staying, took a taxicab, and gave the driver the address. When they came close, the driver asked him, "Are you really sure this is it?" He said, "I'm very sure. This is a world-renowned laboratory of this great company." The driver told him, however, that it was a very crummy neighborhood! [laughter] He was not sure that he wanted to be around here. What happened was that Dow bought an old plant and converted it, temporarily, to a laboratory. Fred McLarrerty, however, had a vision. Dow bought land in a neighboring community and built a very nice laboratory. I also never knew until I read this book that when Ingold retired he recommended me as one of his possible successors at University College London. I found this very surprising, since I was still a little known industrial researcher. He never mentioned anything to me.

THACKRAY: So you knew nothing?

MRS. OLAH: George never considered, however, himself to be what he just said he was.

THACKRAY: The other people in the lab must have been surprised that Ingold came to visit?

OLAH: It was very stimulating. We had some good people there and the lab did good pioneering research, but even at Dow there was no general enthusiasm for the lab.

MRS. OLAH: Which lab?

OLAH: The Dow lab.

MRS. OLAH: In Wayland?

OLAH: Wayland, yes. They closed it down some years ago and sold the facility.

MRS. OLAH: I don't think that the lab was very welcome there in the first place. Wayland is a residential area.

OLAH: Coming back to Wittig, during his visit he had a meeting with the top management at Dow, and they asked for his impressions about research, and Wittig told them some of his impressions and how things could be improved. Supposedly, the research director told him, "Look, Professor, this may all be true, but we are making this year—I don't know what—good profit. Last year it was so much, and next year we'll have even more. So we may be poorly organized, but clearly we're very profitable." These were the golden years, of course. Chemical companies were making a lot of money. Many things have since changed.

THACKRAY: Was Dow a good company to work for?

OLAH: I'll tell you my impressions of Dow. They treated me very well, but occasionally I had these discussions with my research director, and then with his boss in Midland, who was a very decent guy. Sometimes I was frustrated because I thought that we had some interesting results, which could make money for the company. I had this old-fashioned view that if they pay you a good salary, they should make something out of your efforts. I was told by the central research director of Dow on a number of occasions that, "Look, we appreciate that you are doing good chemistry, you publish, and this is okay." He also told me, "You know, I am some years from retirement and everything is going very well. Now you come in here and tell me all these new results. This causes me a problem, because even if all of this works out as you think—and maybe it will, maybe it won't—who knows whether it will be a commercial success. So why should we take the chance? We are doing exceedingly well." And probably each time after such discussions, I got an increase in salary, because he thought that if a researcher came in to see him, his main concern had to be his salary. [laughter]

TRAYNHAM: So you learned the secret! [laughter]

OLAH: Not really, but I think that besides my ambition to eventually go back into academic life, I also felt somewhat frustrated because it isn't very rewarding for a company to spend money on research if it's not prepared to try to get an economic benefit out of it. On the other hand, I realize now that what Dow spent on its small pioneering research wasn't really a very significant amount in the overall picture of the company.

THACKRAY: Beyond that first ethylbenzene area, what were the areas that you did work in, in relation to the company's production?

OLAH: We did, for example, much research on reactions, some of which were quite interesting and could be utilized. I also laid, while at Dow, the foundations for the new superacid density. I was not, however, the only one who did interesting work. Fred McLarrerty, who was the lab director and also a friend of mine, discovered and developed a tabletop mass spectrometer, which he managed to hook up to a GC [gas chromatography]. Fred was the first to have a GCMS [gas chromatography mass spectrometer]. He fought hard with Dow management concerning whether or not they should go into this business, but the idea was turned down. This was in part, I guess, his reason to go back to academic life. But you know, there was one interesting thing. Whereas Dow was not interested, Ted Doan, who at the time was interested in high-tech investments, put some money into this idea, and Finnigan [Thermo Finnigan] was born. Doan was an investor in Finnigan because Dow wasn't interested. Finnigan became quite successful.

TRAYNHAM: Just to discuss that piece more slowly—I mean, I'm aware of Finnigan, but how did that work?

OLAH: Well, I don't know all the details, but I do know that Ted Doan invested, as an individual. It had nothing to do with Dow.

THACKRAY: And Finnigan used some of the ideas that came out of McLarrerty's work?

OLAH: Oh, yes. They marketed the first commercial GCMS.

THACKRAY: Yes, using, in part, some of the Dow developments. Who was it at Dow that you said was doing this?

OLAH: Fred McLarrerty. Fred was a major player in mass spectrometry. But, you see, large companies are not very flexible. First, they do market research, but if something is new, there's no market for it yet.

TRAYNHAM: No one knows they need it.

THACKRAY: Go back to leaving Hungary. You mentioned Attila Pavlath's name. Were there other Hungarian chemists who came west at that time?

OLAH: A lot.

THACKRAY: Who? Can you name some?

MRS. OLAH: Gabor [A.] Somorjai.

OLAH: He was much younger and got educated in the U.S. Miklos Bodanszky, for example, was a very well-known polypeptide chemist and a friend of mine. There were many others as well.

THACKRAY: Have you stayed in touch with any of these people?

OLAH: Some of them, but, you know, even though Hungary is a small country, there are some twelve to fourteen million Hungarians. However, I knew many of the chemists in my generation.

MRS. OLAH: We know most of them.

OLAH: We know most of our generation.

THACKRAY: And Attila Pavlath was in your lab in Budapest?

OLAH: Yes.

THACKRAY: Was he your student?

OLAH: He started with me, yes. When he came to the U.S., he became very successful both in industry and in his work for the federal government.

TRAYNHAM: It's interesting that you both ended up in California.

MRS. OLAH: He was smarter. He came directly. [laughter] We hesitated. I always said that it was too far. [laughter]

OLAH: From what? [laughter]

MRS. OLAH: From life as we knew it in Europe, but now this doesn't matter much. We can fly directly.

OLAH: There were some well-known Hungarian chemists. George [de] Hevesy was one. You know, it's funny, he went to the same gymnasium school that I did, but nobody ever mentioned his name. Never. Even after he got the Nobel Prize.

THACKRAY: Because the school was focused on humanities?

OLAH: I don't know why, but I can't remember anybody ever mentioning his name. Maybe chemistry was not so popular there.

THACKRAY: Did you ever think, in those early days, of quitting chemistry for something more?

OLAH: No. For me, it's not just a profession. It was and is my life. I never thought about it for a minute. Judy is more sensible than I am. I was a very unpractical man for a long while. I never thought much about the practicality of life or anything else.

THACKRAY: Mrs. Olah, you were talking a little bit about life in Hungary when your first son was born. Surviving—can you talk about those years?

MRS. OLAH: It was very hard because I hadn't finish university yet. I was in my fourth year of chemistry. In Hungary, you were able to get help, which was harder—not in California—in the rest of the North American continent. Then came 1956 and everybody around us was leaving. We decided to leave too. It was not easy, but we did it and we have never looked back or regretted it.

THACKRAY: Have you been back to Hungary in recent times?

MRS. OLAH: Recently, yes.

THACKRAY: When did you first go back?

MRS. OLAH: In 1984. It was twenty-seven years before we went back.

TRAYNHAM: That was before the fall of the walls.

OLAH: It's a funny thing. When we decided to leave, we were traitors and so on, and would have been shot if they caught us. Then after a while, if you were reasonably successful, they called you—

MRS. OLAH: Well, they started to call us expatriates in the 1980s, so at that time it was safe to go back. But, you know, after 1956, we all had this dream, a really bad dream, that if you went back, you would be unable to get out again.

OLAH: Now, I am some type of a celebrity in Hungary.

TRAYNHAM: They're glad to claim you now. [laughter]

OLAH: They still try to put emphasis on education in Hungary, although, I'm not really familiar with the details.

TRAYNHAM: Is the gymnasium you attended still in operation?

MRS. OLAH: Yes.

OLAH: In a different location. It had a very nice building close to the Danube that was confiscated and made into some type of university operation. Now it has another location and is back in operation. Of course, you know, the world has changed. The Catholic schools did very well because they had priests and nuns who taught for nothing, and were really quite dedicated. These days, of course, there are fewer young priests and nuns. Therefore, they need to hire teachers, and this is an economic burden. Traditionally, for a long while, Hungary was a feudal state and the Catholic orders had big land holdings that supported them, like the bishops big land holdings.

THACKRAY: Which were all confiscated?

OLAH: Yes. Not just confiscated, but they were parceled out to small peasants.

TRAYNHAM: We're jumping forward here, but as communism collapsed, were you surprised at that?

OLAH: Very much. When we left Hungary, we never thought that in our lifetime it would ever happen. When it happened, it happened fast, and peacefully. Obviously, there were signs.

TRAYNHAM: Hungary became a major route for the break.

MRS. OLAH: Actually, it was the Hungarian foreign minister who opened the border to the East Germans.

OLAH: I'm not so sure how grateful the Germans were! [laughter] The West Germans were really well off.

TRAYNHAM: I traveled in East Germany before and after the fall of the wall, and there was a very impressive contrast.

OLAH: Oh, enormous. It took a while, you see, for that, because the middle-aged, older generation in East Germany had difficulty adjusting, but the young people adjusted fine.

TRAYNHAM: Not all of the impact for the middle-aged and older people was favorable.

OLAH: Age is, however, not always an obstacle. You know that [Karl] Ziegler made a fair amount of money for his institute in Mülheim [Max Planck Institut für Kohlenforschung (formerly known as the Kaiser-Wilhelm-Institut fur Kohlenforschung)]. In his more advanced age, I understand, however, that [Günther] Wilke, who succeeded him, made even more, and not through his outstanding organo-metallic chemistry, but through his caffeine extraction process by CO_2.

TRAYNHAM: Yes. Ziegler spent a semester at LSU [Louisiana State University] in the 1960s, I suppose it was.

OLAH: He was an interesting man. He was very interested in astronomy and the space program. He was an amateur sky watcher. He had a telescope on the top of his institute.

THACKRAY: I don't know if anyone has ever written a book about it, but the immigrant contribution to science in the United States could really make a book in itself.

OLAH: Well, there were some books—[Enrico] Fermi's wife [Laura Fermi] published a book about it (14).

THACKRAY: Yes. About that particular—

OLAH: No, about immigrants she knew. Yes, about that particular period, and people that she knew personally.

TRAYNHAM: I have an idea that I've articulated several times that—it's a matter related to this subject and it is related to the fact that American attitudes are frequently different from those that one finds in Europe—the immigrants in particular are people who had the nerve, the courage to get-up-and-go.

OLAH: They had no choice. [laughter]

MRS. OLAH: Some of them.

TRAYNHAM: But there was a choice. A lot stayed home. The ones that moved were the ones that had the get-up-and-go, and they brought that attitude with them.

OLAH: You have natural selection.

TRAYNHAM: Yes. And the same thing is true with foreign national students in United States' universities. They are persons who have the get-up-and-go spirit, they bring that with them, and it shows up in their relative achievements compared to the general population in the universities. I think that that's true with the contribution in science, music, or other areas. The immigrants have a very positive contribution to make to the country that is almost the antithesis of the political resistance to welcoming immigrants. Yes. You're a good example of the situation I mentioned.

OLAH: I don't know, except that the native Indians and Eskimos—and the newcomers took care of them—everybody came at one time or another. One other thing that comes to my mind this morning about Hungarian scientists, or Hungarian-born scientists, goes back to the Manhattan Project in which a fair number of Hungarians were involved. It was said that in Chicago, on one occasion, Fermi, who led a discussion, was called away. At which time somebody said that as long as he was not here, they could continue to talk in Hungarian. Concerning Hungarians, there was also a claim made in jest that they really arrived as visitors from Mars. Martians came to this planet and they indeed had high intelligence, and knowledge in science, except that they had a problem: they spoke English with a terrible foreign accent! [laughter] Therefore, to disguise their identity, they were looking for some people who had an even worse accent, and eventually they found the Hungarians. [laughter] A Hungarian physicist, George Marx, wrote a book about scientists of Hungarian origins. It's called *The Voice of the Martians* (15).

THACKRAY: When was this published?

OLAH: Not too long ago. You know, Leo Szilard had a delightful little volume published in the 1950s, *The Voice of the Dolphins* (16). The title may have mimicked his. In this little collection of essays, Szilard had one, which is my favorite. The story is not so important. A scientist wants to preserve himself for eternity, so he has himself frozen with strict instructions that he should be thawed up after so many hundreds of years, but something happens, and the involved lawyers got into a struggle. He's prematurely brought back, but anyhow, he's a celebrity in his city. On one occasion, the mayor invites him to a dinner, and he finds himself seated next to a millionaire. The millionaire asks for his advice saying, "I have made a lot of money, what shall I do with it?" The scientist says, "You should start a foundation to support science." The millionaire, however, answers, "I must confess, I hate science." So the scientist sat back thinking, and after a few minutes he came up with a solution, "Look, I've thought about it and I've solved your problem. You still should start a foundation. Put your money there. Start your foundation to support science, but you must put in some conditions: The foundation should engage on its board and its numerous committees the most active and productive scientists. To assure their participation, they should be paid outlandish salaries so that nobody will refuse to accept. Soon they all will sit in meetings all the time instead of pursuing science, which will start to wither. Thus you will achieve your goal, while seemingly being supportive of science." There is much to be learned even today from this story.

[END OF TAPE, SIDE 3]

THACKRAY: Talking about the success of Hungarian science, how do you explain it?

OLAH: You know, Hungary is a little country, but it is proud of its achievements.

THACKRAY: It's a wonderful historical, sociological problem—how do you explain this?

OLAH: I really think that it's education. I don't necessarily believe that there is born scientific talent, like there's musical talent. I would even say that in mathematics you must have a specific talent, but in chemistry, I think that if you take reasonably bright kids and you convince them that chemistry is an interesting field, they can do well. I don't think that there are chemical geniuses, but I may be wrong.

THACKRAY: Though interestingly—George, I'm just thinking of the sort of Hungary and England in the era in which you were born—there would be a lot of barriers against—disincentives—studying politics. In England, there's a very rigorous academic system, but it puts a lot of the talent into political civil service sort of activities.

OLAH: I must apologize for disagreeing with your British example, but you helped me remember something when you said "studying politics." I once read how [Benjamin] Disraeli got involved in politics. Do you know the story?

THACKRAY: No, I don't.

OLAH: Disraeli as a young man was always in great debt and trouble. At the time, there was still a debtor's jail, and he managed to keep out for a while, but on one occasion he was really at the end of his wits. With no family or friends to help him, he got advice, in desperation, from a friend. He said, "You go out, borrow, steal, or do whatever, and get some money, so you can buy yourself a safe seat in Parliament. The day that you are elected an MP [member of parliament], you have immunity and they can't put you in jail." Disraeli followed the advice and became a politician. [laughter]

TRAYNHAM: And a very successful one.

OLAH: Ever since I read that, I doubt that you must be educated as a politician and have a vocation to do great things. [laughter] You may need charisma or charm. Disraeli's initial interest was just to keep out of jail, but he built an empire, right?

THACKRAY: Yes.

TRAYNHAM: While you were still in industrial employment with Dow, you presented a paper at the 1962 conference on mechanisms of organic chemistry [Reaction Mechanism Conference], which had an impact (17). Could you tell us something about that—the beginning of your impact on the area of carbocation chemistry.

OLAH: Well, I briefly mentioned that I was involved in one way or another studying acid catalyzed reactions, I was also interested in their intermediates. Even back in Hungary, I tried, maybe in very primitive ways, to look for these long-elusive complexes or ionic species. When I restarted my work with Dow under much improved conditions, I got back into this. I did a fairly systematic study. It wasn't a stroke of great insight or revelation. I very systematically studied practically all-available Lewis acid halides and their conjugate acids as possible ways to obtain stable complexes or ion salts. Eventually I found in antimonic pentafluoride, which is a very strong Lewis acid, a suitable system, which did the trick.

TRAYNHAM: There was no particular theoretical thought that led you to that choice?

OLAH: No. I'm sorry, but there was no theoretical insight. I assumed that these ions for all practical reason should be stable. They are stable in the gas phase, in the mass spectrometer and so on. When we talked about stability, it became increasingly clear to me that what we were talking about—if we wanted to observe species with long lifetimes, and observe them with different methods, or even do chemistry, we had to differentiate thermodynamic from kinetic stability, because thermodynamically very reasonable and stable species may not be kinetically stable. When you look for a carbocation—an electron-deficient carbon species, i.e., an acid, as an observable species—you can't look for it in an environment that is basic or nucleophilic, i.e., electron-donor in its nature. Acids and bases don't coexist in the condensed state. It was a long struggle to find a solution. I did this empirically to try to look for different Lewis acids with increasing strength, and eventually antimonic pentafluoride provided this. You know, as Meerwein did most of his pioneering work with boron trifluoride, I was fortunate to have come across antimonic pentafluoride, which nobody had looked at before, and which turned out to be a key component for my studies. Later on, of course, I realized that not just antimonic pentafluoride, as a Lewis acid, but also its conjugate acids are effective. These Brønsted acids have very high acidities, and we started a program to explore them. The name superacid, which was given to some of these systems, did not really originate with me, Ron [Ronald J.] Gillespie, or any other of our contemporaries, who have done subsequent work on the strong acid. It is due to J. B. [James Bryant] Conant, one of the great American organic chemists from Harvard [University], who did significant work before he became the president of Harvard, and subsequently a significant public figure. He was the first Allied High Commissioner of occupied Germany and did much to help establish a democratic Germany. When he returned to the States, he was very much involved in the problems of public education. Conant, in 1927, which was a good year as it is when I was born, published in *JACS* [*Journal of the American Chemical Society*] two papers, which had in their titles the names of superacids, not just in the text (18). Conant reported that he had found that some carbonyl compounds get protonated in the strong perchloric acid that he used. He called attention, therefore, to the fact that there may be acids that are much stronger than our conventionally used acids. He suggested the name "superacids." His work was fine, but I really wouldn't recommend work with anhydrous perchloric acid too much because many organic compounds with anhydrous perchloric acid can give very unstable explosive systems. As Conant moved on to more important things, the topic was not followed up.

When we started to work in this field, we found a variety of these very strong acids—mixtures of Brønsted and Lewis acids. In these systems, carbocations, like the tertiary-butyl cation, turned out to be remarkably persistent or stable. At this time, I already had a fairly large amount of quite solid data. We not only made stable ions, but we also studied them quite extensively. In contrast to Hungary, I was able to use all the tools that modern chemistry had, including nuclear magnetic resonance spectroscopy. As I mentioned briefly, in my initial work with Dow in Sarnia, Ontario, Canada, we took our samples in the morning to Midland, Michigan, to run our NMR spectra. This was a pioneering time. Running proton spectra had become fairly routine, but we also needed to run carbon spectra. In the late 1950s, to run carbon NMR spectra was a major achievement. On top of that, we had these ionic compounds. Ionic compounds are not very soluble in non-polar solvents, and we couldn't use polar solvents because our carbocations would immediately react with polar nucleophiles. So we had this problem to prove that our ions were really there in the solutions, albeit in a relatively low concentration. At this time, the highest carbon labeling available was about 50 percent, so when we started with 50 percent labeled tertiary butyl chloride, and made a solution, which hopefully contained the tert-butyl cation, but if the ion had only 2 or 3 percent solubility, we diluted down the concentration of the labeled compound very substantially. There was no way using the known methods to look at it. Ned Baker, however, devised a method, which became known as INDOTR, Inter-Nuclear Double Resonance, which by "sitting" on the sensitive proton signal, allowed one to look directly via double resonance at the carbon signal. We obtained this data, infrared spectra, and others.

I was invited to the ninth Brookhaven Reaction Mechanism Conference in 1962. This was a remarkable experience for me. Here I was a still quite young industrial chemist from an unknown laboratory, invited to give a conference lecture together with such major lecturers as Saul Winstein and Herbert C. Brown. The event turned out to be centered on what had come to be known as the non-classical ion controversy. Brown and Winstein were discussing indirect solulytic data on carbocations, but here was this young guy, who claimed that he had discovered a method in which it was very easy to prepare practically any carbocation, look at them, determine their structure, and explore their chemistry. It still lives vividly in my memory. My friend, Paul Schleyer, recalled that I was pulled aside separately by both Herb Brown and Saul Winstein and both gave, in their own way, friendly advice, which basically said, "Young man, a lot of people have claimed and tried before. Claiming that they could prepare and observe organic ions, carbonium ions, but it always turned out that they were wrong. In all probability, you are wrong, too, so you should be very careful. However, just in case you really have something, you surely should look at this most-debated ion, the norbornyl ion, because you will find that its structure—now, depending who you talk to—is either classical or non-classical." So that's where it stood. There was a lot of skepticism, but look, you must have confidence in what you are doing. If you are really convinced that your experimental facts are right, and we were very careful—we did a lot of work to be sure—then I think you should stand up for your work and are entitled to your own conclusions.

TRAYNHAM: Did you have a personal persuasion about the classical or non-classical character of the norbornyl cation?

OLAH: None whatsoever. Look, I must say one thing. I was convinced and I'm still convinced that we, as scientists, are here to observe what's in nature. We can't influence it. I would have been equally happy whatever Mother Nature would have decided concerning the seven-carbon atom containing a simple bicyclic ion having one type of structure or the other. It wouldn't have made much difference to me at all. I was only interested in the problem. When we looked at the ion, observed it, and interpreted our data, it turned out that it was the delocalized or bridged non-classical structure. The structure wasn't created by Brown or Winstein; it was created by Mother Nature. These norbornyl systems are naturally abundant in terpenes. They are the major component of a significant class of natural products, and their rearrangement was discovered by [G.] Wagner a long while ago. It was Meerwein, who, in 1922, while studying this rearrangement, observed that if he started with a corresponding chloride, Lewis-acid halides greatly accelerated the rearrangement process. Meerwein made the daring suggestion that starting with a covalent compound like isobornyl chloride can rearrange another covalent compound going through an ionic intermediate, a positive ionic intermediate. Now this was a very daring suggestion, which is where it started, and I don't know whether we should go into this any further. I can say a few personal things about this, going back to Meerwein, which I learned from Meerwein many years later.

TRAYNHAM: Please do.

OLAH: Meerwein experienced a lot of problems at the time of this suggestion. His peers, the leading German chemists, were rather conservative. In Germany, mechanistic chemistry was practically non-existent until after World War II, so his peers were very critical of him. They felt that he did not have enough evidence for his ionic intermediate concept. His follow-up papers were turned down by all the major German chemical journals. Meerwein was a very proud individual and he told me that this affected him, but he decided that instead of brooding or making a case out of it to prove his abilities in other fields, he would never again published anything in this field, which was a great loss to chemistry, but maybe this was eventually a gain to some others, like myself.

Anyhow, coming back to the Brookhaven conference and the follow-up. I established what we had achieved and there was a lot of follow-up and eventually hundreds of laboratories got involved around the world, and my stable ion conditions became a well-established technique. One area I got involved in the follow-up was the so-called non-classical ion controversy. At Brookhaven, I showed the NMR of the ion, but at the time we only had a spectrum of the ion at room temperature. At room temperature all the well-known equilibration processes, hydride shifts, and Wagner-Meerwein rearrangements are all going on at full speed, so you only see one singlet signal. Later on, we improved our efforts and got down to lower and lower temperatures, and eventually we were able, at low temperatures, to slow down these processes. By 1969, at an organic symposium meeting in Salt Lake City, I presented a fairly final picture of the ion under conditions where all these shifts were frozen out at -157° C. Shortly after the conference, Saul Winstein passed away in November of 1969. This was not only a great loss of a very fine chemist, but it also caused problems for me. In this still on-going heated controversy, I was left facing the very dynamic Herbert C. Brown, for whom I have high regard. Herb, in this particular case, I think, was very much influenced by the fact that he was not only convinced about his point of view, but also was convinced that he can't be wrong. Now, I'm not in any way criticizing anybody, but I really do feel that as scientists, this is something we should always avoid. We shouldn't really lock ourselves into any one position, because then we start to believe that our position is the only position. Look, I learned some things in my life. Teaching mechanistic-structural chemistry, I always say, there is no such thing as an established reaction mechanism. A reaction mechanism is our best explanation of the facts as we know them at the time. Obviously, there is a continuous progress in science, if new facts are coming along, you must accommodate them. Now, if your concept of a mechanism can accommodate the new facts, it's fine, but if the new facts are in contradiction to the previously accepted best explanation, you have no other choice, but to accept it. This has nothing to do with personalities. I think it's very sad—who am I to criticize? I am probably not a very easy person, but—when it comes to science, I really think that as scientists we have an obligation that we must keep our views based on the facts, and in this case, unfortunately, emotions and so on were running away and running too high. There's nothing wrong with believing in your view, but only up to a reasonable point.

I mean, when you look back, we are talking about history. I'm a great believer in history. The history of science plays an essential part, because you can't talk about science without relating it to its historical marker, to its historical period. For example, not so many centuries ago, the best scientific minds firmly believed that the earth was the center of the universe and that it was flat, too. They warned you shouldn't go too close to the edges, because you may fall off. Now, these were not mad men. They were people who, at the time, with the knowledge that was available, made this conclusion. When new facts became available—obviously today, we are not accepting the flat-earth-concept anymore, because our astronauts took pictures of a round planet Earth.

It's the same in a small way with the non-classical ion controversy. When it was possible to prepare the norbornyl cation, and it's a relatively simple molecule—just a seven-carbon atom containing compound—then it was possible to snap pictures with all kind of spectroscopy, crystallography, and so on. Thus, it was not a tremendous achievement to subsequently establish its structure. To me, this whole controversy just meant one thing. Whenever I am able to come across something in chemistry that I am interested in and am able to develop some knowledge of, I can't resist asking, "So what's the meaning of this? How is this good for chemistry in a broader sense? Is there any use for it?" Whether this particular ion is of one kind of structure or another, *per se,* doesn't make much difference to—not only to society, but also certainly not to chemistry. It may affect some egos, but these are unimportant. To me, the significance came when I started to think about what broader meanings could be derived from it. To put it very briefly, what I was able to achieve was to take a precursor, ionize it to the ion, then stop there, and look at the ion directly by spectroscopy even when isolated chemistry X-ray crystallography existed. Before all the studies on organic ions going back to Ingold and others, whole generations of fine chemists were all deducing indirectly the nature of intermediates from kinetic studies or stereochemistry. Ingold showed that you could have first-order kinetics when you solubilize a tertiary precursor through a carbocation. It was also shown that the ion must be flat, because the stereochemistry starting from an optically active precursor generally leads to racemization. Now, my chemistry was able to stop at the ion. We were making ions and looking at the ions. Then I started to ask myself, "So what's so unique about this bicyclo[2.2.1]heptyl system? Why is the bridged ion formed with such preference?" I realized that what's really happening is that you have a carbon-carbon bond between the C_1 and C_6- carbons in the skeleton, which happens to be located in reasonable bonding distance with the right orientation to interact with the developing pi-orbital of the C_2- carbon. In a way, this looked to me like a Friedel-Crafts intramolecular alkylation. It's well known in Friedel-Crafts chemistry, that if you have an aromatic ring with a side chain attached to it and somewhere in the side chain you start to create an electron deficient center, then the aromatic ring can react with it and you get an intramolecular alkylation. In the norbornyl system, there is no pi-system or non-bonded electron-donor system, but there is a suitable carbon-carbon single bond. I was able to develop this concept of the general electron-donor ability, which extended from pi- and non-bonded electron-donors to sigma donors. So it was a sigma bond, a particularly well-oriented carbon-carbon single bond, which got involved in interacting with the carbocationic center. This gave me the idea that if this works within a molecule, maybe I could do similar chemistry with saturated hydrocarbons and external electrophiles.

So this started me on studying—and it was quite successful—the chemical reactivity of single bonds with strong electrophiles. This greatly extended the scope of what we call electrophilic organic chemistry and allowed the introduction of much new chemistry. To me, therefore, the norbornyl studies, which many consider futile because many chemists have wasted their time on this stupid thing, proved that you do science because you are interested. You don't necessarily always foresee where it can lead you. I'll gladly admit that in my entire career, I was mostly influenced by my curiosity. I was curious and tried to follow up things, but I was also lucky because I had the interest that gave me the ability to keep my eyes open. When you keep your eyes open, sometimes you observe things. As my late fellow countryman, Albert Szent-Györgyi, used to say, "Discovery really means to see something that many others may have seen before, but to realize what it really means." This is a good analogy. I mean, these non-classical ions were considered and looked upon by many other chemists. They were all fascinated to prove or disprove some structural problem, but to me it became a model to try to understand what I consider—and I mentioned at the end of my 1972 paper on the general concept of carbocations, which turned out to be true, that I was convinced that one day the electrophilic reactivity of the single bonds would rank on equal footing with G. N. Lewis' realization of the reactivity of the multiple bonds on non-bonded electron pairs (19). Much chemistry is still pursued based on this realization.

Another strong belief that I have is that I don't believe in sub-dividing chemistry. I really believe in the universality of chemistry. To me, there is no separate organic, inorganic, or whatever chemistry. I happen to be a chemist who worked mainly with substrates, which had carbon and hydrogen in them, but at the same time, I also worked with many reagents and systems that had all kinds of other atoms in them. As far as carbon chemistry or organic chemistry is concerned, one of its guiding principles is based on the German chemist [Friedrich August] Kekule's realization in the middle of the 19th century that carbon is only able to bind to four atoms or groups simultaneously. Of course it was realized that carbon could also be involved in multiple bonding with the same atom. Kekule's concept of the four-valency of carbon is <u>still</u> a guiding cornerstone of organic chemistry. In my work with carbocations, it became, however, quite clear that in some situations, carbon can combine simultaneously to five neighbors, as is the case of the bridging carbons in the norbornyl ion. I did, subsequently, much work probing protolylic reactions of alkanes individually protonating methane. $CH_5 + CH_5$ today is no longer a fictional, assumed species. It's a *bona fide* carbocation and a significant part of chemistry. We have even succeeded in showing that you can doubly protonate methane to CH_6^{2+}. Two years ago we showed that even CH_7^{3+} is a feasible ion. We failed, however, to go to CH_8^{1+}. Charge-charge repulsion seemingly is catching up with us. I feel that if anybody looks back at my chemistry at some time in the future, they will probably see that my ability to show the significance of higher-coordinate carbon chemistry, which we call hypercarbon chemistry, as well as showing the ability of single bonds to react with electrophiles involving two-electron-three-center bonding was my most important contribution.

TRAYNHAM: I can recall from the 1960s era that persons, who did not favor the interpretation of the bridged carbocations that your data was continually supporting, argued that the conditions under which you were able to make these observations were so remote from the solutions in which most organic reactions were run that the data and the interpretation were irrelevant to the solution chemistry that was normally taking place. What was and is your reaction both in the 1960s to such a comment and as you take a backward look now, forty years later?

OLAH: To me the question was and is very clear. In chemical reactions, you go from starting material to products. The pathway is what we try to figure out. There are peaks and valleys on these pathways. The valleys we call intermediates. The peaks we call the transition state. Now, if you want to see a species in the valley, you must find conditions that allow you to stay in the valley long enough, so that you can observe it. The fact that we were able to show that these carbocation intermediates are *bona fide* species solves the problem of the nature of the intermediate. Now how far are you really from these intermediates during a chemical reaction in an entirely different medium is another question. In many reactions you can proceed through a reaction path, where you don't reach the carbocationic intermediate before you start to interact with the reagent on the medium. I go back again to Ingold. There are limiting cases or in-between situations. We know that there exists a continuum. Therefore, I don't think that what I have done and what previously was suggested is necessarily in contradiction. We now know how carbocation intermediates look, what their structures are. On the other hand, in many reactions, you may proceed through a pathway, which isn't reacting completely with the intermediate, and that's the nature of chemistry. On the other hand, one chemical species can only have one structure. In whatever unusual medium you look at, in the gas phase or in solution, one specific chemical species can have only one structure. Now, if you look at the same species, say a highly electron-deficient species in a nucleophilic environment, then the nucleophilic environment will start to impact it. It's the acid-base interaction problem. You can't have an acid as a free acid if you try to look for it in a relatively basic environment. I feel that we gained a substantial way of understanding. But you see, some still attempt to say that there is a different structure for a carbocation depending on what medium you look at it in. This clearly is not in accord with what we know about chemistry. It simply means that electrophilic species are solvated when they interact with nucleophiles. How strongly they are solvated and when does solvation result in forming new bonds, that's an entirely different question.

TRAYNHAM: Shortly after that Brookhaven conference, I believe, you moved from Canada to a Dow installation in New England. Can you tell us something about the circumstances for that move?

OLAH: Yes, Fred McLarrerty, started this lab and invited me to join him. Obviously, it was an attractive location, just down the road from Harvard and MIT [Massachusetts Institute of Technology]. While I stayed there I attended the weekly Bartlett-Westheimer seminars at Harvard. It was a very stimulating atmosphere to be in.

TRAYNHAM: Dow had no resistance to your move within the company?

OLAH: They suggested it.

TRAYNHAM: I see. You didn't stay at that new location very long before you returned to academic life?

OLAH: That's true. Basically, I have always been someone whose heart was in academic life. Fred McLarrerty decided in early 1965 to move back into academic life. He went to Purdue

[University] first, and subsequently to Cornell [University]. After he left, I felt it was time to move on. Paul Bartlett from Harvard, who was consulting in the Cleveland area and was familiar with the place, told me that Western Reserve University [currently known as Case Western Reserve University] was looking for somebody to take over their chemistry department. Paul asked me whether or not I would be interested and then recommended me. Now, I am sure many of his former students and friends were not interested in this job as Western Reserve was not a leading research institution, but with my background I wasn't easily intimidated by challenges. I thought that this was a possibility and we moved to Cleveland. I'm very proud of my Cleveland days.

TRAYNHAM: When did you move to Cleveland?

OLAH: In the summer of 1965.

TRAYNHAM: It was very shortly after that that Case Western Reserve coalesced. It was also named recipient of a National Science Foundation Center of Excellence Grant and chemistry was included in that. I'm sure that gave a new spurt to the department that was emerging from the combination.

OLAH: When I say that I am proud of my Cleveland days, it not only includes chemistry, but some other aspects as well. When I arrived in Cleveland, I found the chemistry department of Western Reserve University on one side of the parking lot and the chemistry department of Case Institute of Technology, where I had some good friends like Jay [K.] Kochi and others, on the other side. Shortly thereafter, I started to think that this really made no sense at all. I started to talk it over with friends in both departments. To make a long story short, within two years, we combined the two chemistry departments. We succeeded in combining them in good spirits. You know, a merger of two entities is not always easy, but it worked out.

TRAYNHAM: There was no hostile takeover. [laughter]

OLAH: It worked out quite well, because my friends had some confidence in me since I served as a very reluctant department chair. My aim was teaching and research, and I considered it a service to be a department chair, and I thought my only purpose should be to help my colleagues. When we combined the two departments, to everybody's great surprise, including the two administrations, it worked out so well that it catalyzed the complete merger of the two universities. In 1969 after the two universities completely merged, I was able to get out of being department chair. This was my short tenure, but I am proud that we did something workable.

TRAYNHAM: Your comment about being a "reluctant" department chair reminds me of Norm [Norman] Cromwell's comment to me. We were both vice chancellors of our respective universities at the same time. He said that he thought chemists made very good university administrators because, unlike persons from many other disciplines, they had something to go back to and they do not feel that they have to defend their administrative position.

OLAH: That's true, but at this stage of my life, I can say this: I have known many fine chemists and fine people. Having observed this, I am very fortunate that I never had any temptation for "power," "influence," or whatever. Some fine people in science become administrators and I think that that's fine—universities need good administrators, leaders, and so on, but some people get used to this and they have difficulties going back to their research, teaching, and just being simple professors. To me, being a university professor is the <u>top</u>. It's what I always wanted to achieve and the most wonderful thing—<u>I</u> feel personally happy and satisfied with it. After all, look—I am not talking about struggling to obtain funding and all these things—you really have a great degree of freedom to do what you love to do. You have the privilege to continuously work with young people, not only to teach them, but also to help them shape their lives in a way, help them personally and in their career, which to me is the most rewarding. Therefore, even if I am director of this little institute, which I helped to bring about, but I am doing this mainly, I certainly won't say reluctantly, to help my younger colleagues. My real goal is to try to help them and I keep my administrative involvement to a minimum. I am well organized and I learned many things as a department chair, mostly when to say no and not to accept to many obligations. You must have gone through this experience.

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OLAH: Also I learned that my colleagues and even students, who came to see me, mostly came to me with problems. Soon I thought, "Heck, some good things must also happen to these guys." Although, they generally did not bother to tell me what they were. I acquired the ability to sit back a little and tell them in "what-they-can-do-to-help" terms, but also in "what-they-can't-do" terms. I never thought that I had a particular power over them or whatever. I still feel the same. Now, this doesn't mean, as I said, that I don't have high regard for good university administrators. Many of them are outstanding people and doing a great job. It's another question that administrators have the ability—they like to build their empires. They act involved in many things that I don't consider of prime significance, at least not for me. I'm a happy man the way I am. Look, I do what I love to do, and I am even making a living out of it. Now, how many people can say that?

TRAYNHAM: Aside from your duties as chair of the department, and then the combined departments of what became Case Western Reserve, what was the primary nature of your research that was going on and developing?

OLAH: Well, you know, I've always had widespread interests, an inquiring mind, and I did extensive research. Recently I was asked to put together with a colleague my selected papers with a commentary on them. The World Publishing Company, who also publishes for the Nobel Foundation, published them. I call the book of my collected papers with commentaries *Research Across Conventional Lines: Selected Papers of George A. Olah* (20) because I have never really followed any one topic exclusively. In my Cleveland days, besides my carbocation-superacid work, which led me here where our major emphasis is now on hydrocarbons, I started to seriously get involved in mechanistic problems in new reagents and reactions. This is basically synthetic chemistry, but one particular branch of synthetic chemistry. You know, organic synthesis is frequently associated with synthesis of large and complex target molecules. These days it also involves self-assembly, and all these other things. I was never really interested in getting involved in large synthetic projects. To prove that you can probably make a complex molecule with many asymmetric centers is a great challenge and I admire this work, but [Robert B.] Woodward, E. [Elias] J. Corey, and others have shown us that with ingenuity, perseverance, and a good group you can put together amazing molecules. There are many fine synthetic chemists who are doing this.

I was interested in trying to find new and improved reactions and reagents, i.e., new tools. After all synthesis needs methodology. I did a fair amount of the work developing new reagents and reactions. The Fieser's collection [*Fieser and Fieser's Reagents for Organic Synthesis*] of organic reagents, if you look it up, you find that for a long while I was very high up on the list of who developed new reagents and reactions (21). It was great fun. I should also mention that I have this philosophy that has always served me well. I work very intensively in a field—when I am involved in something, I really pursue it, but then at a certain point, I say, "This can go on for a long while, it's really not so exciting anymore." When I come to this point, I generally write or edit a book in which I not only summarize the field, which is very useful, but I also put into context what I have done in it. Over the years I did over a dozen such books, starting with *Friedel-Crafts Chemistry*, a whole series of other books on intermediates, reagents, and so on (22). This was also real fun, because it's better that you yourself write about your work than somebody else.

At this stage of my life, I am even involved in the luxury of not only writing about chemistry. I am back where I started. I have become interested in the philosophical and historical aspects of my field. I just finished writing my autobiographical reflections [*A Life of Magic Chemistry: Autobiographical Reflections of a Nobel Prize Winner*] (23). I took the advice of my wife and wrote something about my life experience, but it includes a fair amount of my thoughts on deeper meanings and aspects of science, life, and so on. Maybe it's the last thing that I will write. I don't know.

TRAYNHAM: You stayed in Cleveland for, what, twelve years?

OLAH: Yes.

TRAYNHAM: What prompted you to move since it seemed that your research program was moving quite successfully there?

OLAH: Well, we had a good time, we brought up our children, we enjoyed it, but there comes a point where you feel that maybe it is time to move on. We haven't moved too much despite our getting out of Hungary. After we spent twelve years in Cleveland, we felt that it was time to move on. There were both professional and personal reasons. The professional reason was that whereas my research was going very well, I had a feeling that I could apply my chemistry to the broader area of hydrocarbons, and some of its challenging aspects. Cleveland wasn't really able to accommodate what I had in mind. The personal aspect was that I was nearing fifty, and had started to wonder if this was the place where I want to spent the rest of my life. Also my older son was finishing university and my younger son was finishing high school, and they were thinking about moving to California. They felt that it would be wonderful if the whole family was in California. I had different possibilities before, but I never looked at them too seriously, but at this time, we started to think about it. An old friend, Sid [Sidney W.] Benson, called me up and said that I should come out and visit USC [University of Southern California in Los Angeles], where he had just came back from—he was here for many years and then moved up to the Stanford Research Institute. He felt that USC was at a point where they were seriously thinking about building up chemistry and that maybe I would be interested. That's the way it started. We have been here ever since and we love it.

TRAYNHAM: Good.

THACKRAY: How does the Loker Institute fit in?

OLAH: Well, you know, when I visited here, in the fall of 1976, I knew relatively little about USC. I must confess that I'm not into college football, but we used to watch the Rose Bowl on New Years Day and my family and I were very much impressed by the wonderful sunny weather here, while we were freezing in Cleveland and winter was still a long way away. USC played frequently in this event. This contributed to my interest in Southern California. During my visits, I met Zohrab [A.] Kaprielian, who was USC's executive vice president and an electrical engineer. This position was equivalent to that of a provost. He was a wonderful individual—completely dedicated to trying to build his university to excellence. He told me his philosophy. He said that universities could no longer afford to build up too many areas simultaneously. His idea was to try to attract some people who would excel in their field and hopefully with them he could lift the whole university. For whatever reason, I don't know why, he felt that I may have been such a person to be considered, but it became clear that the physical requisites to accommodate my work and group were limited.

USC had some very good chemists over the years, but somehow they never stayed long enough, and the emphasis was very much on chemical physics. Jerry [Jerome A.] Berson was here for twelve years, but I don't think that he was very happy here. Another friend of mine who was here only two years, but did remarkable things, which were not really recognized, is Ivar [K.] Ugi, who then returned to Munich, Germany. You see Ivar Ugi, really, is the father of combinatorial chemistry. Ivar first discovered multi-component reactions, which are the basis of combinational chemistry. Not only did he do this, but in 1971 he also published a book, *Isonitrile Chemistry*, by Academic Press (24). If you look it up, there are two chapters dealing with multi-component, combinatorial chemistry, and also a reasonable theoretical treatment of how to handle the data.

But, in spite of this, friends warned me that there wasn't too much you could do at USC. But as I told you, I am a very stubborn man—I managed in Hungary, I managed in Cleveland, and I wasn't really very scared about going to USC in LA [Los Angeles]. In order to get started, I had an idea. You know, this was at the time when oil shortages were of significance. After the first oil crisis there was public recognition that our country could face the problem of depleting oil reserves and the need to try to do something about it. I suggested to Kaprielian that a way that we could get started was if the university would consider a commitment to start an institute dedicated to the general, broad idea of hydrocarbon chemistry. He liked my idea and was able to sell it to the trustees of USC, so we came. The commitment was based mostly on faith since nothing was in writing. You know, I probably had more faith in it than my wife, who is much more realistic. She was more skeptical about whether promises would be transformed into tangibles, but I really never doubted it. A chance was good enough for me. Really, what I was given was a chance. Carl [M.] Franklin, who is now the vice president emeritus of USC and a friend, was a great supporter of my appointment and introduced me to his friends the Lokers. He [Donald P.] has passed away since, but Katherine [B.] Loker is still very active, and chairs the board of our Hydrocarbon Institute. They were a wonderful couple with no interest, involvement, or connection with oil or the chemical industry. Her father came to America as an immigrant from the Adriatic coast of Croatia in the later part of the nineteenth century. He was a fisherman, who eventually got his own boat, then other boats, eventually built a large tuna fishing and canning company, and made a fortune. She was an alumna of USC. Her husband was a most interesting man. He was a Harvard man who moved to California and looked at USC with a benevolent eye. He originally became a movie actor, who performed in fifty pictures under his stage name. Eventually he got involved in the family business, and proved himself a very shrewd businessman. They were wealthy people that somehow got intrigued by the idea of the Institute and made gifts to support it, completely unselfishly. I should say that they also supported Harvard University at the same level. We had <u>some</u> competition.

THACKRAY: In hydrocarbons or just generally?

OLAH: Just in general. Harvard's Memorial Hall, for example, was remodeled a little while ago. Mrs. Loker was one of the major donors for that.

TRAYNHAM: Can you identify, aside from your years of employment at Dow, any utilization by the chemical industry of your carbocation research?

OLAH: Well, you know, this is a very broad field, because acid catalyzed reactions, of course, play a very major role in many industrial processes, particularly the oil and petrochemical industry. This is, incidentally, why we embarked on our institute here, which is dedicated to pursue fundamental research on the broad topic of hydrocarbons as well as teaching at the advanced level to provide for the next generation of researchers who will work in the field. We are not only doing fundamental research but we also consider that some of our results have, as you asked, practical application. There are quite a few areas where it has, and I hope that there will be more. I mean, not just our work, but all others as well. When you talk about superacids or high-acidity systems, many of the widely used catalysts have strong acidic characteristics. Incidentally, superacid is not a scientific definition. Superacid is an arbitrary selection of acids that are stronger than conventional acids. Ron Gillespie suggested that in the liquid phase, acids that are stronger than 100 percent sulfuric acid should be considered superacids. The definition of solid superacids is still being discussed.

Now, on the Hammett acidity scale, which is a logarithmic scale, sulfuric acid is -12. Some of the superacids—ones that others and we are working on—are reaching to -30. This is a logarithmic scale, so we are talking about an acidity increase over sulfuric acid of 10^{18}. You know, that is much more than the U.S. National Debt, which to me is a very big number. [laughter] There is a whole range of acidity—I'm not saying that all, but, for example, some practical zeolite catalysts—acidic zeolite catalysts are in this superacidity range. Therefore, understanding some of this chemistry is very significant. I'll give you some specific areas where our work has practical significance.

Alkylation chemistry of isoalkanes with alkenes. Acid-catalyzed alkylation first involves sulfuric acid, then anhydrous HF as a catalyst. This has a very significant industrial application. Lead-free high-octane gasoline contains alkylates, which are branched hydrocarbons—branching of alkanes can be affected very efficiently by acid catalysts. Now, you can either branch an already existing alkane or you can combine two smaller building blocks, like an olefin and an alkane in the alkylation process. These are generally acid catalyzed. It was realized long ago that gasoline needed to have improved properties. Adding lead additives improved the octane rating of gasoline and was used for a long while. The additives did the job. Later on, however, lead was phased out because of environmental considerations. During World War II, a major factor—some even believe affecting the outcome of World War II—was the ability of the Allies to use much improved high-octane aviation gasoline. The Battle of Britain, fought in the fall of 1940, is considered to have been significantly affected not only by the bravery of the young pilots of the Royal Air Force, but also by two technical achievements. One was the improved use of radar, and the other, high-octane aviation gasoline, which the U.S. started to deliver to England for the first time in the summer months of 1940. At the time, Hitler had overrun France and the lowlands, and the

German army was at the British Channel poised to invade England. But for whatever reason, Hitler decided that he wouldn't invade before his air force achieved air superiority, which they easily did, over France and the rest of Europe just months before. In this decisive air battle, things turned out differently. One factor was that the British Air Force was able to use the improved fuel they received from America, which allowed their planes to out-speed and out-maneuver their opponents. This gasoline had an octane rating by today's standards of about 100. The one that was used just months before, and which the Germans were still using, had an octane rating in the low 80s. Here was a technical development, which many believe contributed significantly to the outcome of World War II. The alkylation process was invented by [Vladimir Nikolayevich] Ipatieff and [Herman] Pines. Ipatieff came as an immigrant around 1930 to the U.S. after having escaped from the Soviet Union. He was a well-known Russian scientist who came from a noble family. During World War II, he was the Tsar's general in charge of the Russian's chemical-technological efforts, if you can call it that. When he managed to get out and came to the U.S, he was, I think, sixty-two years old and couldn't speak English. Despite this, in ten years he created the basis of the leading petrochemical industry in the world. He did it working for an industrial company, UOP, or Universal Oil Products, which had an interesting history that I will briefly reflect upon.

UOP was a joint technology company of the major oil companies. Due to concern for violating anti-trust laws, subsequently, it was considered by their lawyers that it would be favorable to make a public gesture and give up this joint technological company, which obviously violated the law (25). They couldn't sell it, so they were looking for somebody who would accept it as a gift. They even had difficulty giving it away. Eventually the American Chemical Society [ACS] agreed to accept it with some assurance that they wouldn't lose money on it. This is basically what started the ACS Petroleum Research Fund. In the 1960s, when ACS decided to sell UOP, they put the money in a fund to support research, which now has hundreds of millions of dollars as its capital. Ipatieff's hydrocarbon chemistry work indirectly became a benefactor of the American Chemical Society.

Now, I'm saying all this because Ipatieff's alkylation chemistry, after the war, using anhydrous hydrogen fluoride as a catalyst became a major process for industry. Hydrogen fluoride is, however, a volatile. It boils at 19.5 degrees. If it gets out into moist air, it forms an aerosol cloud that is very toxic. In the late 1980s, there were three major accidents in the world involving hydrogen fluoride, one in Texas, one here in the Los Angeles harbor area, and one in England. Therefore there was <u>major</u> concern about the toxic nature of hydrogen fluoride, and there was pressure to ban it. There were segments on American television shows, the BBC [British Broadcasting Corporation], and so on about it. Barbara Walters had a special on it. The BBC had a special. At the time, some industrial friends came to me and said that there is this major HF problem, and they tried but they couldn't really come up with an answer on how it solve it. Could I help? Whereas our little institute was not doing any work directly for industry, any contractor, or otherwise, but if we have basic chemistry, which we think has practical implication, then the institute patents it, and we cooperate with industry to find practical applications.

We found a solution to the HF problem. I am quite proud of it because we really tamed HF. We found that additives to hydrogen fluoride tie up the bulk of HF in a liquid ionic complex. You hear much about ionic liquids these days. I believe that our ionic liquid onium polyhydrogen fluoride complexes are really the first practically ionic liquid. One of them is the pyridine HF complex. You take pyridine and HF. You would think that it would just form pyridinium fluoride, but instead it forms a pyridinium polyhydrogen fluoride. The anion is a chain-like polyhydrogen fluoride ion, which can have anywhere from six to eighteen hydrogen fluoride units and is a liquid. So you have your HF complexed and instead of being a very volatile material, it's a quite stable liquid, which contains only a small amount of free HF in equilibrium. In case of an accident, you are not spewing up much toxic volatile HF into the atmosphere. You have a liquid that can easily be washed off and neutralized.

I talked before about my experience at Dow, and why they needed to use such a large amount of aluminum chloride in their benzene ethylation process. If you do research long enough, you store in your memory bank much useful information. You see, industry is also using large amounts of HF in alkylations. Tons and tons of a highly toxic volatile acid is involved. So you ask the question: "Why do you need this? If HF is a catalyst, why do you need to use so much of it in your reactors?" My industrial friends did not have an answer, but I figured it out. It's not the question of whether or not you need all that catalyst. You need a medium that is ionizing in nature to facilitate carbocationic reactions, because strong acids have low nucleophilicity and are ideal mediums for the alkylation reaction of combining an olefin with an alkene. Huge amounts of HF are used to provide the medium, not just the catalyst. With my modification, you can bind the overwhelming amount of your acid to form a stable ionic liquid, which is not free HF. It's very different. Take ammonia and ammonium chloride. Ammonia is a volatile toxic gas. Ammonium chloride is an ion salt. The same is the difference between free HF and the ionic liquid pyridine polyhydrogen fluoride complex. In this ionic liquid complex, you only have a very small amount of free HF present, and this is the catalyst. The process we worked out, which is now available to industry, is overcoming a major environmental hazard without forcing industry to give up a widely used major process. We are working on other environmentally significant processes such as safe oxygenates to replace MTBE [methyl-tertiary-butylether] and similar diesel additives. The trouble is—we must be ahead of our times—it causes difficulties. Industry is still reluctant to invest money in environmental improvements, which do not produce profit directly. Industry, presently, is also under very little pressure from agencies to do something about environmental problems.

I am frequently told, "of course, your improvement isn't producing any extra income." It may be true, but they may save lives. Look at the Exxon [ExxonMobil] Corporation. I think it would have been very wise to improve the safety of their tankers to avoid the Alaska spill, which cost them billions of dollars and could have been prevented. On the other hand, new tanker designs couldn't have been predicted to save money. In a refinery accident, and there are, I think, at least a hundred refineries in the world using HF, many people may be killed or injured. I think there's a great value in trying to prevent such things. Look, if you can prevent illness, if you can do something not to get seriously ill, to me, that's the best investment you can make. When you already have a fatal illness, you may spend a lot of money, but it is too late.

I am a strong believer in environmental improvements. In contrast to most so-called environmentalists, I think, however, that you need to pinpoint the problem and regulate it within reason. I accept that society has a right to regulate, but few people are talking about what modern science and chemistry can do to find new solutions. This will be a major challenge for chemistry in this coming century. We have the ability to develop new chemistry, which can solve problems in a way that still allows society to satisfy its need, but at the same time doing it in a safer and environmentally friendlier way. Methyl-tertiary-butylether, MTBE, is a current problem. It's a very widely used oxygenate additive for gasoline. California just banned it, however, for difficulties that arise when it leaks into ground water and poisons it. For five years, we had data, were telling people about it, and even published with friends from our medical school on the hazards of MTBE (26). We had extensive studies using a modified Ames test that showed the carcinogenic nature of this ether. Nobody was really interested. Now, it has become a public affair and was banned. Again, I'm not arguing that society doesn't have the right to regulate. On the other hand, I believe that we also need new solutions. MTBE can be replaced by safe oxygenates. One of the solutions that have been suggested is to use ethanol. This, however, is the politician's solution, and not a technical one. You know, many politicians are praising ethanol because in agricultural states it is very popular. Now, I happen to know a little about it. Ethanol is a very poor additive to gasoline. I grew up in Europe between the two World Wars. Ethanol was indeed added to gasoline. This is nothing new. It has serious drawbacks besides corrosivity. Ethanol is miscible with gasoline only when it's dry. If you get water into it, you get phase separation. If you get phase separation, your car stops. I'll give you an example. Here on the Los Angeles freeways after a cold night, you have, say, two million cars on the road. During the night moisture condenses in all the gasoline tanks, so half of the cars stall. You have a million cars stalling at the same time on the freeways. I don't think that many politicians would survive the public uproar. If you premix ethanol and put it in storage—dry alcohol is very hydroscopic. This is why strong alcohol burns, because it's a water absorber. It does the same thing in storage. I think it's a very poor idea. I should also mention that the first person to suggest the use of agriculture-based alcohol for industrial use—in this case it was not corn but grain—was [Vladimir Ilich] Lenin. Lenin, in his 1919 industrial program, called for the use of grain alcohol to build a chemical industry in Russia. The Russians who tolerated everything—they tolerated the Bolsheviks, they tolerated being killed, robbed, and starved, but on this point, they were unwilling to go along. They were unwilling to give up their beloved Vodka. It was the only project that Lenin himself withdrew. Brazil, in recent years, had a huge sugar cane to alcohol project, because they grow so much sugar in the Amazon Valley. However, they also gave it up. I don't believe ethyl alcohol is our solution. On the other hand, in the last two or three years we have developed some effective, safe oxygenated additives you can make from simple hydrocarbons. They have excellent octane numbers, and are nearly void of environmental and health problems. We feel that they will be used. Getting something from the laboratory into real life is not easy.

TRAYNHAM: Dr. Olah, can you tell us something about the focus of your current research?

OLAH: Chemists, as you know, always love to talk about their work if they have the chance, so do I. I believe that one should not rest on their past laurels, whatever they are. As long as you still have fun doing new research, you should do it. In recent years, after I received my Nobel Prize, I have been very heavily involved in two very different topics. One is very fundamental chemistry, which is probably the most interesting chemistry I have ever done. I also feel that this work, in the long run, may have significant implications on chemistry, but we are really doing it for the fun of exploring it. The other is a very different area related, as I said before, to my concern for the environment and the future. We have a significant effort going to try to attack some chemical problems of societal significance and environmental concern. In this context, we try to come up with or suggest new solutions based on new chemistry.

Let me talk about the first one. I would call it super-electrophilic chemistry, but you can also call it electrophilic solvation. Now, you know, solvation is one of the most significant phenomenon in physical, organic, or mechanistic chemistry, and it involves the interaction of different solvents, which have varied electron-donor ability with electro-deficient species. That's also what people did in solvolytic studies on carbocationic systems related to the non-classical ion controversy. We talked about this. The whole controversy boiled down to the fact that there was a rate difference between the rate of solvolysis of the exo versus endo isomers in the norbornyl system. The rate difference was found to be three hundred or higher. One of the major proponents, Winstein, said this is due to the fact that exo is faster, but Brown concluded, "The difference, three hundred, is all right." We measured it many times, but it's not because exo is faster; it is because endo is slower. There was a great degree of argument over the effects of different solvents and so on. Now, what we recently discovered, and which—if I am not very mistaken—could become important in chemistry, is the fact that super acids, i.e., a very high acidity system can also act as solvating systems greatly activating electrophiles. In the condensed state, there is no such thing as a naked proton. The proton is always associated with something. In water, it forms the hydronium ion, H_3O^4, in other media, a similar protonated species is formed. There are of course a large variety of other electrophilic reagents, which play a key role in chemistry, but people have not considered that they can also get protonated, or protosolvated, with superacids. To illustrate this let's take the case of electrophilic nitration.

We already talked about Christopher Ingold, who was truly a great chemist. I am a great admirer of what he did. Some of Ingold's most fundamental work was on acid-catalyzed nitration, where Ingold and his associates from their kinetic studies, but also some spectroscopy and other work, firmly established that in acid systems, the reactive nitrating species that is formed is the nitronium ion, NO_2^+. The nitronium ion, NO_2^+, has the same structure as CO_2. It's linear, so its nitrogen atom is linearly bonded to two oxygen atoms, and over it is a positive ion—it's electron deficient. A very large number of studies, including my own, always accepted, without any thought, NO_2^+ as the electrophilic nitrating agent. However, I had some questions in my mind. The first question focused on the fact that an electrophile is an electron-deficient species, and of course the nitronium ion binds to aromatics or even aliphatics at its nitrogen atom. In the nitronium ion, however, there is no empty atomic orbital on nitrogen, and there is no easily accessible molecular orbital for the interaction. So why is the linear nitronium ion such a good electrophile? The only answer that could be given was the fact that it's really a polarizable electrophile. The linear nitronium ion cannot bind on nitrogen, but if it approaches an aromatic molecule, say benzene, which is a π- electron donor, in other words, it's a nucleophile, a base—benzene, as an electron-donor, will start to push a bonding N-O electron pair onto oxygen, and in this way, the nitronium ion bends and develops a bonding orbital on nitrogen. This means that if you nitrate benzene, which helps to activate or displace the polarizable electrophile and allows it to react.

When we extended our studies to the reactivity of single bonds, we studied the nitration of alkanes and eventually methane itself. Compare methane, CH_4, with benzene, C_6H_6. In benzene, you have a π- sextet, which can interact with the nitronium ion, but in methane you don't have a π- system. As a matter of fact, in methane, all valance electrons are taken up, forming the Lewis two-electron, two-center bonds. When we tried to nitrate methane with nitronium salts in aprotic media, there was never any reaction. This was not very surprising based on the foregoing. However, we found that if we added some superacids, nitration did occur. When we put the nitronium ion into a strong superacid, even methane was nitrated.

Of course, benzene nitrates very well, and so do most other aromatics. But if you start to deactivate the ring and you introduce strongly electron-withdrawing groups, nitrobenzene is still nitrated to di-nitrobenzene, but di-nitrobenzene is not further nitrated to tri-nitrobenzene in aprotic media.

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OLAH: So, as I said, when we looked at nitration with nitronium salts, not only saturated hydrocarbon, including methane, but aromatics, which are highly deactivated in aprotic media or slightly acidic media, are not nitrated. Di-nitrobenzene is not nitrated further to tri-nitrobenzene. When you add a strong superacid to the system, something interesting happens and you tri-nitrate very efficiently with good yields. These and other observations caused me to think about what was happening. To cut the story short, we discovered that in these and other cases, the nitronium ion is no longer a nitrating agent. It is a mono-positive ion. You wouldn't think that this small positive ion does anything in acid media, but of course it has non-bonded electron pairs on oxygen. If you put it into a strong enough acid, the non-bonded electron pairs start to interact with the acid. Eventually you can protonate the nitronium ion to the protonitronium di-cation, NO_2H^{2+}. This is an entirely different species from the nitronium ion. It is a di-positive slightly bent ion with an empty pi-orbital nitrogen. I call it a super-electrophile. It has an enormously higher reactivity than the nitronium ion.

I am not saying that you always need this protolytic activation. If you go hunting to shoot a rabbit, you don't need a very high-powered elephant gun. If you have a difficult, inactive target, then you need to attack it with a highly reactive electrophile. Basically, what I discovered was that electrophiles, which can still interact further with strong acids—similarly to the nitronium ion, ions such as the acetyl cation, CH_3CO^+, which is also a very modest linear electrophile—can be protolytically activated. The acetyl cation, $CH_3C\equiv^+O$, is a carboxonium ion, but when activated by a very strong acid, the lone pair on oxygen gets involved with the protic acid. You can call this proto-solvation in relationship to electrophilic solvation. In the limiting case, *de facto* protonation of the oxygen can take place forming the protoacetyl dication, $CH_3C^+{=}^+OH$. This is again a very reactive species because it is bent and has an empty orbital on carbon. This is a general phenomenon. You can understand it in terms of diminishing or eliminating what Winstein called neighboring group participation. Winstein showed that suitable neighboring groups or heteroatoms can interact very strongly with electron-deficient centers. He also showed neighboring participation by pi and n- donor groups.

What I had discovered was how to diminish neighboring group participation by protolytic solvation or protolytic interaction of the neighboring group. Take, for example, a protonated aldehyde or a ketone. Protonated aldehydes or ketones are weak carbon electrophiles, like acetyl cations, because of the very strong participation of the neighboring oxygen. They are basically carboxonium species. If you use a strongly acidic medium, the acid can interact with non-bonded oxygen electron pairs, and therefore diminish the neighboring group participation, which vastly enhances the electrophilic nature of the carboxyl carbon. This turned out to be a very general phenomenon.

You can even affect hyperconjugative interactions. I received the Nobel Prize for my work on carbocations. I thought I knew something about ions such as the tertiary butyl cation, trimethyl carbenium ion. It contains a trivalent carbon atom to which three methyl groups are attached. We made it and studied it in detail. Much theoretical work was done on it too. We should recognize the fact that whatever we think we know in science may become obsolete one day. It is fun if you can make your previous work somewhat obsolete. To illustrate this, a few years ago we observed that when we took a t-butyl ion, which we can make—it's very stable in superacidic media—there is no deprotonation equilibrium and that's why the ion is persistent. Why I was able to make this ion as a persistent, long-lived species is because at high acidities there is no equilibrium dissociation giving isobutylene, which would immediately react with the carbocation under normal conditions. You cannot have an acid and a base together, such as an alkyl cation, and its derived olefin. Therefore, you observe the t-butyl cation as a long-lived species that can't have any isobutylene in equilibrium.

However, one day, for whatever reason, we took a solution of t-butyl cation and added deuterated superacid to it. To our surprise, it started to exchange hydrogens on the methyl groups with deuterium. Now, you could explain this easily if you have a deprotonation equilibrium with an isobutylene, which you then re-protonate or deuterate, but if you know that you have no olefin in equilibrium and it still exchanges, there is hydrogen-deuterium that you must find another explanation for. It is known that the t-butyl cation is internally stabilized by hyperconjugation. In other words, the CH sigma bonds donate back into the carbocationic center, which is called hyperconjugation. When you use a very strongly acidic medium, now the external acids start to interact, the same as I told you—decreasing the neighboring group's hyperconjugative. The reactivity of the t-butyl cation in superacidic media depends on the acid strength. In other words, with increasing acid strength we can even enhance the reactivity of good electrophiles, like carbocations.

This all ends up at the rather intriguing concept of the super-electrophilic activation of electrophiles by protolytic or electrophilic solvation. What I am saying is that there is an equivalent to nucleophilic solvation in electrophilic solvation, which affects any part of the molecule that can further interact with an acid, which cannot only be an unsaturated π- ligand, a non-bonded electron pair donor atom, but even saturated alkyl ligands with sigma-bonded interactions. This opens up much intriguing new chemistry. We have just submitted for publication, after I had a patent issued on it, the superacid catalyzed reaction of isoalkanes with carbon monoxide (27). The related Koch-Haaf reaction is a widely used reaction to make branched acids. It is based on the fact that olefins or alcohols react in acid with carbon monoxide to give branched acid. The mechanism of this is that you first form an alkyl cation, which then alkylates CO to an acyl ion, which is then hydrolyzed. This is an industrial process for making new-branched acids.

In contrast, my new reaction involves superelectrophilic activation of CO, which formulates like isobutane and converts it with very high yield and selectivity to a methyl isopropyl ketone, a branched ketone. This is a very different reaction from Koch's carboxylations. You activate CO to form an active bornyl cation, which reacts as a superelectrophile. I believe there are many other reactions to be discovered. So even at this stage of my life, I greatly enjoy pursuing this new chemistry. We really must have something, because our "batting average" to find intriguing, new reactions is very high.

In a more generalized way, protolytic solvation also means that we, organic chemists,—if I may call us this—were for a long time only thinking in terms of mono-dentate interactions, i.e., in only one-to-one interactions. Our inorganic colleagues, however, for a long while were telling us about bi- or multi-dentate interactions, and I believe the same is happening in electrophilic solvation. It's not only one acid molecule that interacts with one reagent molecule, but a number of them, and they can act as a very strongly activating medium.

We are also interested in solid acids and there may be relevance even to enzymatic systems. I am not involved in enzyme chemistry, but you know, even at enzyme sites you can have an interaction where activation is taking place, not by a single acidic interaction, but by a number of them simultaneously. Recently in Germany, [Rolf] Thauer and his group in Marburg, for example, discovered a new type of dehydrogenase enzyme that does not have metal associated with it. Much was written on the mechanism of hydrogenase and dehydrogenase enzymes based on metal coordination. *Chemical Reviews* a few years ago had a very fat special issue on this topic (28). Now, however, if your enzyme does not have metal in it, you can't explain the mechanism by metal coordination. [laughter] Thauer reluctantly suggested that this must involve some other type of activation, which must be very similar to my *in vitro* chemistry.

There are many new aspects of this new superelectrophilic chemistry. I am enjoying this very much. Maybe I am still lucky, you see, at this stage of my life to have found something that is probably the most intriguing chemistry I have ever studied, and may also have broad use in chemistry.

TRAYNHAM: In a recent issue of *Chemical & Engineering News*, you were among several chemists who were asked to speculate about chemistry in the 21st Century and you referred to your research on carbon dioxide fixation from the atmosphere and the production of alternative fuels (29). Can you tell us a little about that work?

OLAH: This is the other main aspect I am involved in with my colleagues in our Institute. Let me put it in some perspective from where we started. You know, we are using—to the great benefit of mankind— fossil fuels including oil, gas, and coal, which give us not only our energy base, but also allow chemists to produce all the products, synthetic materials, and so on that we use in our everyday life. Now, think about it. The Industrial Revolution was just slightly over two centuries ago. The use of oil really started maybe a hundred thirty years ago. The first commercial oil production was in Pennsylvania, around Titusville, around 1860, and some refineries started shortly thereafter. It really blossomed with the automobile becoming a major means of transportation. With the easy availability of oil and gas for producing electricity, they became increasingly competitive and convenient to be used instead of coal. Now, regardless of their source, these fossil fuel reserves are not renewable, at least not renewable on our human time scale. Nature took hundreds of millions of years to produce them, and mankind, in two hundred years, did a pretty good job of using them up. I don't want to make a prediction, but oil and gas will certainly start to run short within this century and coal in the next century. We'd better face up to this.

It's not just a question of our use. It's also a question of how many of us are users. The world population, according to the United Nations last fall, reached six billion. When the twentieth century started, it was a billion-and-a-half. There are all kinds of scenarios, optimistic or pessimistic, but it's clear that by the middle of this century, we will be nearing ten billion. The optimists say, "We will soon level off." Others say, "We don't know." All people want to live reasonably, so it's not just a question of whether everybody in India or Mainland China will drive a car. A steady increase of the standard of living increases the need for energy, and this will cause, whether we like it or not, a major impact on our lives.

Now, what are we doing about it? I'm afraid, very little. You know, we have this institute here, the Hydrocarbon Research Institute. I'm even embarrassed to say that the sixty people working here—eight professors, their students, and post-docs—represent probably more capacity to do basic, long-range research than the three major oil companies combined. This really scares me because it looks to me that there is very little concern about the long-range future. Now, I guess the major oil companies, if one day their business isn't profitable anymore, can walk away from it, close down, and go into other business ventures, but mankind will be here. Our grandchildren and their children will be here and they need energy. They also need materials based on hydrocarbons.

The question is what can we do about it? One thing, of course, is to use what we have more efficiently. We must also use oil and natural gas as chemical raw materials and not only to generate energy or as a fuel. As you know, Fischer and Tropsch in the 1930s worked out a very reasonable way to convert coal and later natural gas into synthesis gas [syngas], a mixture of carbon monoxide and hydrogen via incomplete combustion, and then use syngas to put it together either to produce mixtures of hydrocarbons or methyl alcohol. Methyl alcohol in the world is produced today exclusively from syngas. Now, the problem with syngas is twofold. First you use a fossil fuel to produce it, which, as I pointed out, is limited in its availability and is not renewable. Or, alternatively, you need to find some ways to get away from fossil fuels and find a new approach. One approach we are pursuing is to convert methane directly to methanol without going through syngas. When you produce syngas, besides all the other problems, you use up half of your energy in the combustion process, so it's a terribly wasteful process. I am no great believer in the future of syngas, not only because of what I said—I consider it wasteful and so on—but also because I always try to pursue my own new chemistry. We are working hard on a reasonable way to convert methane to methanol without going through syngas using a very selective process, which is basically utilizing a catalytic halogenation step with subsequent hydrolysis and regeneration of the halogen in a catalytic cycle that allows this conversion to be both selective and economically feasible. This is to find a solution for our long-range independence from fossil fuels.

All hydrocarbons, when you burn them, the carbon in them forms carbon dioxide, the hydrogen forms water. This is why they are not renewable, or the process is irreversible on the human time scale. I started a while ago to think about it. What are the possibilities to reverse this process? Can you take carbon dioxide from the atmosphere and reconvert it by chemistry to useful fuels and chemical materials? Of course, nature does it. When you look out at everything, what is green is a wonderful factory of nature's doing photosynthesis. Capturing carbon dioxide from the atmosphere, this is close to a miracle itself because the concentration of carbon dioxide in the atmosphere is very low. It's around 350 parts per million, i.e., 3.5×10^{-5} of a percent. It's very low, but still it's essential for our life. Without carbon dioxide, we wouldn't be here. Nature manages this. Part of the problem is that it only recreates plant life. It doesn't form hydrocarbons directly. It takes nature an additional long, long time to do this and we don't have hundreds of millions of years to sit back and wait for it, so what we need is to use chemistry for this purpose.

Can it be done? We know that we can do it in the laboratory. Can it be done economically? That's the big question. We now know that it is possible to take carbon dioxide and reduce it to methyl alcohol. There are all kinds of known ways to reduce carbon dioxide to methanol. You need for this, of course, hydrogen, and in order to produce hydrogen, if you just split water, you need much energy. So, basically, if we could have unlimited energy, which hopefully one day we may have, maybe from fusion, then we could seriously consider that if energy is not a problem, we can split water and use the hydrogen to reduce CO_2 to methanol, but we don't have unlimited energy. The question of what good is it to make methyl alcohol also comes up. Maybe I am a visionary. Maybe I am ahead of my time, but I believe that whereas the nineteenth century was dominated by coal, in the twentieth century oil and gas was added, in the twenty-first century we will start to see methyl alcohol becoming an essential raw material for mankind, not only as a fuel and for energy, but also to produce all the materials that we are now producing from oil and gas as well as coal.

Why am I saying this? I got involved with my colleagues and friends from Caltech [California Institute of Technology] and the Jet Propulsion Laboratory of Caltech ten years ago in a project that was directed to find a new generation of fuel cells. Fuel cells are devices that convert chemical energy directly into electricity. This fuel cell concept is a hundred fifty years old. A Scotsman, John Groves, in the middle of the nineteenth century observed while studying the electrolysis of water, i.e., using platinum electrodes in a cell and passing through current, which forms hydrogen and oxygen—he may have mixed things up. He observed the combining of hydrogen and oxygen in the cell over the platinum electrode and water was formed. In the process electricity was also produced.

For a long while, this remained a curiosity. Nobody made any use of it. Then came the space age and it became necessary for space vehicles to have a reliable and sufficient source of electricity. Batteries were not sufficient. Batteries still are not efficient, useful storage devices. They are still very heavy and can only store a limited amount of electricity. On a space flight, say on the shuttle, you also use solar panels to produce electricity, but for a constant source, lead-acid or even lithium batteries are insufficient. So they dusted off the fuel cell concept and were quite successful. They use hydrogen-oxygen-burning fuel cells in all space vehicles and they worked very well, most of the time. There is, however, the danger that when you use hydrogen and oxygen, a devastating accident can occur. Hydrogen can combine with oxygen in a very explosive nature. The astronauts are very brave people. They ride rockets, sitting on top of tons and tons of liquefied hydrogen and oxygen. In civilian use, only in a big, static installation can you handle hydrogen and oxygen reasonably safely. You can do it, but otherwise it's a very serious problem. Using a high-pressure storage vessel in itself causes a grave problem. When we started our project, at the time sponsored by the [United States] Defense Department, who was interested in developing reliable, small sources of electricity based on the fuel cell principle, we followed another path.

As I mentioned, most proceeded using the original hydrogen-oxygen principle, sometimes also using liquid hydrocarbon fuels, which are first put through what's called the catalytic converter. The catalytic converter is like a small Fischer-Tropsch unit. It splits the liquid hydrocarbon fuel, which can be diesel, gasoline, or even methyl alcohol, into hydrogen and CO. Then they separate the hydrogen from CO. The CO is oxidized into carbon dioxide and is exhausted while the hydrogen is used in the fuel cell. In recent times, fuel cells have become a very popular topic. The public knows about it. All the big carmakers are working on fuel cell cars. They are all using the same principle. They are all using hydrogen and oxygen as gases or generated via a catalytic converter.

We pursued from the beginning an entirely different approach. We said, "Let's see whether we can build a fuel cell around a liquid fuel used directly. Not hydrogen at all." We never worked with a hydrogen-oxygen fuel cell. We never used a converter. We developed a methanol-burning fuel cell in which methanol can be directly burned in the fuel cell. This has, of course, enormous advantages, which vastly simplify the device. You don't have the whole process of using, preparing, and separating hydrogen with all the problems associated with its handling. There is another major problem involved, which is also difficult to overcome if hydrogen is generated via a catalytic converter forming syngas, carbon monoxide must be completely separated. The fuel cells use catalysts, and these catalysts are all poisoned by traces of carbon monoxide. If you generate syngas, i.e., a mixture of hydrogen and CO, even if you separate it—it's very difficult to separate the last traces of CO, and—if you have any CO in your hydrogen feed, your catalyst will be rapidly poisoned. I don't think any motorist would enjoy riding in a car with the uncertainty that his fuel cell may be poisoned at anytime.

Anyhow, we were quite successful. The joint USC-Caltech developed direct-oxidation-methanol fuel cell is now a reality. It's licensed to some of the big automakers working on its practical development—also for smaller applications like cell phones, laptop computers, etc. Our main interest, however, remains the availability of natural fuel. Fuel cells, incidentally, are very efficient devices. If you have an internal combustion engine or any thermal engine, you are running up against the Carnot limitation and efficiency depends on increasing temperature. With a fuel cell there is no Carnot limit, so the fuel cell can achieve very high efficiency. We are now at around 40 percent efficiency, which is twice as good as the most efficient internal combustion engine, and hopefully we can improve it further. We are also very much interested in where the methanol is going to come from. I was just expressing to you my view that the future of methanol is not in Fischer-Tropsch-based-methanol using a fossil fuel, but in CO_2 conversion to methanol.

What we have done so far is only the beginning, but it's quite an interesting beginning. We started to work on the principle that maybe we could reverse the operation of our fuel cell. Our fuel cell burns methanol and produces CO_2, water, and electricity. We started to work on reversing it. How about if you take CO_2 and water in your fuel cell and you operate it backwards by passing electricity over the catalyst? Of course you must pass electricity through it and the electricity must come from somewhere, but basically, we <u>know</u> that we can reverse the process. Now, that's quite important because even today our fossil fuel burning power plants in off-peak periods have excess electric capacity. Atomic energy, I think, is one of the tremendous achievements of the twentieth century. It's very unfortunate that in the minds of many it is mainly connected with the development of the atom bomb. Therefore, society equates atomic energy with the atom bomb. Our atomic power industry is practically at a still stand. Not one atomic power plant was built in the last twenty years in the U.S., and none are being planned, which is a tragedy. I am <u>not</u> promoting irresponsible use of atomic energy. I fully realize the safety hazards, which need to be overcome, and also we must find safe ways of disposing of radioactive waste. Nevertheless, I am firmly convinced that we have no choice, not only in the U.S., but worldwide. In this century, we will use atomic energy on a massive scale, but we should make it safer and cleaner.

Even today in our fossil fuel burning power plants, whether they burn coal, oil, or gas, the use of electricity is cyclical. In off-peak periods, the use of electricity is much lower than peak periods. You can feed electricity into a power grid and so on, but even in our great country where the time difference from one coast to another is three hours, there are still lengthy off-peak periods where our power plants have much excess capacity. Power plants could, in these off-peak periods, use their own electricity to recycle some of the excess CO_2 they are producing. Why is this important? There is a thing called Global Warming, which is a major environmental concern in our society. It reached such a point three years ago that the world leaders of a hundred and sixty countries came together in Kyoto and signed an agreement that basically pledged that the industrial world would decrease by 2020 its carbon dioxide emissions, there would be exceptions for the underdeveloped world and credit for others, for a while. This is all wonderful, except that nobody has yet suggested how this really can be achieved. The simple truth of the matter is that we need more power, we need more energy, and if we don't use atomic energy, we are stuck. Saving and trading carbon questions between the rich and poor countries is not going to solve the energy problem of the world.

Look, I know all about alternate energy: hydro, wind, solar, and etcetera—I'm all for it. But they are all of limited scope in the overall picture. The truth is that we need a lot of energy, and we can't produce it without using atomic energy. As long as we are in our present situation, we are facing the fact that we are burning fossil fuels excessively, which are increasing the CO_2 content of the atmosphere, which affects global climate. The question is to what degree. Now, a lot of people argue—I wish I could say that this is all hogwash, but it is not. It goes back a hundred years. There is very solid scientific data showing the relationship between carbon dioxide content and temperature. However, the question is what is the course or the consequence. In any case, what are we going to do about it? There are all kinds of schemes. One is to collect CO_2 and sequestrate it at the bottom of the oceans. I don't think this solves the problem, and it's very expensive. What I think could be done, even now, is to take at least some sources where CO_2 emissions are very high, I'm not talking yet about trying to take out CO_2 from the atmosphere at random. This is tough because the concentration is so low—I mentioned to you—350 parts per million. I mention this just as a curiosity that the noble gas argon is present in the atmosphere in a concentration that is twenty times higher than the CO_2 concentration, but this low concentration of CO_2 is essential because it is a greenhouse gas, it has an effect. We could at least diminish CO_2 emissions by taking out some of it, where it's very highly concentrated, and therefore more easily handled, and recycle it through chemistry. Eventually, many energy sources will be used. Photoelectric energy will be used. The sun, of course, provides unlimited energy and is good for another four-and-a-half billion years. Maybe we could find enzymatic ways to cleave water to produce hydrogen. I am a firm believer that it's in our ability even <u>now</u> to convert CO_2 to methanol. Thus we should seriously consider the use of methyl alcohol as an efficient fuel for fuel cells with the understanding that methyl alcohol can be produced long after our oil, gas, and even coal reserves have been exhausted. CO_2 will be around on earth as long as there will be life on earth. Chemists, who are generally blamed for all kinds of pollution and so on, are the people who will make this work. Incidentally, atomic energy is really the only "clean" energy not producing carbon dioxide as a product.

It is looking ahead in time, but I do believe that the generation we are educating now will find it very challenging to work on some of these problems. Why else is methyl alcohol good? A lot of effort was done in the 1980s to convert methyl alcohol directly into gasoline. Mobil [ExxonMobil Corporation] worked out a process based on zeolite ZSM-5 where they were able to convert methyl alcohol into gasoline, or aromatic hydrocarbons. All this effort was based on syngas-based methanol. In a way, it was really improving Fischer-Tropsch by going through methanol as methanol goes over the zeolite catalyst to give a better mixture of hydrocarbons. What we worked out were ways to convert methanol with catalysts that are not zeolitic, although other zeolite catalysts found by Union Carbide, UOP, and others were also very effective. You can take methanol and quite efficiently condense two methanol molecules to ethylene and water in a bimolecular dehydration. The usual dehydration of methanol, of course, is that two molecules of methanol give dimethyl ether, which is also a bimolecular dehydration reaction. Over acidic/basic catalyst even non-zeolitic ones, you can, with high efficiency, convert methanol into ethylene. Or if you want, you can carry the reaction further and make propylene. Now this, to me, is a very significant situation, because if you can make ethylene and propylene, by known chemistry, then we can make all the compounds that we presently make from oil or natural gas. I am not such a visionary that I will say that in the future methyl alcohol could become a less essential chemical. It is a clean and efficient fuel in fuel cells, but also a very efficient raw material to prepare all the hydrocarbon products, which we are presently making from oil and gas.

I feel that in the long run, this probably will be a quite significant aspect to fulfill our society's need. You see, I am trying to emphasize that we should try to provide society with its needs in an environmentally friendly and clean way, but through new solutions. There are presently few chemists who have real interest in hydrocarbon chemistry, or even catalysis. I don't need to tell you this. The glamour fields these days are biotechnology, computer technology, software, and all these other things. Someone working on such a trivial problem like methyl alcohol or ethylene is considered to be something of an oddball. I have a feeling, however, that when we come to the point, which will inevitably come, it won't be a crisis that happened overnight. Look, oil is still cheap. A year ago, it was eleven bucks. Now it's twenty-seven or twenty-eight bucks. I don't want to scare anybody, but I think that within a decade or two, we can have oil prices of a hundred dollars because when supply goes down and demand goes up, you don't need to be an economist to know what's going to happen. Consider the bargain we still have. You go and fill up your car and you pay for a gallon—a dollar seventy or maybe two dollars. Imagine, if you will, the need to make this by synthesis. Methyl alcohol will become a key fuel. We have the infrastructure today to dispense methyl alcohol into cars instead of gasoline. I'm a great believer that chemistry—much maligned chemistry—will one day be credited that it lived up to man's expectation and helped to solve a major problem and provided for future generations a relatively clean and efficient fuel. Also when oil, gas, and coal are gone or getting scarce and expensive, we will have an alternative raw material based on the air available to us.

TRAYNHAM: As you move in your current research activities along the lines that you've just been discussing so interestingly, namely the conversion of carbon dioxide, a by-product of industry, directly into methanol, do you think that you're going to have to change the name of the institute from the Hydrocarbon Institute since you're not passing through hydrocarbons on the way? [laughter]

OLAH: No, but we are making hydrocarbons. Look, I'm very proud to be a chemist. I am horrified by some of my colleagues who now rename their chemistry departments to departments of chemical-biology. Some of our premiere institutions don't have chemistry departments anymore. They have chemical-biology departments. I am proud to be a chemist and not a hyphenated chemist. When you talk about hydrocarbons, you are really talking about organic chemistry. What exactly is organic chemistry? Once it was said that organic chemistry is the chemistry of carbon compounds. Of course we know now that there are many carbon compounds that have nothing else in them like fullerenes, buckyballs. Similarly many carbides are not considered organic compounds. Thus, there are many carbon compounds, which have little to do with the real chemistry that organic chemists are involved with. People are now starting to seriously say that organic chemistry is the chemistry of hydrocarbons and their derivatives. In this context, I'm happy to point out that methanol is also a derivative of the simplest hydrocarbon, methane.

[END OF TAPE, SIDE 7]

OLAH: As a university professor, one of my great pleasures in life is the privilege to work with bright young people all the time. My former students—and I include not only my graduate students, but also my post-doctoral associates—are really my wider scientific family and I am very proud of them. Of course, you realize, talking about my life, that I never had the good fortune to be associated with a leading academic institution. I came from a small Central-European country, I worked in industry, worked at a small university in Cleveland, Ohio, and I came here to the University of Southern California, which I love and do my best for, but still cannot be considered a prime institution. Therefore, I think that my students have probably a harder time than if they had worked with somebody at a leading institution such as Harvard, Princeton, Stanford, MIT, or Caltech. I am not complaining about it. I think that premiere institutions rightly attract the best students in the country. They also have outstanding faculty and so on. Also their graduates are sought after for academic and industrial positions alike not just because of what they achieved, but also because they come from a blue-ribbon institution, which gives them this aura. Well, coming out of Case Western Reserve University or USC is not the same. Nonetheless, over all the years, I've had many wonderful, dedicated, good students that I'm very proud of. Some of them are now university professors, but many others are doing very well in industry. In academic life a pedigree is very important. Look, my own department here is very much inclined to hire anybody who would come from an institution like Case Western or our own school. We hire graduates from the big name schools. But it's all right that maybe the larger part of my students have gone into industrial jobs. It's wonderful to see that most are doing well. I had, over the years, I think, over two hundred or so graduate students and post-doctorals, and I'm very proud of them all.

TRAYNHAM: Do your university duties here at the University of Southern California include classroom teaching, or are all of your duties associated with the research institute?

OLAH: I am in a somewhat privileged situation now, both concerning my endowed chair and, you know, this Nobel Prize impresses some people especially university administrators. They give me a substantial degree of freedom. I like to teach, and it may surprise you that I even like to teach freshmen. I have been teaching, for some years now, freshmen seminars. I do this for non-science and non-chemistry students. It's great fun because I believe that everybody should have some literacy in the sciences. You don't need to become a full-fledged chemist or scientist, but you need to have some understanding of the sciences. I will tell you a few topics that I have taught. With two colleagues—one from economics and one from linguistics—we taught a course on the interrelationship of science, the humanities, and economics. As a text for it, as background reading material, we used [Johann Wolfgang von] Goethe's *Faust* (30). Now, you may ask, "What has Goethe's *Faust* to do with this?" A lot. As I said, I read a lot, so I know a fair amount about Goethe, who was a real renaissance man, a great poet, and so on. He also had much scientific interest. Some of his ideas, by today's standards, were naive and wrong. He was a great proponent of [Isaac] Newton and he had his own idea about light. He never believed that light could be broken up and so on. He was also involved in economics. He was a finance minister of his German principality for a while. I'm telling you all this because his famous *Faust*—the first Faust, the old Faust is an alchemist's story. Faust is—you may call him—a chemist. An alchemist. He made a pact with the devil and all this, but it is an alchemist's story. He wants to make gold, using the philosopher's stone. The second part of *Faust*, which he wrote much later in his life, and which was published when he died, is a different story. It even has an economic aspect. It was written at the time when paper money first came into use. Goethe uses the example of paper money, which was nothing more than a piece of paper guaranteed by a reliable person—a prince, king, or banker—that gave assurance that the presenter could get gold for it. Goethe emphasizes that paper money on its own can create new wealth, because it represents capital, which can then create all kind of things. His conclusion is that whereas attempts to make gold with the philosopher's stone failed, wealth was really achieved in a different way, through the use of paper money.

Students react very well when you challenge them. This year I will teach a course entitled "Societal Needs, Environmental Concerns, and Scientific Solutions." I try to challenge them about the view that society has needs and that we should rightly be concerned about the environment, but at the same time we should try to find solutions. Besides my own examples in chemistry, I will use two other examples. One is the DDT, dichlorodiphenyltrichloromethylmethane, story. Rachel Carson's *Silent Spring* was a very moving book (31). It really created the environmental movement, and there is much good and truth in it. On the other hand, I think she missed one crucial point, namely, that condemning DDT because it does harmful things doesn't mean that we should not try to find solutions to its side effects. Look, millions of people are still dying of malaria. In the Third World this is an enormous problem, and I think there is a relatively easy solution—at least I know of one. We worked on it. We are still, but we are, I think, at a point where I can say something about it. You can make safe insecticides, which are as good as DDT, and do not accumulate in the food chain. You see, the trouble with DDT is that it's too stable. DDT has no high toxicity in humans, but it builds up because it's so stable in the food chain and has all these side effects. Now, generally, we try to make materials and compounds more stable. In this case, the opposite is needed. What we need to do is to make compounds that are effective, but not very stable, so that they won't accumulate.

This is not an unreasonable approach. I don't know that our compounds will ever make it into practical use. Nobody wants to touch the problem. First of all, we need to give them a new name. We never should mention a DDT analog. But it could be a solution to the malaria problem, which is very real, but few are aware that a chemical solution is possible to fight it without endangering the environment.

Because we're in California, I mentioned the MTBE question. Again, a problem was created with this gasoline additive. It was realized that when seeping into ground water, MTBE can poison it, but instead of just banning it, I think we should try to come up with a solution—better substitutes. CFCs, chlorofluorocarbons, were a relatively easy problem. It was realized that they harm the ozone shield, so to eliminate CFCs—nothing really harmful is happening if you shave with an aerosol, which has something else in it other than CFCs as its propellant—you can also make refrigerators that have some other coolant in them, but you can't ban CO_2. Look, even politicians can't ban CO_2! [laughter] They can ban DDT. On the other hand, as you realize, the industrial countries are very much blamed for Global Warming because they burn fossil fuels. Kyoto, however, will not respect a worldwide ban because—at least in my mind rightly—to balance a better living for millions of people in the world against environmental harms, which we must minimize. What I am trying to say is that we can help the world by developing—we, the advanced countries that have a strong scientific base—new solutions. I also tell my students—maybe I should try to tell this to politicians or industrialists, too—that to me, green chemistry also means profitable chemistry. Green is also the color of the greenback. I don't think that there should be a polarization where on one side are the consumers who are hurt, and on the other, industry, who only wants to make profit. I don't think any industrialist really wants to hurt anybody. I think that industry can make a good profit by developing new advanced environmentally benign processes that serve society's needs, but at the same time are also economically feasible.

THACKRAY: George, I have one last question. There's been a lot of literature about the impact of the Nobel Prize. What's your own personal experience, positive and negative, about the impact of the Nobel Prize?

OLAH: Again, I'll try to express just my views, and please feel that I have the highest admiration for my colleagues, and I understand them. For me, it's a wonderful recognition, of course, but it really hasn't changed me. I really don't think that because the Academy in Sweden gave me a prize, it has made me a different, better, or "greater" human being. I greatly appreciate it, but I stayed the same. I still enjoy my chemistry. In the book I'm writing, I tried to comment in a chapter on this (32). I also probably learned to live with the prize. I keep my proportions. Most importantly, I learned to say no to many things. I concentrate on what I really think is important in my life. I try to avoid the trap that some of my colleagues have fallen into when they get the prize that all at once people consider you to be a universal oracle on all problems from politics, to ethics, everything, but it isn't so. I think that it's a wonderful recognition, but that's it.

TRAYNHAM: You have won other awards as well. In fact, now there's a major award that bears your name that the American Chemical Society gives annually in petroleum chemistry—the George A. Olah Award in Petroleum Chemistry. Can you comment any about that award and how it came about?

OLAH: Yes, I will. I hope I'm not hurting anybody's feelings, because I can't tell you how this came about. I also want to comment on something else. A friend and colleague of mine, Art [Arthur] Adamson, who was somewhat involved in ACS affairs—I am not—mentioned to some mutual friends, including Dr. Carl Franklin, that he learned that there was an award of the American Chemical Society called the Petroleum Award, which was going to be discontinued because the oil industry couldn't afford to finance it anymore. Art suggested that maybe if they could find a sponsor, or raise an endowment, it could be continued. From what I understand, Dr. Franklin and some others volunteered to put an endowment towards it. I also understand that Dow Chemical pledged a significant sum to it, and I guess Ted Doan had something to do with it. For whatever reason, the ACS decided to name the endowed award to be given in perpetuity for me. I greatly appreciate that, however.

Let me add to what I said before about the Nobel Prize as not being important to mention, however, I am grateful to my adopted country and for the opportunities I have had from USC, my university. We donated a significant part of our prize money to establish a chair here and one of my colleagues, Professor [G. K.] Surya Prakash, is the chair holder. In Hungary, my native country, we also helped to establish an award for young chemists and so on. Look, this is nice. When you get an award, it is nice to give back some of it where it counts most to show your gratitude. But again, I do not have the false notion that I am now some type of special person just because I received an award.

TRAYNHAM: Well, it's an interesting coincidence that this year's recipient of the George A. Olah Award in Petroleum Chemistry is one of your colleagues here at the University of Southern California. I know you had nothing to do with that.

OLAH: Indeed, I had nothing to do with that.

TRAYNHAM: But it's an interesting coincidence.

OLAH: Well, you know, what I am trying to do here at our institute is to attract some good young people because the future is in young people.

TRAYNHAM: Do all of your co-workers at the Loker Institute have appointments in the department of chemistry?

OLAH: All the senior staff members are professors. We all teach and work for our salary, like everybody else. In addition, we have, I think, a pleasant Institute, which is a home for us and for our students, I think—it's very nice to have people with mutual interest interact with the Institute. The sum of our efforts, I believe, is more than individual efforts.

TRAYNHAM: Do you have graduate students working in the Institute with you?

OLAH: We have graduate students and post-doctorals, and, on occasion, undergraduates also doing research. We have a program where we bring in undergraduates in their last semesters. Germany has a specifically wonderful program for such, the Adenauer Fellowships [Konrad-Adenauer-Stiftung Internship], where an undergraduate can get this fellowship for such study. We had quite a few of them with us. We have some very selective undergraduates, but mostly graduate and post-doctoral students.

TRAYNHAM: Does the staff at the Loker Institute include permanent non-faculty technicians?

OLAH: As you know, in the U.S., this does not spoil us, particularly in chemistry. We are a completely self-sustained entity. We have our own facilities, including spectroscopy and others, and we have a small but very efficient staff.

THACKRAY: George, I said that I had asked my last question, but here's a supplementary one. Since you're the grandson, chemically speaking, of Emil Fischer, what similarities and differences do you think he would see in you, the Southern California professor? [laughter]

OLAH: Yes, he's probably turning in his grave that one of his grandsons got so far away from him. [laughter] On the other hand, you know, I am still a natural product chemist. Methane is a product of nature. Even CO_2 is produced in nature. Look, America is a very different place than Germany. Incidentally, Fischer's son came to the U.S. and was a professor at Berkeley for a long time.

THACKRAY: I didn't know that.

OLAH: That's right. I think he wanted to get away from his father's shadow. As a matter of fact, if you visit the Deutsches Museum in Munich, there is an exhibit of Fischer, including his lab chair, some of his books, and so on. Fischer's son, who had kept his father's papers and memorabilia, donated everything. So you see, even Fischer's son got to go to America. I am proud of my Fischer connection. I don't know, however, whether he would recognize me. [laughter]

TRAYNHAM: You mentioned earlier in your discussion about your philosophy and your approach to your current research focus that you are hopeful of making the world safer for your grandchildren.

OLAH: If it is possible—if only by contributing in a minimal way.

TRAYNHAM: I want to lead into this question. Do you have grandchildren at the present time?

OLAH: We have two, a boy who is six and a little girl who is four. Each of my sons has one. I hope none of them will become chemists. [laughter] It's not good to have too many chemists in the same family.

TRAYNHAM: Have either of your sons become a chemist or a scientist?

OLAH: My oldest son has an MBA, while my younger son majored in chemistry at Stanford, but my everlasting gratitude is to Stanford because by the time he got his bachelor's degree, he had decided he wanted to be a physician. That's what he's doing. He's a very successful physician in Pasadena.

TRAYNHAM: What's his specialty?

OLAH: He is an internist and is not in research. He has seen too much research through his parents. [laughter] Even from the beginning, he was dead set on what he wanted to do. He wanted to help sick people. We need dedicated physicians. I'm very pleased because I'm very well known in Pasadena, not necessarily as a chemist, but as the father of my physician son whose patients include many of my colleagues from Caltech.

TRAYNHAM: And your other son?

OLAH: My other son has an MBA. He works with a large life-insurance company.

TRAYNHAM: Can you think of anything else that you would like to include to complete your oral history for this particular recording?

OLAH: We had a nice discussion. I would like to end by thanking you two gentlemen for coming and listening to me. As you realize, I was delighted to tell you about my life and my work. I don't know whether or not many will have an interest in it, but I thank you for coming. It was a real pleasure.

THACKRAY: George, we thank you. I think you're very well known in the profession as someone full of fascinating ideas, and your own life story certainly has its fascination. Thank you.

OLAH: Thanks again for coming.

[END OF TAPE, SIDE 8]

[END OF INTERVIEW]

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