THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

GORDON M. KLINE

Transcript of an Interview
Conducted by

Jeffrey L. Meikle

in

Lake Worth, Florida

on

15 and 16 May 1987

Gordon Kline
JH
3/15/96

BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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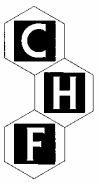

GORDON M. KLINE

1903 Born in Trenton, New Jersey on 9 February

Education

1925 A.B., chemistry, Colgate University
1926 M.S., chemistry, George Washington University
1934 Ph.D., chemistry, University of Maryland

Professional Experience

1926-1927 Research Chemist, New York State Department of Health
1928-1929 Research Chemist, Picatinny Arsenal, War Department

National Bureau of Standards
1929-1935 Research Chemist
1935-1951 Chief, Organic Plastics Section
1951 Chief, Organic and Fibrous Materials Division
1951-1963 Chief, Division of Polymers
1964-1969 Consultant

1936- Technical Editor, Modern Plastics
1936-1960 Editorial Director and Consultant, Modern Plastics Encyclopedia

ASTM Committee D-20 on Plastics
1937- Member
1948-1952 Chairman
1941-1954 Chairman, Federal Specifications Plastics Technical Committee
International Standards Organization
Technical Committee 61 Plastics
1951- Member
1951-1958 Chairman
International Union of Pure and Applied Chemistry,
Section on Plastics and Polymers
1951-1967 Member
1959-1963 Vice-Chairman
1963-1967 Chairman
Macromolecular Division
1967-1975 Member
1976-1987 Observer

Honors

1952 Honor Award, Washington Section, American Institute of Chemists
1953 Exceptional Service Gold Medal, Department of Commerce
1954 Award of Merit, American Society for Testing and Materials
1965 Rosa Award, National Bureau of Standards
1973 Charter Member, Plastics Hall of Fame

1986 Award of Excellence, ASTM Committee D-20

ABSTRACT

Gordon M. Kline begins by discussing his work at the National Bureau of Standards during the 1930s, where he focussed on applications of organic resins to aviation. This is followed by a general discussion of his work as head of the Bureau's Organic Plastics Section and as Technical Editor of Modern Plastics. A lengthy section of the interview is devoted to Kline's experiences in Germany in 1945, investigating German plastics laboratories, plants and processes. The next major portion of the interview treats his experiences as an administrator of the Division of Polymers at the National Bureau of Standards and his role in setting national and international standards of testing for synthetic polymer products. The interview concludes with a brief discussion of his education at Colgate University and at the University of Maryland.

INTERVIEWER

Jeffrey L. Meikle holds an A.B. and an A.M. in American Civilization from Brown University and a Ph.D. in American Civilization from the University of Texas at Austin. He is Associate Professor of American Studies and Art History at the University of Texas at Austin. Dr. Meikle is the author of Twentieth Century Limited. Industrial Design in America, 1925-1939, published in 1979 and is currently writing a cultural history of plastics in the United States.

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INTERVIEWED BY: JEFFREY L. MEIKLE

LOCATION: LAKE WORTH, FLORIDA

DATE: 15 and 16 MAY 1987

MEIKLE: We are beginning our discussion with Dr. Kline's first years at the National Bureau of Standards during the 1930s. The first hour of our discussion, regarding his childhood, education, and early professional life, was inadvertently not recorded. We will return to that period at the end of the interview. Why don't we start where we left off, with your being furloughed soon after being hired as a chemist by the National Bureau of Standards.

KLINE: The stock market crash occurred in late 1929 and unemployment increased. Roosevelt was elected President in 1932 and took office in January of 1933. Shortly thereafter the budgets of the regular departments of the government were drastically cut to provide funds for the various Works Progress programs to take up the unemployed. The budget of the National Bureau of Standards, which was part of the Department of Commerce, was cut 50%. This meant, of course, that the staff would have to be cut by approximately 50%. After the first round of furloughs took place, the particular division that I was working in, the Division of Organic and Fibrous Materials, was still not fully in balance as far as budget was concerned, so it was necessary to furlough two additional scientists. Curiously enough the physicist who had transferred from Picatinny Arsenal and myself, who also had worked at Picatinny Arsenal, were the last two to be furloughed. That was July 1, 1933. At first I had some visions of working full-time at the University of Maryland to do my thesis work for a Ph.D. degree, but I had a wife and child at that time, and quickly realized that there was no way that I was going to be able to be unemployed. At that time the National Bureau of Standards was operating with approximately 50% of funds transferred from other government agencies to do work in fields of interest to those agencies. One of those agencies was the Navy Department, as well as the other military departments. I had been working on airplane dopes for the Navy previous to this period. The first thing I did then was to go down to what is now the Navy Bureau of Aeronautics and tell them that I had some reason to believe that the crashes of the dirigibles Macon and Akron were due to the absorption of water by the coating on the fabric which contained the helium within the dirigible.

MEIKLE: This was a theory that you had come up with on your own?

KLINE: Yes. They were using a gelatin-latex coating on the gas-cell fabric, and of course it was well known that gelatin was a very hygroscopic material. And they also knew that the crews were constantly having to drain water out of the dirigible

resulting from condensation of moisture in the gas-cell. We had been doing work previously for the Bureau of Aeronautics - I had a good contact with Ed Sullivan in the Bureau of Aeronautics - and he came up with $1,500 but it was necessary to have $3,000 for me to be able to go back to work on the payroll for the Bureau of Standards doing research.

MEIKLE: $3,000 being enough for one year?

KLINE: For one year, yes. These were incredibly minute sums compared with the millions and billions that we talk about today. That's all he could find in his budget to finance this work on the moisture problem. So I went over to the Lighter-Than-Air Division of the Navy and there was a Commander Fulton in charge of that particular work. We laid out the problem to him and in a few days he came up with $1,500. So there was the $3,000 which was transferred to the National Bureau of Standards. So on July 15th I was back on the payroll of the Bureau of Standards working on this problem of absorption of moisture by the gas-cell fabric coating. That work did indeed prove that the weight of the dirigible could double in just a few weeks due to the process of taking up moisture from the air and that in all likelihood that weight of water was sufficient to have caused the successive crashes of the Macon, the Akron and the Shenandoah.So this was published in the [National Bureau of Standards] Journal of Research, but it was too late to take care of that situation (1). Of course, they have since discontinued using gelatin-latex as the coating, and are using other synthetic elastomers for that purpose that are not hygroscopic. But it was an interesting piece of work, and from that time on the various departments of the government supplied the Bureau with the necessary funds for continuing work in the field of plastics and resins.

MEIKLE Your own research work remained heavily in aeronautics and plastics, didn't it?

KLINE: Yes. Of course, the two industries, the aircraft industry and the plastics industry, were really expanding together at that period in the 1930s. There was tremendous interest in possible applications of plastics in aircraft construction, so the National Advisory Committee for Aeronautics, as it was called then, NASA today, sponsored work at the Bureau of Standards. The Navy Bureau of Aeronautics also was very active in supporting work. The first major project was on the problem of airplane dopes for the fabric covering of airplanes. It is incredible today to recall that the fighter planes which were used on the aircraft carriers during World War II were all propeller-operated, fabric-covered airplanes. Before the War they were using cellulose nitrate, of all things, to get the necessary tautness in the fabric, and they were having difficulty with sparks from the stacks coming down on the fabric cover and then setting fire to the airplanes. So we were asked to develop a fire-resistant covering. We cooperated with the Naval aircraft factory in Philadelphia in doing that work. First of all we

tried using cellulose acetate in place of cellulose nitrate and didn't quite get the necessary tautness that was required for the operation of the plane. Cellulose acetate butyrate came into the picture from Tennessee Eastman Co. about that time. We were able to work out a plasticizer system with the cellulose acetate butyrate that did indeed provide a satisfactory doped fabric. This was in the 1930s. When World War II came in December of 1941, all the fabric coverings on the planes on the aircraft carriers were doped with cellulose acetate butyrate and performed very satisfactorily. That was one area in the aircraft field in which we worked.

Another very important field was the area of glazing - transparent glazing - for the aircraft in the windshields and all the other windows. They had been using, of course, glass, which was a hazard and heavy, twice the weight of most plastics. It was about this period that polymethyl methacrylate came into commercial production. It was half the weight of glass and was soon being used as a replacement for glass in the airplanes. But there was a difficulty which arose from the fact that this material would craze - that is, little cracks would form in the material and would interfere with the vision and, of course, also weaken the plastic. We found in the course of doing tests on PMMA, as the polymethyl methacrylate was called, that the crazing could be eliminated by stretching the cast sheet. We also found, as an added dividend, that by multiaxially stretching the acrylic sheet, that this brittle material became very tough. Whereas the ordinary unstretched PMMA would shatter if a bullet hit it, the new material - the stretched material - would not shatter. A hole would form in the sheet, but otherwise the windshield would remain intact.

MEIKLE: Is that, in effect, similar to the cold drawing of nylon?

KLINE: It is physically the same principle. But cold drawing is a uniaxial process, whereas multiaxial stretching provides two-dimensional toughness. For the acrylic sheet it was necessary to have the strength in all directions. That work was described in full in an NACA technical report that was published giving the whole history of the work on transparent plastics at the Bureau of Standards (2). Today the multiaxial stretched acrylic is used on all commercial aircraft as well as military aircraft. Other transparent plastics have come into the picture, but the principle remains the same - that the stretching provides the toughness.

MEIKLE: Was that an empirical development or did it begin as a theoretical one?

KLINE: It was empirical. As I say, we were first trying to find out why the material crazed and how to prevent it, and we discovered that when we stretched it in one direction that the tendency to craze was reduced, but also the strength of the material was increased. So the next step, of course, was to make that uniaxial increase in toughness, multiaxial. We at the

Bureau of Standards at that time were never concerned with taking out patents. Actually the patents for the multiaxial stretching were taken out by a firm in California, Swedlow, who were in the business of making windshields for aircraft.

MEIKLE: Is that Dave Swedlow?

KLINE: Yes. Do you know Dave Swedlow?

MEIKLE: No. But I know of him.

KLINE: They were in the business of doing this and, as I say, I don't have a patent to my name. The government became more conscious later of the necessity of protecting the results of their new developments. Now, everything that NASA sponsors is always patented, and the same is true of the Bureau of Standards. Precautions are taken to obtain the basic patents on new developments. But at that time it never entered our head to go to the Patent Office with such things.

MEIKLE: Was multiaxial stretching devised right before or during World War II?

KLINE: That was before. This was in the late or middle thirties.

MEIKLE: Because the one publication I've seen on that is from the early or mid 1950s.

KLINE: That was the NACA final report (Report No. 1290, 1956) summarizing all the work (3). But there were individual reports that were published much earlier. I have the record of publications over here in my file. Our first publication on transparent plastics for aircraft appeared in Modern Plastics in January 1936 (4). There was one other aircraft application that we were active in and that was the use of resin-bonded plywood and the development of suitable adhesives to give a durable, weather-resistant material for use in aircraft construction. The British, of course, were also very active in that field, and you remember the Spruce Goose that Howard Hughes built. I was invited out to California to the first demonstration of the use of plywood in the construction of such a large airplane, but there were many smaller planes that were built. Of course, the British built a very famous fighter bomber of resin-bonded plywood when they were running short of aluminum.

MEIKLE: The Mosquito?

KLINE: Yes. I think you're right. The Mosquito and the Spitfire came about the same time. But I think you're right. The Mosquito was the plywood plane, but they did find that resin-bonded plywood was a much superior material for the propellers on the Spitfire than aluminum. During the War, the British used resin-bonded propellers on the famous Spitfire. We did a great deal of work and had quite a number of publications

relating to the use of such plastic materials or synthetic resins, in broader terms, in those airplane applications (5). Eugene Vidal, who became Secretary of Commerce around that period, also was interested in the so-called plastic airplane and he came to the Bureau of Standards on many occasions to consult with us on what we were doing in the field.

MEIKLE: Why do you say "the so-called" plastic airplane?

KLINE: Well, because the wood actually was providing the basic strength for the fuselage and the resin was a binding agent and it was really acting as an adhesive rather than a plastic. Today they speak of composites which are glass-fibre reinforced or carbon-fibre reinforced or nylon-fibre reinforced. Of course in those composites, the resin is the matrix holding the strength elements in place. So to my way of thinking, the use of the term "plastic airplane" back in those days was a misnomer, but it caught public attention and obviously the newspapers and the radio always spoke of the "plastic airplane." There was no television at that time so it didn't get on the air in picture form.

MEIKLE: But there was a great deal of publicity about plastic airplanes and plastic houses. I also remember seeing a photograph, I think it was in Modern Plastics, of a plywood car that someone had built - very sleek and stylish. Around 1942.

KLINE: Right.

MEIKLE: What did you think of all that publicity at the time? People were talking about a plastics age.

KLINE: You must remember - this is a subject we haven't discussed yet - I had become technical editor of Modern Plastics in June of 1936. And this was after the Plastics Section was organized in January of 1936.

MEIKLE: At the Bureau?

KLINE At the Bureau of Standards, yes. This caught the eye of the publisher of Modern Plastics and they asked me if I was interested in becoming technical editor of Modern Plastics, which was the only magazine in the field in the United States at that time. In fact, there were only two magazines - one in Germany called Kunststoffe, which means "artificial material," and Modern Plastics in the United States. There was no competition, no apparent conflict of interest, so the Director of the Bureau gave me permission to serve as the technical editor of Modern Plastics. It was very fortunate for me because it established a direct contact with the plastics industry in a way that was not possible when I was merely a chemist at the National Bureau of Standards. It gave me the opportunity to meet all of the executives and the directors of research, chief chemists and so forth, in the various firms of which there were relatively few, unlike today. So I met a good many persons who I would not

normally have met as a government employee.

MEIKLE: Did these firms include materials suppliers as well as fabricators and molders - everyone?

KLINE: Yes. They included the broad population. The Society of the Plastics Industry had been organized about that same time

[END OF TAPE 1, SIDE 1]

KLINE: ...so the association with the magazine was a very fortunate happenstance.

MEIKLE: And the Society of the Plastics Industry was organized then as well.

KLINE: Yes, that was organized around the same time - in 1937. They are celebrating their 50th anniversary this year. I met, for example, Leo Baekeland, who discovered Bakelite. Lawrence Redman, who was the head of one of the chief competitors of Bakelite which, later amalgamated into the Bakelite Corp., and I met Dr. Fritz Pollak who discovered urea-formaldehyde resin. He came to visit us in Bethesda. So I was able to have these contacts with various and sundry individuals.

MEIKLE: Was Baekeland still active in his company when you met him or had he pretty much retired from it?

KLINE: He was nominally head of the company but his son was really, I'm sure, doing the work. Leo Baekeland was a very interesting individual. I had many contacts with him, particularly at the Gordon Research Conferences which were being held just outside of Baltimore back in the 1930s. He was very talkative in those days and interested in exploring what was going on. We went to Miami on vacation in December 1940 and he invited us to his estate in Coral Gables which he had purchased from William Jennings Bryan. He had a yacht there which he had called the Ion. He had palm trees all over this estate - all kinds of trees, not just palm trees - which had been sent by the various affiliates of the Bakelite Corporation from all over the world. He took us around and showed us his various trees and had comments on some of them. Charlie Parsons was the secretary of the American Chemical Society, or chief executive, although I think his title was Secretary at that time. And Baekeland said that this was the Charlie Parsons tree because it was a rather large rotund structure, like Charlie Parsons.

At the Gordon Research Conferences we used to go swimming in the afternoon, and if you happened to run into Dr. Baekeland when you were on your way to a swim, you had to, I wouldn't say resign yourself, but maybe have the opportunity of talking with him for one hour before you got involved with your swim. But of course, it was a privilege to hear what he had to say and to have known him. Roy Kienle was another individual; he was associated with General Electric Company and was very active in the development of alkyd resins. He attended the Gordon Research Conferences.

Also Dr. Emmett Carver from Eastman Kodak. There were a good many. And Tom Midgley who discovered - or was the inventor - I don't know whether you call it inventing a chemical compound, but anyway he had the patent on tetraethyl lead. Individuals of that type were at these polymer conferences, the Gordon Research Conferences. Dr. Neil Gordon was a professor at the Johns Hopkins University. Although he never actually did any research in the field of polymers, he was active organizer and interested in developments in chemistry.

MEIKLE: How did you see your role after you had become Technical Editor of Modern Plastics and you had met people in the industry? You were working at the Bureau of Standards as head of the Plastics Section and then of the Polymers Division. How did you conceive of your position between governmental agencies, such as the military, and private companies?

KLINE: I always was very careful to avoid any conflict of interest. Subsequently there were other magazines that came into the field of plastics and I always tried to give them equal access to any articles that they might be interested in publishing in their journals as well as in Modern Plastics. All of the work for Modern Plastics was done outside of office hours. None of the work was done at the Bureau. I did that after hours or weekends which turned out to be quite a burden at that time - moonlighting. But of course one advantage was that it practically doubled my salary and there were always questions as to whether I would have remained at the Bureau of Standards if I hadn't had that second source of income. Before the War started, the publisher of Modern Plastics, Charles Breskin, had asked me, in fact, talked me into going to Modern Plastics as the editor of the magazine. I went up to the Director, Dr. Lyman Briggs, and I told him about this offer. Dr. Briggs was a fine gentleman and very even tempered and very deliberate in what he said. But he became very red in the face and pounded the table and said, "Who is this that is robbing the National Bureau of Standards of its plastics expert in this time of great need?" This, of course, was in 1941. And I said, "Dr. Briggs, you gave me permission to be Technical Editor of Modern Plastics and if you want me to remain at the Bureau of Standards (the War hadn't started yet for us) I will remain as long as you feel that I am needed here." So that is the way it was left. During the War I made many contacts abroad. I had some very unusual experiences during the surveys that were made in Germany just toward the end of the War, but I had reached the conclusion by the time the War ended that I was not interested in becoming the editor of Modern Plastics. I was still willing to handle the technical side, but I realized that science and the editorship of Modern Plastics were in conflict. And Mr. Charles Breskin himself, I think, realized it because he never asked me at the end of the War to fulfill that commitment. After the War I was also asked by the firm, U.S. Rayon, to be their Director of Research at what was the magnificent sum of $25,000 a year, when my government salary was still on the order of half of that. I considered it but not on the basis of salary because the income from the technical

editorship brought my total income to approximately the same figure. So I declined and thanked them for considering me as a candidate. But what happened was that Dr. Herman Bruson of Rohm and Haas, their outstanding chemist, who was very famous for his various inventions and patents for basic resins, became the Director of Research for U.S. Rayon with a $50,000-a-year, five-year contract, but at the end of the five years he was no longer Director. So there is always a hazard in accepting offers of that type.

A little later - in Harry Truman's time as President -there was some Congressional action (or some departmental regulation I think would be a better word for it) regarding conflict of interest, and Truman had issued a statement for government employees regarding avoiding conflict of interest. So I wrote a two-page memorandum to the Director of the Bureau stating that I had been technical editor of Modern Plastics since 1936 and in view of the fact that these new regulations had been issued, I wanted to again have the department - not just the Bureau of Standards but the Department of Commerce - approve my continuation as technical editor of Modern Plastics outside of regular hours. And I received the approval, fortunately, so I think the record was clear that the two of them were compatible. In fact, Julius Klein, who was Secretary of Commerce at the time that I first became technical editor, subsequently visited me at the National Bureau of Standards on business for one of his clients, and said that his policy had always been that there should be as much contact with industry from the Department of Commerce as possible, because that, after all, was appropriate for a department so-named. The Department of Commerce was there to work with industry.

MEIKLE: Before the War, did the Bureau do work for industry? Could companies use the Bureau or your section as a kind of research organization or did you take up problems that seemed to be of general interest that they had brought to you?

KLINE: In the plastics field, better than 50% of our funds came from other government agencies. So as long as I was at the Bureau of Standards, both in charge of plastics and subsequently in charge of the Division, it was essentially based on at least 50% of the budget coming from other government agencies. Not from industry. The Bureau of Standards did have a beginning program for research associates from industry, but there was never any work from the plastics industry while I was there. No. Our work was primarily divided into two parts. The work supported by the Bureau was on fundamental research and we became known, by the time I retired, as the leading plastics laboratory in the country, both for the research work we were doing and for the papers and scientific reports we were publishing. Of course, later on the university laboratories in plastics sprang up - Princeton had a laboratory and of course Brooklyn Polytechnic under Herman Mark - but at that time in the early 1960s, our advisory committee did present us with a compliment that we were certainly one of the leading laboratories.

MEIKLE: I would like to ask first what you were able to accomplish because of your dual positions as head of the Organic Plastics Section at the National Bureau of Standards and as Technical Editor of Modern Plastics. Was that a unique kind of situation? What were you able to do in those two positions?

KLINE: It was unique in the sense that at that time there was only one magazine covering the plastics field in the United States and it gave me the opportunity to disseminate information to industry in a broader fashion, through publishing the results of our research on plastics to the industry as a whole. The situation back in the middle 1930s was such that when I became Technical Editor in June of 1936, there were very few articles sent directly to Modern Plastics from industry sources. Most of the firms thought that their research was proprietary information and they were not interested in publishing results of their research that might benefit other firms. In one of the early issues after my taking on the editorship, I exhorted the executives of the plastics industry to recognize that they would benefit by having such information made available in the literature; they would learn as much about what other people were doing that would be of sufficient benefit to warrant them doing the same. Otherwise there would be duplication and triplication of a great deal of research. But it was several years before the industry came to this realization. There was a publication by the American Chemical Society at about that same time that expressed the same viewpoint. In a survey of plastics up to that point, they said that most of the information regarding plastics was in the patent literature rather than in the scientific or trade literature. We found that to be very true. Of course, that meant that from 1936 until about 1939 or 1940, as Technical Editor I had the responsibility for either soliciting articles from the commercial laboratories, or preparing the results of our research at the National Bureau of Standards for publication in Modern Plastics. That was advantageous from the perspective of getting out information on what the Bureau of Standards was doing in plastics research, but the one-way street was not too beneficial to the industry as a whole. I remember when I was appointed in January 1936 to be the first Chief of the Plastics Section, I made a trip around to the laboratories of the various major firms at that time. This meant visiting roughly only nine or ten laboratories. At one of those laboratories I was met by an administrative executive of the firm, and I told him that there had been a Plastics Section formed at the Bureau of Standards and we were interested in telling their chemists what we were doing, and we would be very much interested in having them publicize some of their work in the field of plastics in the magazine. To my surprise, the administrative executive said, "We do not allow any visitors to talk to our chief chemist." So I never did get to see or talk to a chemist there. I talked to only those individuals who were strictly on the business side.

MEIKLE: Which company was this?

KLINE: I'm trying to remember. It's no longer in existence anymore. Let me say that it was not one of the major chemical companies. Some firms at that time, however were even reluctant to send their leading chemists to scientific meetings for fear that they would tell something to a competitor over a drink that the firm considered to be proprietary information.

MEIKLE: Was that one of the reasons why some firms were reluctant to join in the Society of the Plastics Industry when it was formed?

KLINE: The S.P.I. was organized in 1937. It was purely a social organization at that point. Because of my position as Technical Editor of Modern Plastics in those early days, I was invited to attend those meetings as a representative of Modern Plastics. But there was very little discussion at those early meetings of what was going on in the way of technical research in the laboratory. It was mostly on the business side and the social side. About that same time the American Chemical Society organized a plastics group and the ASTM organized a plastics committee and these two organizations brought the chemists and the mechanical engineers from these firms together in preparation of standard test methods of test and in the discussion of research in polymer chemistry and on the properties of plastics. After these organizations came into existence, the exchange of information began to flow in a much better and more beneficial way.

MEIKLE: You played a role in founding the ASTM plastics committee, didn't you?

KLINE: Yes. I have the record of the formation of the ASTM committee. There was an organization meeting held at the Waldorf Astoria. We started out very high class. That was on July 1, 1937. Most of the work on synthetic resins and plastics had been done by ASTM Committee D-9 on Electrical Insulating Materials. But by 1937, of course, the materials of the industry were going into much broader industrial fields and it was felt that the time had come for test methods to be standardized. That first meeting resulted in the recommendation that the committee - ASTM D-20 Committee on Plastics - be organized and that five subcommittees be organized covering strength properties, hardness properties, thermal properties, optical properties and permanence properties. They appointed the chairmen for these five sub-committees. Because of our work at the Bureau of Standards on the permanence, on weathering of plastics and resistance to chemicals and so forth, they asked me to serve as chairman of the permanence properties subcommittee. The second meeting took place on October 27, 1937, at the Hotel New Yorker in New York. It was at that meeting that the real work on the preparation of standard testing methods started. I have a copy of the minutes of that meeting as well as the organization meeting. The minutes of the October 27 meeting ran to 14 pages, so quite a bit of work was accomplished in a one day meeting. Mr. Warren Emley, who was Chief of the Division of Organic and Fibrous Materials at the Bureau of

Standards, served as Chairman of D-20 at that time.

Of course ASTM proceeded with deliberation to arrive at a consensus...

[END OF TAPE, SIDE 2]

KLINE: ...taking into account the various methods that were being used in the industry. Actually it was several years before a consensus on the testing procedures was reached and the test methods published. In the meantime, World War II had started in 1939 and the military became more and more conscious of the fact that plastics were going to be a major factor in providing materiel for them. They became somewhat concerned that they had to have standardized tests methods so that the information that they would be getting from the various laboratories would be comparable. This concerned particularly the Air Corps, and J.B. Johnson, who was head of the Materials Group at Wright Field, called a conference in Dayton, Ohio to try to get agreement by industry representatives on methods of tests for the fundamental mechanical properties of plastics. It was a very interesting meeting that was held July 8, 1941. For some reason I served as the fellow that was responsible for writing up what had been agreed upon, and my notes say that this draft was prepared by G. M. Kline, National Bureau of Standards, August 22, 1941. It summarized the testing conditions and the testing procedures for some 19 items, the first having to do with conditions of testing and the speed of testing and then there were 17 other specific types of tests that were of interest particularly to the Air Corps, with respect to the use of plastics. These methods, of course, in large part, were in agreement with what ASTM Committee D-20 was talking about in their sessions, but as I said previously, the work at the ASTM Committee was proceeding slowly and Wright Field felt that it could no longer wait for those methods to be agreed upon and published. I remember that Conference at Wright Field very well because although we didn't get into the War until about five months later, this meeting was very important in preparing the way for information that the military services could use with some degree of reliability for the many applications for which plastics were used during the War period.

MEIKLE: Did the Organic Plastics Section expand during the War?

KLINE: Yes. When the Section was organized in January of 1936, it consisted of Ben Axilrod and myself. In other words, one Chief and one Indian. When the War ended in 1945, there were 40 of us in the Plastics Section and we were doing all of the major testing for all of the military services. The military services sent representatives out to the Bureau to observe the testing of items that had been prepared for them by the various plastics molders. As I say, it required expansion to approximately 40 personnel, but qualified personnel were difficult to find during the War period because - every trained technical person could readily find employment. I had very good connections with the University of Maryland because I had taken my Ph.D. there. I knew the Dean of

Women at the University of Maryland, and many of the girls that had some technical training in the course of their studies wound up in the Plastics Section at the National Bureau of Standards doing very capable work in handling these tests. The Plastics Section at the Bureau of Standards became known as Dr. Kline's Finishing School. But as I say, we were fortunate to have such an excellent source of qualified personnel available right next door.

MEIKLE: We talked a little bit already about your involvement in aeronautics during World War II. What about the trip to England you made, I think it was in 1942.

KLINE: Yes. Can I discuss the formation of the American Chemical Society activities in plastics before we go into that, because it occurred the same year? The ASTM Committee D-20 was formed in 1937 as I just said, and actually it was during the same period of 1936-1937 that the start of plastics activity in the American Chemical Society took place. We had a meeting of individuals who were interested in working on plastics at Chapel Hill on April 12, 1937. It was there that the agreement was reached that an Organic Plastics Section of the Division of Paint and Varnish Chemistry should be formed. I have in my files the signatures of a rather long list of individuals who signed a petition to form this Organic Plastics Section, and as I go down the list, all the leading firms in the plastics industry had their representatives sign. Here is Arthur Doolittle of Carbide & Carbon who was very well known for his work on plasticizers. Dr. Baekeland, who needs no introduction, also signed that petition. Roy Kienle; well, as I say, all of the major technical personnel. G.F. D'Alelio from General Electric. That name will be familiar to many people. And H.F. Meindl and Harry Dittmar from Du Pont. I have the complete list and the original signatures of those individuals. So this resulted, in 1937, in the Organic Plastics Section being organized as a section of the Division of Paint and Varnish Chemistry with myself as Chairman. In 1938 Herman Bruson, whom I mentioned before, was the Chairman. Then H.R. Dittmar of Du Pont in 1939 and W.I. Patnode of General Electric in 1940. That group became known finally as the Organic Plastics Group rather than Section, probably because of the confusion with local Sections. Ultimately, the group was incorporated into the Division of Paint, Varnish and Plastics Chemistry, which later became known as the Organic Coatings and Plastics Division with alternating chairmanships of the Division, first from the Protective Coating side and then from the Plastics side. Today it seems unbelievable that the title was not changed at the time of the first formation, but in 1937 the protective coatings industry was a much larger component than synthetic resins. It is one of those things that post facto it would have been much more logical to have a broader title. Well, ultimately the plastics industry finally outgrew the protective coatings side of the picture and plastics became represented by a separate division of the American Chemical Society. Then somewhat later the interest in the fundamentals of polymer chemistry became such that the pure side and the applied side separated into the two different divisions.

MEIKLE: When did that take place, roughly?

KLINE: Well, I'd have to look at the record.

MEIKLE: Would that be in the 1950s sometime? [1950: ed.]

KLINE: Yes. I think actually that occurred in the late fifties or early sixties. I think I have records of that, but I just don't remember the exact date. Of course they have changed the name of the applied side again within the last couple of years. But those two divisions, the Polymer Chemistry Division, and the Applied Polymer Chemistry Division [now the Polymeric Materials Science and Engineering Division], are two of the largest divisions in the American Chemical Society, as they should be.

MEIKLE: Just to get away a little bit from the Wartime period. Was there a tension in the Bureau of Standards - we talked about this a little in the car at lunch - was there ever a tension between the pure and applied sides? Could you trace that out?

KLINE: A good deal of the work at the Bureau of Standards - at least 50% of the work, particularly in our area of organic materials, and also in the inorganic field, was on the more practical side. Fifty percent or more of the work actually was supported by funds transferred from other government agencies. The emphasis during the 1940s and up until the early 1950s was very much on the applied side. Then the laboratories in industry were expanding and the polymer laboratories in the universities were undertaking much more of the pure science research. The new Director of the Bureau, Dr. Edward Condon, came in at about that time, 1950, and he thought that the emphasis in the Bureau's approach should be less on the applied side and much more on fundamental chemistry and physics of materials.

The first duty I had after I became Chief of the Division in 1951 was to reorganize the work of the Division to stress the fundamental side, and the Division was renamed the Polymer Division. This was in the middle 1950s. The materials sections - textiles, paper, leather, rubber and plastics - were abolished and new sections were organized in terms of research on the fundamental properties of polymers. It was a logical transition because more and more of the applied side of research was being done either in the commercial laboratories themselves or in the universities with funds being supplied by industry on sponsored projects. It was a relatively easy transformation. The one thing troubling me at the time was that in some of the divisions the new division chiefs lost sight of the fact that, after all, we were the National Bureau of Standards and that we had the primary responsibility for developing, with the cooperation of industry, standards, particularly standards for property testing methods and also standards for materials and products, to a lesser degree.

MEIKLE: Performance standards.

KLINE: Performance standards, yes.

MEIKLE: The date I have here for the reorganization is 1951. Does that sound right?

KLINE: No. That was the year I was appointed Chief of the Division of Organic and Fibrous Materials, as it was known then. And I don't remember the exact year in which it became the Polymer Division, but the reorganization became my responsibility.
On the question of standards, I even had individuals who were in some of the other divisions tell me that their division chiefs did not encourage them to participate in the work of the ASTM, which of course was the primary private, non-governmental organization for preparing standards. And they were told that if they wanted to participate in the ASTM and the work of ASTM committees, it would be on their own time and at their own expense. This, I felt, was losing sight of the fact that we had equal responsibility for continuing at least for a reasonable proportion of our work in cooperating with ASTM in the development of standards and specifications. We had regular meetings of the Division Chiefs in which we were called upon to report on the activities of our division. I would have the various section chiefs report on the progress in studying the fundamental properties of plastics, - mechanical, optical, permanence, and so forth. In chemistry, Dr. Leo Wall and his colleagues were doing outstanding original research in the field of polymerization.
Then, in conclusion, I would take up the last half hour of the two-hour session, describing our work in the field of standards, both ASTM standards and International standards under the aegis of the International Standards Organization in Geneva. One of my more frank individuals, Dr. Donald McIntyre of the group working under Dr. Wall, told me that he could see my eyes light up when I got to that portion of the agenda and started to report on the work on standards. Dr. Allen Astin, who succeeded Dr. Condon as Director of the Bureau and inherited this change in emphasis, would smoke his pipe at these sessions where we as division chiefs told what we were doing. As I said in preparing a report on my career at the National Bureau of Standards, he never uttered a discouraging word during the half hour I talked about standards. I think, well I know, because he was really a personal friend, that he certainly approved of the continued activity in those standards activities, both national and international.

MEIKLE: I've been reading through a lot of articles in Modern Plastics from that period, and the industry itself was very concerned - at least the journal was very concerned - about the lack of standardization in the industry. There was a big hue and cry about sub-standard materials, about materials being used for purposes other than what they were intended, and about labelling problems and informing consumers of the nature of plastics.

KLINE: Yes. In the early stages of the development and use of plastics there were, as might be expected, some misapplications of

the materials. Of course, there was the question of flammability and of the possible hazard of volatile materials and combustion products being formed from plastic materials. We did a great deal in the Plastics Section, working on the mechanisms of degradation of plastics, both through natural weathering and aging, as well as under combustion conditions. We published many reports in this area. I remember one thing that stood out. We made a particular study of the combustion of polyvinyl chloride and proved very conclusively that in the process of combustion of PVC, there was never any vinyl chloride formed. This was very important because vinyl chloride had been proved to be a carcinogenic material, and in the chemical plants it was very necessary to keep to a very minimum the amount of vinyl chloride to which the workers would be exposed. So it was equally important to have positive evidence that in a fire, there would be no vinyl chloride formed as the result of the combustion.

MEIKLE: When did these studies take place?

KLINE: That work on PVC was in the late 1950s. A 1961 bibliography supplement to NBS Circular 494, Plastics Research and Technology at NBS (1950) lists 65 papers published on Permanence Properties during the years 1938 to 1961.

MEIKLE: Let's talk about your work during World War II now. I'm interested, especially, in your visit to England and your three-month tour of Germany. I'm curious as to exactly what kinds of things you did in England. I guess that was in 1942.

KLINE: Yes. At the invitation of the British Ministry of Supply and the Admiralty. They requested that the War Department send over to the U.K. a representative to discuss the work that was being done in the United States on plastic materials and also to be shown the activities that were being done in the United Kingdom on the use of plastics in War. Of course, they entered the War in 1939. We were two years behind them and they had already - by August of 1942 - done very substantial work in converting plastics into materiel that was needed for the Armed Services. I spent approximately a month there. It was my first trip in an airplane. I went over in one of the flying boats of the American Export Line. We took off on August 21st from the East River in New York.

[END OF TAPE, SIDE 3]

KLINE: I was surrounded by a group of very young men in civilian clothing. I learned later that except for myself and three or four other individuals who were on the flight, they were all Air Corps pilots being ferried over to Britain for training under wartime conditions. We landed at Gander Lake, on the water of course, for refueling and then we took off again. Gander Lake is not all that big. They circled and circled and circled to stir up the water because they have to have wave motion to lose contact and get off the water. When they started to run for take-off from one side of the lake, it looked to me like they were sure headed

for that forest on the other side of the lake before they got away from that adhesion to the water. But anyway, we got off the Gander Lake, but halfway across the Atlantic Ocean my seatmate said, "I see that we've lost one of our inboard engines." I said, "How do you know that?" "Well. You see the red glow...(these were propeller planes, of course)...You see the red glow behind the outboard motor and there is no red glow on the inboard motor."

MEIKLE: There were four motors?

KLINE: Yes. A four-motor plane. So I said "Yes. I see that." So that slowed us down. Then when we got in sight of the Irish Coast about dawn I heard this terrific whine and the other inboard engine went out. That slowed us down some more. We ultimately got to Shannon and landed on the Shannon River and we were taken by boat from the flying boat over to the shore. The pilot's colleagues were over there and they asked him what kind of trip he had and he said, "Terrible." As far as I was concerned, it was routine because it was my first trip. Anyway, we got on another flight there in Shannon. Flew over to Bristol and took the train from there to London. I saw in the newspaper the next day, or two days later, that there had been a bombing raid on Bristol the day after we went through there. A bus had been struck and people killed. But that was true throughout most of the entire month I spent in England and Scotland, that whatever city I was in, there was no raid. I went to Cambridge to see Sir Eric Rideal in Cambridge University - a well known physical chemist. And he said, "You should have been here last night. We had a real fireworks display up in the sky. We watched it from under our dining room table out through the glass doors. And you missed it." Well, it was just as well, but I was able to take back when I returned on September 19 from that month's trip a lot of very useful information as well as samples of plastic products they were making in the United Kingdom.

MEIKLE: Was their work much advanced beyond American work?

KLINE: Only to the extent that they obviously had had to start much earlier. They got into the War in 1939 and they were much more geared up to production than we were. When we started making products in large volume, we had to go through the early stages of building the plants to make the resins just as we did with synthetic rubber. There was the early emphasis on the production of synthetic rubber because our supply of natural rubber was cut off. Those synthetic rubber plants had priority over some of the synthetic resin plants. But the main emphasis at that time in the United States was in the construction of these new chemical plants to make the raw materials. But the testing work really began in 1940. Testing of products began on a larger scale in 1941 and continued through 1945.

MEIKLE: Then in essence, the plastics industry in Britain had developed really in much the same fashion as that of the United States. They developed in parallel fashion through the exchange of patents and technologies.

KLINE: Right. Of course, at that time there were relatively few types of plastics compared with what we have today. The cellulosic plastics and the phenolic plastics were available. Polystyrene to some extent. The alkyd resins were available. But the important polyolefin products were wartime newcomers. ICI in Britain had begun the pre-war development of polyethylene. Actually Carbide & Carbon Chemicals Corporation, later part of Union Carbide, had also started, before the war, the development of polyethylene in this country for insulation of electrical cable that was very much needed for radar work during the War period. It was curious that the technology for production of polyethylene in the United States was developed by the Liquid Nitrogen Division of what is today Union Carbide, and that information was transmitted to their German IG Farben affiliate from the United States. We later learned in Germany that the same technology was used to produce polyethylene that the Liquid Nitrogen people had developed in the United States. ICI had developed their own technology separately and the Du Pont Company operated on that technology.

MEIKLE: Du Pont had a cross-licensing agreement. Sent some people over.

KLINE: I believe so. There is an interesting anecdote on that. The government had subsidized the production of polyethylene in a plant built by Union Carbide in Charleston, W. Va., and in a Du Pont plant also in the Charleston area. I had been through the IG plant in Düsseldorf during my 1945 tour of the German plastics industry or what there was left of it in early 1945, and I particularly had seen the plant in Gendorf, Germany which was down near the Austrian border. It had not been bombed. I had seen the whole process and I remember John Crawford of ICI, who was one of their leading polymer chemists, after going through the polyethylene plant at Gendorf, said, "If that process works, it's a marvel. It's beautiful." I also went through a plant in Germany where they were using the emulsion process that ICI had developed and I found that that process had all sorts of problems with it and it was relatively inefficient. So the government was going to sell the two plants in West Virginia and they asked me to go down and make a technical report on the two processes from the standpoint of what the plants might be worth. Ordinarily that would be no job for a bench chemist. It would be a chemical engineer that would be more involved in that sort of thing. But I had had this experience of going through the plants in Germany with an expert, John Crawford. I went to the Du Pont plant first. A good friend of mine from my ASTM days was in charge of the plant and he took me through and told me very frankly what all their problems were with the production of polyethylene at that plant. Then I went over to the Carbide plant, which worked on the high-pressure process of making polyethylene.

MEIKLE: The same process that had been used by IG Farben at Gendorf.

KLINE: Yes. It came from the Liquid Nitrogen Division. An administrative individual from the business side of the picture acted as my guide. He took me through the initial part of the process where they were pumping the ethylene into the high pressure unit. I asked him some questions regarding the equipment and the process and he got a frown on his face. He said, "Are you an auditor?" "No. I'm from the National Bureau of Standards." He said, "Are you the Kline that wrote the reports on polyethylene production in Germany?" "Yes." "I have to call my New York office before I show you anything more." Of course, the plants were owned by the Navy and I could have demanded to see anything I wanted to see there, but anyway, he came back with the directive from the fellow in New York that he could show me the plant in which the finished product came out after it was polymerized, but they would not take me into the high-pressure polymerization unit. So we went over to the finished product building and there coming out of the supply line were pellets of polyethylene exactly as they came out in the plant that was operating in Gendorf, so I didn't need to see the high-pressure unit. I knew that it was the same process that I had seen in Germany and that not only was it working to planned capacity, but actually was capable of exceeding the original planned capacity. In my report to the Navy Department, I said essentially that the Du Pont plant could be sold for scrap but the Carbide plant is a very efficient and a very productive plant and should be appraised accordingly. And that is what happened. The government got a reasonable settlement for the money they had put into this Carbide plant. Within a year after that report, the Du Pont plant was torn down. It was scrapped.

MEIKLE: When was this report made?

KLINE: Shortly after I returned to the United States in late 1945. I returned from Germany in July and this was somewhat later that year.

MEIKLE: The government was trying to decide what to do with its plants after the end of the War.

KLINE: Yes. With respect to the German plastics reports, I went over at the request of the Ordnance Department. The various branches of the military were sending representatives. I don't know why it wasn't handled at a higher level but usually, each unit of the Army or the Navy would send civilian experts to represent them and to report back to them. I happened to be chosen by the Ordnance Department to go to Germany to appraise what the developments were in synthetic resin technology during the period 1939 to 1944 in Germany, which meant primarily the IG plants, although I visited other plants as well. I made three separate trips back and forth. I would visit certain plants. The first trip I made alone as I was among the first of the technical investigators to arrive on the scene.

MEIKLE: So you would come back to the United States?

KLINE: No. To London. The headquarters were in London. We were in Army uniform at the time. I had a card which said if I were captured by the enemy I would automatically be a colonel in the United States Army. I also had a civilian identification, of course. I went over by myself on the first trip and I had a jeep and a driver assigned to me. I visited a number of plants and got some experience in operating as a civilian in an essentially military establishment. You quickly learn that you have to beg, borrow or confiscate whatever you need. We had no real authority except that we were in military uniforms and had Eisenhower caps. We were called "capon colonels" - because we had the appearance but we didn't have the authority.

I remember one time on one of these trips, Sid [Sidney D.] Kirkpatrick, who was the editor of Chemical Engineering (at that time I believe it was Chemical and Metallurgical Engineering) had the simulated rank of general because that was based on your income and his income ranked him in the category of general. He was travelling for the Chemical Warfare Service. Wherever we went we had to get a billet assigned from the Army for where we would spend the night. This was a process of standing in line and waiting for the sergeant to find a livable billet for you. These lines were fairly long. Sid came in and looked at this line. He was a little impatient. I knew Sid very well and we were great friends. He was an energetic individual and knew his way around. He walked up to the sergeant's desk and everybody in the room could hear him when he said, "I'm General Kirkpatrick and I would like a billet for the night." The sergeant looked up at him and said, "As far as I am concerned, you are lower than the lowliest private. Go to the end of the line." Of course, Sid didn't make any big deal about it. He wasn't offended but I think he was a little embarrassed by that remark. But that was the sort of thing one encountered. I'm merely citing that. I remember another instance. John Crawford and I were seeking a billet. We were traveling in a group of six and he was in charge because we were in a British zone. There were four Britishers, a Canadian and myself in the group.

MEIKLE: What were their backgrounds?

KLINE: They were from the plastics industry in Britain and the Canadian was from the plastics industry in Canada.

MEIKLE: With commercial firms?

KLINE: That's a good point. All of them worked for commercial firms. John and I went to get a billet and we approached the billeting headquarters and the fellow - the sentry standing guard there - sprang to attention. Crawford had all the red decorations that were on a colonel's uniform in Britain and I had the Eisenhower cap and he sprang to attention. But he could see that I didn't have the eagle on my shoulders. Probably Crawford didn't have any of the insignia for the British Army either. That's one thing we didn't have - they didn't let us have any bars or any eagles or stars. He sprang to attention, saluted, and he said,

"Pardon me, Sir. But will you tell me, Sir. What are you, Sir?" So we explained it to him. But anyway we went on in and the British sergeant in charge said to me, " Do you know what your Army did to us? They pulled out of here yesterday and they took every stick of furniture, every bed, every chair, every table with them. And we are having a terrible time trying to find billets for our own people, let alone technical investigators." But after that he, of course, did find a room with a bed for Crawford and one of his cohorts, but the rest of us had to sleep on cots. We had three cots but we needed one more. So we went back to the headquarters - this was late in the evening - and there was just a Welsh guard there on duty. And we told him that we needed a cot. He didn't know what the word cot meant. We finally were able to explain that it was a folding bed. Oh, he said, "There's one over there in that closet but it belongs to the American Army." "That's just what we are looking for!" So we picked up the American Army's cot. We were driving around for another two weeks and we kept all four cots for the whole time. We took them everywhere we went so we wouldn't be shy of beds to sleep on for the rest of the trip. I cite these things to show you some of the problems you ran into.

One more! On my first trip the Ordinance Department gave me orders to go to Hamburg to investigate the Bakelite plant. They made arrangements for me to fly to Hanover and supplied me with a jeep and an Irish driver there. So we got onto the Autobahn that runs between Hanover and Hamburg. Celle, halfway between Hamburg and Hanover, was the British headquarters. Actually we went around Celle on the Autobahn. Here I was with the Eisenhower cap on my head and they knew that only officers with the rank of colonel or higher were permitted to wear the Eisenhower cap in the War zone. The War was still going on. This was May of 1945. There were sentries along the road after we passed Celle and they would spring to attention, salute, and wave us on. Finally we came to one sentry who sprang to attention, saluted and said, "Pardon me, Sir. Would you tell me, Sir, where you are going, Sir?" "I'm going to Hamburg." He said, "We haven't taken Hamburg yet." That was the closest I ever came to using my military pass which read "Valid only if captured by the enemy" and assigned me the rank of "Colonel in the U.S. Army." I think the next sentry would have asked me in German, "Achtung..."

MEIKLE: But you wore uniforms because if you were somehow taken prisoner, you would have been considered spies?

KLINE: If you were in civilian clothes. Yes. In a War Zone you had to be under the aegis of the military, you had to have the uniform. As I say, the only difference was the actual insignia on the shoulder.

MEIKLE: You mentioned that each military branch sent in its own expert. There must have been a lot of duplication. I'm aware of the book German Plastics Practice (6).

KLINE: Yes. By John DeBell, William Goggin and Walter Gloor,

who were sent over by the Quartermaster Corps.

MEIKLE And they were doing a similar kind of tour, right?

KLINE: Actually, I travelled with De Bell, Goggin and Gloor on one of our assignments. The technical investigators on plastics all knew each other and we'd meet in London.

[END OF TAPE, SIDE 4]

KLINE: The operations were all centered in one building in London's Grosvenor Square. Naturally there was a good deal of contact. It was an efficient operation for those of us that were traveling for the military organizations. There was a separate committee - I think known as CIOS, I've forgotten exactly what it stands for - the CI is for Civilian Intelligence. It was organized, I believe, under the auspices of the Department of Commerce or the National Academy of Science. I've forgotten which. Those individuals were travelling under civilian auspices and they were having a terrible time getting any place because the military was not particularly interested in what they were doing. They used to come over when I was in London and ask me, "For heaven's sake, how could we get some access to these things." The general feeling was that the acronym for that group was CIOS but it was pronounced "chaos." Actually I did a good deal of work for the Quartermaster Corps in the National Bureau of Standards and very little for Ordnance, but it just so happened that Ordnance was the first one to invite me to represent them. There was no inefficiency among these individuals that were assigned from the military agencies.

Dr. Francis Curtis, who was Vice President of Monsanto Company, was over there and we became very good friends. I had never met him before, but we were compatible and enjoyed talking with each other. We shared a room in Heidelberg. He had the rank of general because he was Vice President of Monsanto. I drove in a jeep with him from Munich to Heidelberg and he said, "Why don't you come with me to the quarters that I've been assigned to. I'm sure they have two beds and you might as well have the other bed." Of course, I agreed and it turned out that this was the Schloss Hotel in Heidelberg. Although this was still during wartime the waiters serving in the dining room at the Schloss Hotel were in black ties and tuxedos. It was supposed to be a transient hotel where the people stayed only a limited number of days. But like everything else in the Army, if you were in, you were in and nobody was going to kick you out unless you did something you shouldn't do or asked too many questions. They assigned Curtis a room with two beds because I was with him. He left the Heidelberg area after about two days, and I remained in the room with the two beds by myself, and the sergeant would ask me when I came down in the morning preparing to go out in the jeep, "Are you leaving, Sir." And I would say, "Oh, no. I'll be back tonight." Finally after three or four days of that, he finally said, "Do you mind if we move you to a room with a single bed?" And I said, "Oh, no. That's perfectly alright with me." So I moved to the single bed room and then

again the sentry said, "Are you leaving, Sir?" And I said, "No, I'll be back." The final day, when I did leave, he could hardly believe it. He said, "You're not leaving, are you?" Anyway, that was another of those funny things that happened.

MEIKLE: What did you find in touring the German plastics plants that was especially of interest?

KLINE: Of course, when we went into these laboratories or plants, we asked for the Chief Chemist or the Chemical Engineer in charge of operations. Usually they were available and the staff was available in the neighborhood. It was total destruction. You've seen some of the pictures that were published in Modern Plastics that I took while I was over there. Usually these chemists were living in the area, I don't know in what, but they were there and very cooperative in answering our questions regarding the plant. I remember one fellow who was talking about polystyrene and his boss was very surprised to hear one of his chemists speaking such good English. He apparently did not know that he was conversant in English. I heard him ask him where he learned his English.

We were told in London to look for what were called the KUKO reports, which was an abbreviation for reports of the Kunststoffe Kommission. These were reports prepared by the various plants of IG Farben on the work they were doing on various polymers. They were prepared by the chemists that were in charge. They were submitted to the Kommission which was a high level commission of IG Farben executives from the various plants who apparently met periodically and presumably discussed developments. There were, I think, about twelve volumes, rather heavy folders with hardback covers, in which these reports were filed. Every IG Farben plant we went to during April and May, we kept asking as a final question, "Do you have a set of the KUKO reports?" And the answer always was, "We were directed by orders from Hitler to destroy all that information and we did." Finally, in June on the trip that I mentioned I took with Dr. Curtis, we went down to Gendorf, Austria, where the polyethylene plant was. It was on the Austrian border. They had built the plant in a group of pine trees. It was so well hidden and well camouflaged that it was never bombed. The chief chemist there wasn't particularly cooperative. He didn't volunteer information. He would answer what we asked him, but he was not cooperative in the sense that he was proud of what they had done and wanted to get some acknowledgment that it was good technical work. So we asked him some questions about some development, probably on polyethylene. And he said, "I don't know the answer. I'll have to look in the KUKO reports." I looked at John Crawford and he looked at me. AHA! He reached up on a shelf in the laboratory we were in, pulled down one of these volumes and found the answer to what we had asked him. We went on questioning him. This was in the American Zone, so I was in charge of the group. I said, "Thank you for the information. We have been instructed to take the KUKO reports back to headquarters." He became very red in the face. He pounded the table. And he said, "There will be another war." We knew we would be on the IG blacklist after that. We got a duffel bag - it took a duffel bag

to hold these reports - and put it in the armored car we were traveling in at that time. We had quite a discussion on the way back. My four British companions were from Bakelite, from British Resin Products, from ICI and from Celanese. R.E. Richardson was from Duplate Canada in Ontario. I said, "Now, look. If we turn these over to the Army, we're never going to see them again." They all agreed. We had enough experience with the Army by that time to know that that was true. So we decided as a group that we should keep these German reports for translation and publication. Even the Germans later said later that an IG plant employee during the War was never told what the other plants were doing. They had never seen the actual fellows that were doing the work. Even the authors of the reports had never seen the other fellows' reports on what they were doing. They all said they learned more from the reports I published in Modern Plastics based on those KUKO reports, than they ever knew during their employment.

MEIKLE: So every plant did not have a copy of this set of reports.

KLINE: They had a copy. But, I suppose only the head fellow saw it - and apparently they were not allowed to tell their staff what was going on in the other plants. Yes, the top executive in each plant would have a copy of them, obviously. I say obviously because whenever we asked, they said they had had them but they had been destroyed. So, anyway, we decided first of all if we ever turned them in, we'd never see them again. Then it was decided that since I worked for a government organization and didn't have any proprietary axe to grind or any reason to withhold any information that was in those reports, that I was the logical one to keep them. Of course that put the bee on me as far as responsibility for keeping them was concerned. When we got back to London we had to write copious reports on what we had learned about plastics during our investigations. I didn't know German well enough that I could read the KUKO reports by myself. Neither could any of the others, so nothing appeared in our reports from those KUKO reports.

MEIKLE: So your first report was simply based on your observations and your interviews with people.

KLINE: That's right. I'll get to that in a minute. Anyway, I took those reports in a duffel bag and then I returned to the United States in July. I took them with me very carefully to make sure that I always had them in my possession. I did not let them get out of sight. Got back to Bolling Field in Washington and we had to go through Customs. I had a duffel bag and I guess I had another piece of luggage. First he pointed to the duffel bag that had the KUKO reports and asked me to open that. Well, actually in retrospect I guess it wouldn't have mattered. He wouldn't have known what they were anyway. They would have meant nothing to him and it wasn't any contraband. He wasn't connected to the military. Anyway, I sort of had a resigned expression, I guess, because he said, "No. Open that other bag." That had all of my

clothing in it and I opened that and had nothing to hide. So I got them back to the Bureau of Standards.

I had a girl working for me who had come over from Germany. She was not a technically trained person but she could read German with no problem. The technical terms were somewhat of a mystery to her, but she did an excellent job of translating most of those KUKO reports into English. By then, I had been so much exposed to German, that I could detect errors in her translation and, of course, as far as the chemical terms were concerned, could put the correct technical term in. So when she finished translating them, I would take the time to compare her English text with the German text and make whatever corrections were necessary. I lost that knowledge of German in later years from disuse. We were about three years in the process of translating those long reports. We worked from 1945 to 1948 in translating those reports into English and publishing them in Modern Plastics. First, all of those translated reports were submitted to the Department of Commerce for clearance - to the Civilian Intelligence Group. In July and August of 1945 after we got back, I wrote my detailed summary report of 32 pages on the developments in Germany. The first 16 pages of the report actually were published in October of 1945 and titled "Plastics in Germany 1939-1945." First the Department of Commerce had submitted my report to the Army and asked for clearance and for release to the plastics industry. The Army said, "No. It is classified secret." The reports that the English had, the French had, the Russians had - the industries in those countries had free access to all this information and firms in this country were not getting the information that we picked up. There were some professors connected with the Civilian Intelligence Group, and they were furious. One of them persuaded me to go ahead anyway and publish it in Modern Plastics which I agreed to.

When I read now of what happens to some of the fellows who publish secret reports, I wonder that I was so dumb in those days. But I had the same feeling - that this isn't fair. The industries have it in England, France and Russia, so why shouldn't our industry. Furthermore I had risked my life to get the information. So I sent a copy to Modern Plastics, had it set in type by the middle of September, when we normally close our issues. I guess we had held it up and had not scheduled it for the October issue. But President Truman came along in the middle of September with an executive order that all of the information acquired in Germany during the War by the technical investigators should be released for distribution to the public. That saved my neck. The article appeared in the October issue and the pages are numbered with letters running 152a to 152p (7). It was quite a comprehensive report. Obviously it was inserted into the magazine at the last minute. Then Part II of that report went into the December issue, another 16 pages (8). In the meantime, in November two translated KUKO reports were published. Then nearly every month after that, from January 1946 until November 1948, there were one or more reports on the German developments published in Modern Plastics (9). Finally the publisher of Modern Plastics said, "When are we going to finish up these reports on developments in Germany. They've been going on for over three years." By that time we had reached

the end of the reports that really contained new information.

I got many letters from industry telling me of the significance and importance of these reports to the industry. There was one published in June 1947 on developments in polyurethane by Dr. Otto Bayer of IG. I had a letter from one of the chemical executives of the Eastman Chemicals Co. later, several years later in the mid-fifties, saying that they had read the reports on polyurethane developments back at the time they were published, and wished that they had paid more attention to what I had reported, because they got into the polyurethane field much later than some of the other U.S. firms.

MEIKLE: Was the polyurethane information new?

KLINE: It was new then, oh yes.

MEIKLE: That was a field that was totally unknown here?

KLINE: That's correct. Dr. Bayer of Germany was the pioneer worker in this field and he had done his work during that period, well, perhaps some before 1939, but not known here.

MEIKLE: What were some of the German developments that Americans were able to utilize sooner than they would have if it hadn't been for your reports?

KLINE: That really calls for...

MEIKLE: You mentioned a polyethylene process, one proving better than the other.

KLINE: The high-pressure polyethylene process, as I pointed out, had actually been developed in the United States before the War started. The U.S. process had been transmitted to some company affiliation in Germany - the high-pressure process which was the successful process - before the War. That was just before we entered the War, obviously, and it led to the development of the polyolefins. Polyethylene, polypropylene, polybutadiene, and so forth were all major developments since the War. Polyvinylpyrrolidone, which is a specialty product, was the subject of one of the KUKO reports. The work by J.W. Reppe on the use of acetylene as the basis for plastics was new information to the industry. Research on polyvinylcarbazole and polystyrene was of a good deal of interest. All of these reports even on polymethyl methacrylate, yielded specific information that was new to the United States plastic industry.

MEIKLE: How about various processes and ...

KLINE: The response that I got from the various executives - and they were very liberal in their expression of appreciation for the time and effort that were being made to give them the results of that work - indicated that they were indeed extremely useful to the industry. I had many letters. I am always surprised, and

have always been surprised during my years serving the plastics industry, by the way in which the industry has acknowledged the contributions which we made at the National Bureau of Standards through an activity of this type - dissemination of information. The British were tickled to death to have us publish these reports because that made them generally available in the literature. They never did it. They had the KUKO reports. ICI, I'm sure had translated the German reports.

[END OF TAPE, SIDE 5]

KLINE: A similar situation developed with Professor Guilio Natta who was responsible for the work on the catalyst systems used in the preparation of polyolefins with tactic crystalline and oriented linear structures.

MEIKLE: Linear polyethylene?

KLINE: Yes. There was one other chemist who also contributed a great deal.

MEIKLE: Karl Ziegler.

KLINE: Ziegler's work was very well known. Natta's work was also pioneering work. They received a combined Nobel Prize for their work on catalytic systems for producing linear polyolefins. Actually I knew Natta very well. I had met him many times in the course of the ISO work in Italy. Meetings in Italy, both through the International Union of Pure and Applied Chemistry and the ISO [International Standards Organization]. He and Mrs. Natta had invited us to their home just outside of Milan on one occasion and we had lunch with Mrs. Natta. I believe he was elsewhere at the time, but Mrs. Natta invited my wife and I out to their home and served us a very special luncheon of this speciality of the region which turned out to be sparrows that had been shot and cooked with their heads on. If you can imagine biting into a sparrow and getting buckshot in your ... Well, we survived. But anyway, as I said, they were extremely cordial. Prof. Natta, of course, had become paralyzed in one leg, and she had to help him walk. When he walked to a podium or to his seat, she would put one of her legs behind his and move it forward. He could not move the one leg forward. She was a marvelous woman. We had become very close. He had been in Washington on a couple of occasions and I entertained him locally in Washington.

When he received the Nobel Prize for his work on the catalytic systems for polymerization of olefins, I happened to be in England. ICI had a copy of the Nobel Prize speech which was delivered in Italian. They had translated it into English. My good friend, John Crawford, with whom I had travelled through Germany and become great pals, gave me a copy of the ICI translation of Natta's Nobel Prize speech which went into details regarding the whole process, in much more detail than had previously been published. I couldn't credit John Crawford or ICI for the translation, so I had to use it anonymously. I knew that Natta was going to be in Washington shortly after I came back with that translation. We were having

lunch together at a downtown restaurant and I congratulated him on winning the Nobel Prize and on the talk that he had delivered in connection with the presentation, and said I would like to publish that in Modern Plastics. He didn't hesitate. He said, "Of course." So I went ahead and published it (10), under his by-line, of course. And shortly after it appeared in print, I got a very nasty letter from his associates in Milan asking me on what basis I published Prof. Natta's Nobel paper, and if I were going to publish it, why didn't I publish it in full, including the section on the crystallization of these polymers. I replied that I had had lunch with Dr. Natta in Washington and he had given me permission to publish the report and that the only reason that I didn't publish the crystallization section was because I didn't have the photos that showed the crystallization and that part of the paper was meaningless without the pictures, so I had to delete that part of the lecture. If they would send me the pictures, I would be glad to publish that section too.

I never heard another word from them. It takes a certain amount of brass, I guess. But I was fairly confident, knowing Natta so well. Why would he say no? It didn't mean anything to him and there wasn't any reason why his talk shouldn't be published in Modern Plastics. In this letter they sent me, they also said they had made some agreement with another publication in the United States, not a plastics magazine, but a scientific magazine, for publication. In general, as far as Modern Plastics is concerned, we don't mind duplicating publication of a paper which has appeared in another non-plastic magazine. Of course, we observe copyright, but when you get permission from the author to publish something, you don't have to inquire about... I knew nothing about their plans or his plans... No, it wouldn't have been Natta's plans, it would have been someone else in that laboratory that would have been dealing with that particular aspect.

MEIKLE: I would like to ask one more question about the War. Not so much about your own experiences, but what would you say the impact of the War was on the development of the plastics industry? How would you compare the plastics industry as it was, and as the public perceived it, say in 1938, with 1948 or 1950. In other words, what did the war do to that industry?

KLINE: - I published in Modern Plastics every year since 1940 an annual review of developments in plastics and at the end of every decade, in other words, in January of 1940, 1950, 1960, 1970, and 1980, I published a summary of the developments in plastics during that decade (11). I have those comparisons of what took place from decade to decade in the records that I have over here. To answer your specific question, I would have to consult those decade reports to pinpoint specific developments. But I will say, that as far as plastics were concerned it was a whole new ballgame after the end of the War because of the fact that all of these plants had been built under government auspices for the production of the monomers and the polymers. On the industrial side, the civilian side, applications of plastics was

all set to take off on a tremendous surge, which it did, of course. What was, when I first became Technical Editor of Modern Plastics, a 125 million pound industry has become as of now (1987) approximately a 50 billion pound industry. I have some figures that I have prepared which show that from decade to decade the growth of the industry slowed down from six-fold, then down to three-fold, then down at present to two-fold. Stanford University had a survey out that they published in 1973. I quoted it when I was elected as a charter member of the Plastics Hall of Fame in 1973. They were predicting a growth of ten-fold from 1972 to the year 2000. That adds up to 100 billion pounds in the year 2000. And as I said, we're now at 50 billion pounds and of course a two-fold increase would reach the 100 billion from the present day level, in the next 13 years. The figure as far as related to the Gross National Product, GNP, is even more impressive: from 1.9% to 7.2%. In other words, metals and glass are going down as materials for construction and general use, and plastics are replacing those heavier materials, and, as far as glass is concerned, a more fragile material. Of course, the problems of the environment have to be resolved during this coming decade too.

[Slight pause in tape]

KLINE: In 1951 my activities became more administrative. I was much more occupied with getting funds to support the work of my colleagues in the Division.

MEIKLE: We probably should talk a little bit about that tomorrow exactly how you went about getting funding.

KLINE: Most university chemistry departments know what it is, they go through this process all the time of trying to get government grants for research. There is no pleasure in it and it takes your time away from scientific activity. But they have to do it and I had to do it for the people who were working for me. The ISO work we haven't covered at all and I want to mention that. That began in 1951. Also I want to mention the publication of The Analytical Chemistry of Polymers in 1959-1962. I think that was a significant contribution at that time. Of course, it is hopelessly out of date now, but that's the way things go. I have some figures on how fast knowledge becomes obsolete. One paper I read has something to the effect that in one decade 50% of knowledge becomes obsolete. Two decades 25%, three decades 12½% is still pertinent. In four decades you're down to the situation that 6% of the knowledge you had when you began 40 years previously is not obsolete. It is the same with this book, The Analytical Chemistry of Polymers. In its day, it was the primary source of information but today it is out of date.

MEIKLE: I noticed that the copies of the second and third volumes at the University of Texas Library are still being checked out all the time.

KLINE: Yes. It has some basic information but actually there

is so much new information, that it may be that from the historical angle or from the background angle, it is of importance, but I have no illusions about it being a really useful source. It's of more interest for background.

[Slight pause in tape]

MEIKLE: A note on the evening of May 15th. The previous fragment was recorded as Dr. Kline and I were closing out the interview sessions for today and we began plotting out what we would discuss tomorrow. When I mentioned that we would cover what he had done after 1951, he began by saying he was primarily involved in administrative things. At which point, I thought it was worthwhile to turn the recorder back on for a second.

[Slight pause in tape]

MEIKLE: This is the morning of May 16th. Yesterday we had carried our discussions through the World War II period and into the 1950s. And I will start this morning simply by asking you how your work at the Bureau changed during the post-War era and the kinds of things you found yourself concerned with.

KLINE: Beginning with the decade of the fifties, my responsibilities changed rather drastically. Up until that time, of course, I had been the Chief of the Plastics Section. In 1951 I was promoted to the Chief of the Division which covered six sections. Also in 1950, the question of participation in international standards arose. As Chairman of the ASTM Committee D-20 on plastics at that time, I appointed a committee headed by Ed Cooper of the Du Pont Company to sound out the industry on participation in the International Standards activities and their willingness to support a secretariat for such activity in the American National Standards Institute. The Committee reported back that they felt that it was desirable to participate and accept the offer of the ISO, the International Standardization Organization, to take on the responsibility for the secretariatship. We made a survey in early 1950 of ASTM D-20 committee members and we received just one negative vote. It was a very interesting response from a very good friend of mine in the industry, but it is typical of some of the attitudes towards international participation. I'll read this very short statement. "I believe the benefits to be gained by participation will not justify the effort, and that it is optimistic in the extreme to think that the work can largely be done in ASTM meetings. An ASTM representative at an international meeting will have his hands tied anyway, and can be outvoted at will. [That is, by other countries] As long as we are giving everything away, why not retain the right to specify what we are giving and how to test it?" That was the only negative.

MEIKLE: Who was that from?

KLINE: Actually from Joe Ryan of Libbey-Owens-Ford Glass Co. He was a very outspoken individual. I played poker with him many

times and he was not quite as uncooperative as that particular response would indicate. Well, this led to the first meeting. But first let me say that, realizing that this international standards activity was going to take a considerable amount of my time, I consciously made the decision that my assistant in the plastics section, Frank Reinhart, would take over the primary responsibility for the work on the ASTM plastic standards in Committee D-20 and I would devote my time on standards work to the ISO standards for plastics. The reason that I did this is based on an early experience in my career at the National Bureau of Standards. The chief of the division in which I worked had a background entirely in inorganic materials, and had transferred to head the Division of Organic and Fibrous Materials from the Cement Division of the National Bureau of Standards.

MEIKLE: This was?

KLINE: Mr. Warren E. Emley. He was very much interested in organizational activities but of course he had no background whatsoever in the organic materials field. When the ASTM plastics committee was set up, he found himself as the first chairman of that committee which was perfectly logical since he had participated in all the early discussions of such work in ASTM. But then about a year or two later, a federal specifications committee was organized.

MEIKLE: This was in the 1940s?

KLINE: No. This was still in the late 1930s. This Federal Specifications Committee was to be composed of plastics experts from the various government agencies, military and civilian, that would be concerned with applications of plastics. Of course they wanted Federal Specifications to cover the materials that they were going to use. The proposal was that again Mr. Emley would head up that particular group, even though in no way could he classify as an expert in plastics. This was so absurd that I went down to his office, this was about 1939, and said to him, "Mr. Emley, if you are going to be the plastics expert at the National Bureau of Standards, I am leaving." This, of course, took him somewhat aback but he quickly assured me that in no way would he take over such responsibilities and that he would nominate me for the chairman of that particular committee. From then on we had no difficulty and worked cooperatively. I remember one incident - at a section's chief meeting in the Division - we were discussing the capabilities of Frank Reinhart and he said, "Well, there is one thing that I would like to say. Now that Frank Reinhart is assistant chief of the plastics section, I no longer worry about whether you are enticed to leave the Bureau of Standards." One of the other section chiefs spoke up and said, "Well, that's the sign of a good executive." Anyway, we got along. There was no difficulty in working with Mr. Emley. He finally accepted the fact that he was not an expert on organic materials and polymer chemistry. He retired in the early 1940s to accept a professorship at Rutgers University in the inorganic field.

[END OF TAPE, SIDE 6]

KLINE: To get down to details in the ISO committee work, it was obvious that the work should be directed by someone very familiar with the field. So, as I say, in the 1950s, my primary attention on standards work turned to the international sector. I remained a member of ASTM Committee D-20 on plastics. I am still a member today and vote on the various and sundry ballots, primarily terminology work in the field to which, as an editor, I can make a contribution. I can no longer contribute, of course, to discussions of testing methods because I don't have access to a laboratory. But in terms of terminology, I can contribute.

The first meeting of ISO/TC-61 on plastics took place in New York on September 18, 1951. We had representatives at that meeting from Australia, France, the Netherlands, Sweden, Switzerland, and the United Kingdom, in addition to a very large delegation from the United States numbering some 15 individuals. It was a very successful meeting in organizing subcommittees and in agreeing upon priorities with respect to what we should work on initially. I was elected Chairman of the ISO/TC-61 Committee at that time and Dr. Dubois of France was selected as the Chairman for the working group on terminology. That meeting was followed by a second meeting in Turin, Italy, in October 1952. The minutes of that second meeting are much more copious than the ones from the organization meeting. There was a great deal of work accomplished during the interim period. Just as in the ASTM work, and even more so, a consensus had to be reached regarding the methods of test and regarding the meaning of technical terms that are used in the industry. Again the results were several years getting into published form. This would be expected, even to a greater extent, where you have to deal with representatives from different countries who speak different languages. And, of course, the part that I participated in to the greatest extent was the one on terminology. We looked, first of all, at a list of equivalent terms in English, French and Russian. Some of the other countries added their own German and Spanish equivalents, but the official languages of ISO were English, French and Russian. It is surprising how difficult it is sometimes to come to an agreement on what a particular technical term covered. It was obvious that definitions were needed so the work on definitions of terms was also promptly initiated. The third document that the terminology group handled was the question of symbols or abbreviations that were used in the literature for shorthand reference to materials (12). That was the general scope of the work of the Committee ISO/TC-61 in the terminology field. That work was initiated in 1951. Now in 1987, we have sent to the ISO headquarters in Geneva, a 200-page document covering definitions. The first 100 pages of that document cover actually the terms and their definitions and the other 100 pages have indexes, French-English, Russian-English and English-Russian. An English-French index was not needed because the terms in the document are arranged in alphabetical order in English, with the French equivalent definition in the opposite column. This document should be out before the end of this year

and represents a culmination of a tremendous amount of work through that period.

MEIKLE: Through the entire 35- or 36-year period?

KLINE: Actually, the first definition document of any consequence was published in 1978 and it was a relatively short document of 30 pages or so. This document is, as I say, 200 pages, and is a real contribution. In 1984 there were a sufficient number of published ISO plastics standards, which are always available as separates, to warrant the publication of a handbook on plastics standards in two volumes, both an English set and a set in French. The Russians are responsible for publishing the documents in their own language. That is a very brief summary of the work in the international field. The Committee meets every year in a different country - they have only missed one or two years and they have been doing it since 1951. It has brought me into contact with the plastics experts in all of these different countries. It has been a very rewarding activity from the standpoint of broadening my vision with respect to what was going on in these other countries in the plastics field - again benefiting both my work at the National Bureau of Standards and my work as Technical Editor of Modern Plastics. I remained as Chairman of the Committee for eight years. In 1958, which was our 8th meeting, the meeting again was held in New York and following that meeting a new Chairman took over the responsibilities. The country that has the secretariats for these ISO committees is generally given the privilege of having the Chairman of the Committee appointed from one of the members of their delegation. There has been a succession of individuals from the United States that have held that position. That has been, as I say, a very important activity that did occupy a good deal of my time during the 1950s at the National Bureau of Standards. It is one activity in standards that I have continued to participate in and have only missed two meetings during the period from 1951 through 1987.

MEIKLE: This is going to be a very simple question, the answer to which may be obvious. When you talk about definitions, are they usually chemical definitions in terms of chemical formulae, or in terms of manufacturing process.

KLINE: The scope of the terms is very broad. They cover everything from definition of particular polymers, the definition of polymerization processes, the definition of fabrication processes and, of course, definitions of property terms in all of the various fields, mechanics, optics, thermal and permanence properties, and so forth. Sometimes, some members object because some of the terms we are defining can be found in the ordinary dictionary. However, because of language difficulties, we have found that these are sometimes terms which you would think that everyone could agree upon without the necessity of writing a special one for the plastics work, but in many instances misunderstandings arise unless we have written a definition for such a term specifically as it applies to the plastics industry.

So it is an extremely broad document from that standpoint.

MEIKLE: Did you ever run into problems of nations or firms not really wanting to disclose their manufacturing techniques, wanting to keep the definitions broad enough to hide what they were up to.

KLINE: No. The collaboration among the group that specializes in terminology is very open and frank. Actually, when we are defining terms we are not getting into proprietary details of processes. So there is never any problem with regard to the content of the definitions. The primary problem is sometimes getting the U.S. and the U.K. to agree on what is proper English. There was a Russian Chairman of the terminology group at one stage and we were having a discussion with the U.K. delegates about the proper English. He looked somewhat baffled and he couldn't quite follow the discussion. I said, "Mr. Chairman. We are just trying to agree on the proper words in English."

MEIKLE: While you were focusing on this, what were some of the other concerns you had at the National Bureau of Standards in the Division of Polymers?

KLINE: That's a good question because this was the time when I took over the position of Chief of the Division in 1951 . That, of course, meant that I was spending a lot of time working with the section chiefs from the leather, textiles, paper and rubber sections as well as the plastics section. So my activities as Chief of the Division were much broader than plastics. It was also a time in which the emphasis at the Bureau of Standards shifted from being concentrated to a large extent on products and materials, to fundamental research on the chemistry and the physics of materials. The Division during that period was completely reorganized shortly after 1951 and became known as the Polymers Division rather than the Division of Organic and Fibrous Materials. Sections were set up to deal with specific areas of the chemistry and the physics of polymers. It was a very interesting change and we had some excellent scientists in charge of that work. They have made major contributions to the fundamentals of plastics and polymers and the work has been well recognized throughout the scientific community.

MEIKLE: Who would you single out as among the more important chemists at that time and what were they involved in at the National Bureau of Standards?

KLINE: I hesitate to single out particular individuals. Dr. Leo Wall was an outstanding contributor. Dr. Samuel Madorsky also. Dr. Robert Simha, Dr. Jacob Mazur, but I obviously am leaving out many. The staff of the Division at that point was about 150 and about a third of those were Ph.D.'s. They were all extremely capable and I really should call the roll of the whole group. Of course the bibliography of the publications by the National Bureau of Standards at that time will show who these

individuals were and their contributions.

I will add that during that period of international concentration, I also participated in the work of the International Union of Pure and Applied Chemistry (IUPAC). This activity on polymers in IUPAC was conducted by a group of us from the countries that were participating in the ISO work. A Plastics and Polymers Group was formed in IUPAC, and we were concerned primarily with having seminars and symposia on advances in the chemistry of plastics. During that period of the 1950s, there were quite a number of such IUPAC seminars and other symposia on plastics and polymers in which I participated by reporting on the developments at the National Bureau of Standards. I was out of the laboratory by that time and out of any direct responsibility for close contact in the laboratory with the work that was going on. But as Chief of the Division that was involved in such work, I presented reports at various symposia in Europe on what we were doing at the National Bureau of Standards. These were always very well received. They took place in Germany at the great Kunststoffe meetings that were held every three years. The IUPAC meetings were held, normally, every other year, and I have a series of publications summarizing the development of our fundamental research at the Bureau of Standards in this field. They were usually translated into French and German and in the case of meetings in Italy, they always appeared in Italian as well as English. It was an interesting period and of course a useful way to become informed on what was going on in the way of fundamental research in these other Western countries. It blended with the work on standards because, of course, the work on standards has to be based on the best technology resulting from work on the fundamental side. So it was a good combination; covering, through the ISO work, the developments in terminology on the testing and specifications work, and the activities of IUPAC on the progress being made in fundamental chemistry. It was a very good combination. The work on polymers ultimately was assigned to a Commission on Macromolecular Materials and the Plastics and Polymers Group was absorbed into that overall committee on Macromolecular Materials.

MEIKLE: This was within IUPAC?

KLINE: Yes. That was within IUPAC. I was a member of the Macromolecular Commission and have participated, and continue to participate today as a correspondent, in the work of the Macromolecular Nomenclature Commission of which Dr. Norbert Bikales at the National Science Foundation is Secretary. We have excellent cooperation between the ISO work and the IUPAC work in the field of developing the definitions due to my work as a liaison representative between ISO and IUPAC. I have a file here devoted to that particular continuing activity.

I want to say one thing about the work at the National Bureau of Standards in this Division of about 150 persons. I may have mentioned this before, but at least 50% of our budget at NBS in this particular field was supported by a transfer of funds from other government agencies that had special problems that they wanted us to work on in the plastics field. The responsibility

for obtaining these transferred funds to keep half of the staff on the payroll, of course rested ultimately on my shoulders as Chief of the Division. I am sure that there is many a head of a Chemistry Department in the universities in the same predicament. They have their associates, of course, write up the proposals and so forth, but there is a final editing process and also a final contact process where the head of the department or the Chief of the Division has to take a part. I had worked on transferred funds at the Bureau practically from the time that I arrived there in 1929, and I was very familiar with that activity and knew what the proper approaches were. But I must say that that was the least satisfactory duty that I had to perform and one that I was most relieved to shed when I retired in 1963.

MEIKLE: You had mentioned yesterday that in the early days the National Bureau of Standards maintained the primary laboratory for plastics research in the country. As you moved into the 1950s and the 1960s, with universities opening up more research laboratories and getting more defense contracts, did you find that you were in competition with the universities?

KLINE: No. Princeton University had established a program on plastics early in the 1940s, I would say, and of course some activity had also been started at Brooklyn Polytech in this field. But the real spread of university participation, specifically the setting up of laboratories and courses and research programs dealing specifically with plastics and polymers, began about 1960. This was practically at the end of my career at the Bureau. In what I had referred to as our Visiting Committee there were quite a number of well-known polymer chemists: Ray Boyer of Dow and Charlie Overberger of the University of Michigan, and J.F. Downie Smith, a mechanical engineer at the U.S. Shoe Machinery Corporation...

[END OF TAPE, SIDE 7]

It was at the time of my retirement that the Chairman of the Committee, at a dinner we had at the Cosmos Club, made the statement that there was no question but that during this period of the fifties and the early sixties, the Polymers Division of the National Bureau of Standards was one of the outstanding laboratories in the country contributing to our knowledge of polymer structure and reactions.

MEIKLE: On that subject I wanted to ask about the three-volume work that you edited, The Analytical Chemistry of Polymers (13). How did that fit into the scheme of things?

KLINE: That was a definite major contribution to the polymer field during that period. The publishers of the High Polymer Series, Interscience Publishers in New York, had asked me to undertake the preparation of a twelfth volume for the series, concentrating on laboratory analytical procedures. It was a natural, of course, as far as my work was concerned, because of the fact that a primary responsibility of the National Bureau of

Standards was the development of testing methods for materials. It was supposed to be the twelfth volume but it turned out to become a twelfth volume in three parts. For the first volume, I contacted the leading manufacturers of polymers and asked them to contribute chapters on the methods they used in analyzing the various types of commercial polymers. I had an excellent response to that request. That first volume appeared in 1959, that means that the actual work probably started about 1955, as it takes many a day, particularly when you have multiple collaborators, to get the manuscripts completed. Some authors are very prompt - others drag their heels. Anyway, that first volume appeared in 1959 and was very well received and actually translated into a number of different languages which I will get to later. For Parts II and III, the chapters were all prepared by individuals working in the Polymers Division at the National Bureau of Standards. These chapters were on special techniques used for determining the structure of polymers and for determining special properties such as glass transition temperatures, which are really a part of analytical chemistry. We took the term "analytical" broadly, to mean any method used in determining molecular structure or identification procedures for chemical analysis. As the editor of those three volumes, of course, I read very carefully every manuscript and sent suggestions to the authors for additional details where necessary. Also in certain cases where one or two of the authors were slow in coming up with the final manuscript, I actually wrote part of the chapters for them. That was the exception, however. The final volumes, Parts II and III, were published in 1962. That three-volume contribution to the High Polymer Series became recognized throughout the world as a source of information on how to analyze and determine the structure and chemical characteristics of polymers. It was translated into Russian in three volumes. On the occasion of one of our meetings in Russia, in Moscow, subsequent to...

MEIKLE: This was at an ISO meeting.

KLINE: Yes. An ISO meeting. The Russians very kindly gave me two sets of the three-volume translation - the Russian translation. I have a copy of the Hungarian translation of Volume I and I understand that translations were also made in other languages. Of course, I am sure that the Russians did not pay any copyright money for permission to publish. I remember that, when the Hungarians translated the volume, I received a small royalty payment as part of my share, but nothing ever came through from the Russians as far as payment was concerned. But at that meeting in Russia at which I received those two sets of books, there was a meeting with some IUPAC representatives.

Particularly, Dr. V.A. Kabanov of Moscow was present at this meeting. He was one of their leading polymer scientists. We were introduced and I was talking with him. He finally said, "Are you the Kline that was the editor of the three volumes on Analytical Chemistry?" I said, "Yes." "Now, I place you." He was a much younger fellow and he had not been associated in any way with the ISO activities. Other members of the Russian Institutes did

participate in those ISO activities but he was strictly on the IUPAC side. He made a very nice compliment regarding the three-volume series on The Analytical Chemistry of Polymers. He said, "When my students come to me with problems relating to the determination of structure or composition, I tell them to go to Kline's book on analytical chemistry." So I naturally was pleased with that.

There was a tremendous amount of time that went into the preparation of those three books which were at least of some use in the universities as well as in the commercial laboratories. For a lot of those manuscripts the proofreading was done right over here at the Gulfstream Hotel, right across the street from where we are talking, during my so-called vacation periods in Florida. With a Division of 150 people, you don't have time to do that type of editorial work during the day at your office. I will also add that when I retired in 1963, the Polymers Division was split into two parts and the new Division Chiefs each had about 75 individuals to take care of, rather than 150.

MEIKLE: In editing those books, did you devise the general outline of what you wanted it to contain and then farm out various sections?

KLINE: Oh, yes. Sure. I let people know the outlines - that was my responsibility. I worked very closely with Dr. Eric Proskauer of Interscience Publishers and with Edmund Immergut who was also connected with Interscience Publishers. But it was my responsibility as editor to decide what materials should be covered and what companies would be asked to write the chapters. The same is true of Volumes II and III - even more so because I was in direct charge of the work of these individuals and, of course, naturally I had discussions with the staff to work out a consensus. I don't want to say that it was entirely outlined by myself, but we had staff meetings to determine what should be covered. Of course, it was my responsibility to decide which author would have the primary responsibility because a number of chapters were actually written by multiple authors.

MEIKLE: I would like to ask a general question at this point. What sorts of things would you like to emphasize about your later career after your retirement? And what kinds of things have we missed?

KLINE: We haven't mentioned the work at the National Bureau of Standards on the preservation of the Declaration of Independence and the Constitution of the United States, which I swore to uphold when I took the Oath of Office for employment at the National Bureau of Standards. That was an interesting assignment. The Declaration of Independence was on exhibit at the Library of Congress and the Constitution was tucked away someplace in the archives of the Library where it was not subjected to direct sunlight. It is in excellent condition. The original copy of the Declaration of Independence today is barely legible, partially because of exposure to light. Back in 1823 a copperplate engraving was made by a wet-sheet transfer of ink from the

original document. I guess it must have been the only process available at that time. Considerable damage was done to the writing that had been inscribed on this parchment, a proteinaceous material, sheepskin. Fortunately, that plate now gives us facsimile copies that are much clearer than the actual original. And, also on exposure at the Library of Congress, it was discovered that some bookworms had gotten into the case where it was exposed, and were gradually eating away the edge of the parchment. So the Library came to the National Bureau of Standards and asked us to look into the question of better preservations methods. The whole story is told in the NBS publication entitled "The Preservation of the Declaration of Independence and the Constitution of the United States", which I wrote (14). I was assigned to head up that project because, of course, parchment is an organic material and a protein polymer so it was a logical choice. Our textiles, paper and plastics sections were also actively engaged in a study of the permanence properties of these materials.

We drew on the experience that had been derived over a period of many years prior to the time of this request in 1950. We developed a plan to put these original parchments into a Thermopane enclosure (a glass enclosure), to back them up with very pure cellulosic sheets made in the paper mill at the National Bureau of Standards, and to put a mixture of helium and some moisture in the enclosure to keep the parchment from becoming brittle, and to seal them up in that atmosphere with a detector to indicate whether there was any leakage. This activity received a tremendous amount of publicity. I should say that the Libbey-Owens-Ford Company supplied the Thermopane enclosures and also the technicians to actually do the sealing of the containers. On September 17, 1951, the 175th Anniversary Year of the signing of the Declaration, there was a ceremony held at the Library of Congress at which President Truman and various other officials including the Chief Justice of the United States, Fred Vincent, and the Chairman of the Senate Committee that was concerned with the work at the Library of Congress, Senator Theodore F. Green, all participated in the ceremony held at the Library of Congress regarding this protection of the documents. Actually, the ceremony was held prior to the actual sealing operation and was purely put on for the media, for the purposes of television and the news reporters. Subsequent to the ceremony held at the Library, the actual documents were brought back to the National Bureau of Standards. We had a small laboratory there where the documents were actually sealed up. That is going to receive a flurry of publicity this coming September 1987, I'm sure, during the celebration of the signing of the Constitution. I know the National Bureau of Standards plans to set up an exhibit portraying its part in contributing to the preservation of those documents. We subsequently also enclosed the Bill of Rights in the same enclosure and the same atmosphere. There were a great many requests following that work in 1951. I know I received a letter from Israel regarding the preservation of the Dead Sea Scrolls and we had a request from the Governor of Texas who wanted some Texas manifesto preserved in a similar enclosure.

MEIKLE: We have a Declaration of Independence too.

KLINE: Right. Well, anyway, those two were just typical of a half a dozen or more requests that we received for treatment of similar historic documents. The Libbey-Owens-Ford representatives played a major role in supplying the technicians to handle the sealing of the glass enclosure but they were not interested in sending their representatives all over the world to do these things. We were never able to cooperate with these people, but I am sure that they benefited from the detailed discussions of what was needed to protect these documents for decades or centuries to come. They benefited from our experience.

MEIKLE: Are those protection methods still in place? They haven't had to be renewed in any way?

KLINE: No. In the initial stages of preparing the enclosures we did find some leaks around the entrance point of the leak detector device, but those were corrected. Actually the documents were transferred shortly thereafter from the Library of Congress to the National Archives. We did additional work at that time with arranging filters over the light sources and filters over the cover of the case in which these enclosures are on exhibit at the National Archives. The reason for putting these filters both at the light source and on the cover of the case is that if we had a light source with the filter without the reciprocal cover over the case, the documents would appear yellow. They wouldn't appear in their natural color. But by putting a filter both at the light source and the document case, they appear to be their natural color. Our Optical Division at the National Bureau of Standards played a major role in deciding exactly what wavelengths should be allowed to beam onto the documents in the exhibit hall. There were other Divisions of the Bureau which also participated. But it is all described in that publication on preservation of the documents cited above.

There was one other activity on sealing of documents that occurred a few years later when they remodeled the front entrance of the Capitol building in Washington, laid a new cornerstone, and deposited a copper box in the cornerstone with various and sundry documents relative to this period. The next time they make extensive renovations to the Capitol - maybe another 100 years - they will take those out and look them over. They requested us to use the same process of sealing them in a helium atmosphere with some moisture to preserve those documents. At that ceremony at the Capitol, it was President Eisenhower who did the honors as the official layer of the cornerstone.

There was another similar assignment, outside really of the field of polymers, except for the fact that organic materials were involved. In June 1952 I was requested by the State Department to go to Germany to take measures to ship St. Stephen's Crown and Robe and other Hungarian relics from Germany back to this country. This was a top secret assignment. I was cleared for top secret information. The reason for the transfer was that in 1952 the American Commissioner who had been in charge of the occupied area in Germany after the War was being withdrawn and the responsibility

for the area was being turned back to West Germany. There was this set of historic, nearly 1,000-year-old, relics that were of sufficient importance that they wanted them to remain under our jurisdiction until the time was propitious for their return to Hungary. Because of the fact that the robe was a 977-year-old robe woven primarily of an organic material and more susceptible to damage than the metallic crown, sceptre and orb, I was asked to use the benefit of the experience we had had in the preservation of the Declaration and try to be sure that no harm was done to these historical relics during the period that they were under our surveillance. So I went over to Bonn, Germany, where these relics were stored. Actually there were no more than six individuals who had any knowledge of where the relics were and where they were going to be shipped. The robe, of course, was fragile. It has gold threads all through the embroidery work on the robe.

[END OF TAPE, SIDE 8]

KLINE: The robe needed to be shipped in such a way that there would be no creasing of the fabric. Otherwise, undoubtedly it would have cracked. So we had a big plywood container prepared - spread out ten or twelve feet. This robe was extremely heavy. I don't know how St. Stephen ever managed to stay upright when he had this around his body and the heavy crown on his head. We tried it on for size, incidentally. We had this special container prepared in Bonn and took special precautions to avoid movement of the robe within the container. Then the State Department took over the job of shipping the relics by destroyer or cruiser to the United States. Then we had a representative from the National Bureau of Standards go to Norfolk to pick up these packages. He had no idea what was in them but he was instructed to take them from Norfolk to Fort Knox, outside of Louisville. Then I went with the representatives from the State Department in Washington to Fort Knox. The Declaration and the Constitution had been stored at Fort Knox during the War. They have underground storage vaults where articles of this type can be stored in a relatively good conditioned atmosphere. The lock on the particular vault in which the relics of St. Stephen's were stored was sealed. I had a house key with me at the time and the imprint of my house key was among those that were put on that seal at Fort Knox.

There was a story in the Palm Beach Post, September 23, 1965, on the St. Stephen's relics which told how the Hungarians had secretly taken these relics from their museum and turned them over to the American soldiers for obvious reasons - with the Russians advancing. They were taken from Hungary to Bonn, Germany; the newspaper story went on to say - "And there the trail ends." At a 1964 ISO meeting in Budapest, a Hungarian hostess told a British delegate, "We don't know where they are today, but we think they probably are in the Vatican." Years later the story came out. President Carter made the decision to turn them back to the Hungarians, and on January 6, 1978, Secretary of State Cyrus Vance formally presented them in Budapest to the President of the Hungarian Parliament. The National Bureau of Standards in its news

bulletin (NBS Standard, Jan. 25, 1978) had a big two-page spread - a story of the history with the heading "Now It Can Be Told" with pictures of all of the relics and the robe.

A year or two later the ISO meeting was held in Budapest, and of course, I was very much interested in going down and seeing them on exhibit there. I went with John Lawrence of SPI and our wives and we went into the room. There was a long line of people going through that particular hall to see these relics, just as at National Archives there are frequently lines of people lined up to see the originals of the Declaration of Independence and the Constitution of the United States. While I was in that line I got to talking to one of the Hungarian guards and I showed him this picture from the National Bureau of Standards of all these relics and how they came to be at Fort Knox and how they were returned. He looked at that and he said, "No, no. Put that out of sight. Don't show that here." But actually as I say, the State Department took every precaution to see that no damage and no harm came to these relics while they were in our possession. They were stored underground for a short period. In other words, the Hungarians had dug a hole in the ground and put them in the ground until they were able to contact the American soldiers and have them taken to Germany. Of course, St. Stephen was supposed to have been the founder of the Hungarian Empire. The Hungarians knew that the United States had had them somewhere and they were a little irritated that it took so long to get them back to where they belonged in Budapest. That was the reason this guard, when he saw it said, "No, put that away. Put that away. Don't let the people see that." I don't know whether he thought they might turn on me and hold me responsible for the years in which they were stored in Fort Knox.

MEIKLE: International crisis.

KLINE: Right.

MEIKLE: I have one other rather general question I wanted to ask and it is something that hasn't come up yet at all. It has to do with the image the American public has had of plastics which seems to have seesawed back and forth, positive - negative, over the years. There is no image problem with stone or concrete or wood. Why is it that plastics have had to fight a bad name or bad image?

KLINE: Actually with stone or wood or metal - there are many failures. We've seen a lot of stories recently about bridges that have been collapsing, and they are not made with plastics. Other materials have their shortcomings as well. The industry is very conscious of the problems of the environment and the position of plastics in that environmental problem. I think we have previously discussed the fire problem and that hazard. But the other major problem is the increasing volume of plastics. We mentioned 50 billion pounds at the present time annually and the fact that that is continuing to grow with the possibility of reaching 100 billion pounds by the year 2000. We have always in

our NBS work tried to extend the life of plastics from the standpoint of their use but there is the problem of litter. Packaging is one of the major fields for plastics use. There is concern over the disposability of plastic containers and the plastic foam or plastic sheet materials that are used in the packaging industry. Of course, in addition to plastics, you see beer cans, or metal cans I should say, thrown all over the place. You see shattered glass containers on the roads that have been thrown out of cars and broken up. As you say, plastics have had this bad reputation, but the litter problem applies also to these other materials.

The industry is conscious of that. They have many studies under way - pilot plant work - in some cases actual plants that have been built to recycle these plastic materials by recovering the resin or other chemicals and reusing them. There is also the other disposal method of burning the materials to generate electricity, which is economically feasible and is done not only in this country but abroad to a considerable extent. The problem with burning any organic material, whether it is garbage or newspapers or wood clippings and so forth, is that they generate carbon dioxide and there is this concern about the increasing level of carbon dioxide in the stratosphere and its effect on the transmission of the necessary ultraviolet and infrared rays. So that problem pertains to plastics, of course, in a major way, but it also applies to many other things. When you burn coal you generate carbon dioxide; certainly when you burn oil you generate carbon dioxide. It is recognized that this is a problem that is going to have to be faced very broadly. A recent issue of Modern Plastics had a considerable treatment of the present stage of development of recycling of plastic materials and the necessity of having the proper equipment - if the plastics are burned - to filter out some of the more objectionable by-products (15). In the beginning of the plastics industry, back in the 1930s and 1940s, the emphasis was not so much on the hazards to the environment; it was on the fact that they were substitute materials and some people felt natural fibers, cotton and wool, were much superior to synthetic fibers because they were natural and these others were artificial. But obviously they complement each other. Most of the shirts we buy nowadays have a label on them "65% polyester and 35% cotton". So there is a blend there of materials as there should be. In fact, the word in German for plastics is Kunststoffe which literally means artificial materials. That was back in the 1930s, before the War, that there was this public reaction.

MEIKLE: Do you think that that has pretty much subsided?

KLINE: Oh, yes. I think people realize now that plastics are excellent materials for many applications. The fact that glass and aluminum are twice the weight of plastics helps to promote the better image for plastics. But let me assure you that this is well known to the industry and it is one of the major problems that the Society of the Plastics Industry is working on.

MEIKLE: Except to return at the end to the early part of your

life, I don't really have any further questions. So why don't you conclude with anything you would like to add about your career in general.

KLINE: Maybe this is a little egotistical but I have, during the course of my career, received literally hundreds of letters from industry and from the universities, commenting on the contributions that have been made by the work done on plastics at the National Bureau of Standards. I have two or three of those that I have selected here to indicate the tremendous support and recognition that our work at the Bureau has received over the years I spent there. For example, there is a letter I received dated February 14, 1945. This letter is actually addressed to me at Modern Plastics in New York rather than at the Bureau of Standards, which is very often the case. Prof. Norris Shreve at Purdue University, a Professor of Chemical Engineering, says in his letter, "I cannot tell you how many times I have referred inquiries regarding plastics to your magazine, telling many correspondents to either subscribe to this magazine or to refer to it at a library. Modern Plastics is outstanding largely because it combines two aspects, frequently separated. I refer to the technical and business sides. You do not neglect either, indeed you show them in their true working union." This is a spontaneous letter. Unfortunately I have never met Prof. Shreve. I have had further correspondence with him, but this is typical of the type of letter received regarding the work that I have done at Modern Plastics.

Another one is from Herman Mark, which I introduce because he has also done one of these oral histories. In a letter he sent me on January 26, 1946, Herman says, "Dear Dr. Kline. I have, of course, studied and restudied your very interesting articles in the various issues of Modern Plastics in 1945 and would like very much to have reprints of all of them," indicating that at Polytech Institute of Brooklyn they were also recognizing and benefiting from our publications made at the National Bureau of Standards.

MEIKLE: Those being primarily the publications of what you found in Germany?

KLINE: Yes. But he is referring to 1945 and you remember our reports on the developments in Germany actually went on month by month into 1948.

MEIKLE: So he is referring to the earlier ones?

KLINE: He's referring to the first reports published during October, November and one in December, 1945. Of course we had published many other articles in 1945, so he was referring both to the German plastics reports and to the reports of our research at the Bureau. In other words, he didn't limit his request to the German plastics report. And finally some 40 years later, I had a letter from Dr. Bill [William O.] Baker who was Chairman of the Board of Bell Laboratories. This letter came July 8, 1980. He also has participated in this oral history program. I have had

many contacts with Dr. Baker from the time that he was active in the Bell Telephone work on plastics. He says, "Dear Gordon: I was delighted to hear from you and to have an occasion to say again how creative and distinguished is your role in the technology and, in recent decades, in the publication of information about plastics and polymers. You have formed an indispensable link between the science and technology of a most vital and fast moving field of materials. I have had the pleasure of seeing generations of research and development people,as well as production and application groups, literally educated by the publications you have helped to shape." At this stage of my life, I take some satisfaction in being able to reread some of these letters that have been so generously sent to me. Of course, naturally they are not referring specifically to me alone but they are, I am sure, recognizing that there were many associates of mine at the Bureau of Standards who were responsible for many of these contributions.

One of the most significant events in my life was my selection as a charter member to the Plastics Hall of Fame, along with nine other individuals from the plastics world. This was in September of 1973. At the induction ceremonies, Lee Iacocca, who was then Chairman of the Ford Motor Company, subsequently he transferred to the Chrysler Corporation, was the guest speaker. Louis Rahm was also one of the inductees; he was in charge of plastics work at Princeton University which was really the first university laboratory that got involved with plastics as a program. Lee Iacocca in his introductory statement said that "My first contact with plastics came as a student of Prof. Rahm's at Princeton University." And Louis Rahm looked up and he was astounded by that statement by Lee Iacocca; I expect that the general course which he gave to the students involved a large number of students, and Lee Iacocca was just one more name among hundreds. But anyway, everybody gave Louis a round of applause. It was a very interesting incident at that dinner.

MEIKLE: Now, since I inadvertently missed an hour of our conversation yesterday, we are going to start over again and talk about Dr. Kline's early years and his introduction to the profession of chemistry. You were born at Trenton, New Jersey, Feb. 9, 1903. Please describe your family background.

KLINE: Well, the family background is that we were what today certainly would be called at the poverty level. My father was a printer by trade, but in Trenton, New Jersey there wasn't much work for printers, so he was a salesman of candy at a weekly wage of $12.00 as I recall. Certainly not more than $14.00. We lived in an Italian community in Trenton. There were a lot of immigrants from Italy working in the mills in Trenton.

Roebling's Mills, which made huge cables for supporting bridges, etc., was right behind our house. I have a passing memory - I was only about five or six years old at that time - of when the Roebling's Mills caught fire and burned for two to three days and, of course, the ashes continued to smoke for another month. They finally built a railroad track into the plant and hauled away the debris in freight cars. That was where I was to

begin with. We subsequently moved to a better part of Trenton. But it was in 1916 that my father finally got a job as a printer at the Curtis Publishing Company in Philadelphia and we moved from Trenton to Merchantville, New Jersey, where I took my first year in high school. My father subsequently took the Civil Service examination as a printer and was offered a job at the Government Printing Office in Washington, D.C. at approximately $40.00 per week and we moved to Washington in 1917. I'm sure that my salary when I graduated from George Washington University with a Master's Degree in 1926 and went to New York State Health Laboratories was $40.00 a week. That was the beginning salary for chemists with a Master's Degree. So it is difficult to reconcile such figures with the wage scales today. For example, when I retired from the Bureau of Standards in 1963, my salary there was the top permitted for Government scientists - it was a special Public Law 313 position, one of 300 such positions authorized. It was roughly $18,000 a year. Today, that same position pays in the $70,000 range.

[END OF TAPE, SIDE 9]

KLINE: In Washington D.C. I went to Central High School and I took my first chemistry course under a Dr. Clinton. He was a very fine teacher and we had a good deal of laboratory work associated with that course. In fact, when I entered Colgate University in September, 1921, I was excused from freshman chemistry because I had taken chemistry at Central High School. It was recognized as being equivalent to what the universities were teaching at the freshman level. The same was true in other fields. I was excused from freshman English at Colgate because I had had my senior year of English at Central High School in Washington. I was very fortunate to have had such an excellent beginning training at the high school level. The question was asked, "How I wound up having a career in chemistry?" I started out as a pre-med and my fourth year of college training was taken as the first year of medical school at George Washington University.

MEIKLE: And that was applied toward your Bachelor's Degree at Colgate, as well.

KLINE: Yes. I went back to Hamilton, New York and received my A.B. degree, (curiously enough), in June of 1925. After I had finished the fourth year of college work at the George Washington University Medical School, I ran out of money. As I said, my father's weekly salary in those days was roughly $40.00 to $50.00, at the most. There was no money available to continue in medical school so I took the Master's Degree work in chemistry at George Washington University, working part-time in the Department of Agriculture as a Student Assistant.

MEIKLE: Let me ask you. Who was the major professor there?

KLINE: It was Dr. Solomon Acree, who was a very well known chemist at that time, in particularly the field of ionization and

pH. Classes were given at night, and there were many of the government chemists who attended those classes to get the benefit of Dr. Acree's knowledge of the field. I remember one in particular, Dr. Orville May, who was at that time working in a Department of Agriculture laboratory. Subsequently he left the government and ultimately became head of the Coca Cola Corporation. And he was just one of several I could cite. Anyway, I wound up in a biochemical position in Albany, New York.

MEIKLE: This was after you received your M.S.?

KLINE: Yes. That's right. This would have been July of 1926. I went to Albany to do work in the biochemical area at the salary of $2,080 which, again, if you break that down to a weekly salary, was about $40.00 a week.

MEIKLE: And you were working for the State?

KLINE: I was an employee of the Department of Health of the State of New York. Yes. I quickly realized there that the major positions and promotions were for individuals who had M.D. degrees rather than chemistry degrees. So I took steps, for that reason, and for another reason which I will cite later, to apply for a civil service position as a chemist. In February of 1928, I was offered a position at Picatinny Arsenal in Dover, New Jersey. The laboratory had been completely destroyed by an explosion of TNT that had been stored at Lake Denmark, close by. Research work on explosives was just beginning again there after the terrific explosion in 1926. In those days and I think perhaps today, civil service employment in Washington, D.C. was based on quotas for states. I had been living in the District of Columbia which is not a state, so I was not particularly likely to get a position in the civil service unless I could claim a State residence. So the year in New Jersey gave me residence and eligibility for becoming part of New Jersey's quota of civil service employees. I don't think they really pay any attention to this anymore.

I'll cite a brief incident at the Picatinny Arsenal. They were concerned with the stability of smokeless powder and they had made a long study in which they found that it deteriorated to some degree and then became stable. It is cellulose nitrate. But they weren't too confident that that was a reliable result, so my first assignment was to again study the stability of cellulose nitrate. The laboratory in this arsenal was closed at 4:30 or 5 o'clock. Then all the lights were turned out and all the electricity was turned off in the laboratory for safety reasons, so it was impossible to conduct continuous hydrolysis experiments. I suspected that if it were continuously heated in water, this deterioration from hydrolysis would continue. I got permission to rig up an outfit out-of-doors with a supply of electricity which they would leave on at night because there was no hazard to the building. Sure enough, what had happened previously was that they heated the smokeless powder for eight hours in the daytime and left it at room temperature 16 hours overnight during which the nitric acid by-product was reabsorbed

by the smokeless powder; so they reached an equilibrium as far as desorption and adsorption was concerned. Of course, it turned out that the process of hydrolysis was a continuing affair which was the only logical reaction from the standpoint of basic chemistry.

Dr. Acree was at that time at the National Bureau of Standards in charge of research on farm waste products with money transferred from the Department of Agriculture - farm waste, in those days, being considered to be as much of a problem as it still is today. Farm waste and farm surpluses. I contacted him and of course he immediately took steps to have me transferred to the Bureau of Standards. It was a very fortunate circumstance. It was my objective to get back to Washington so that I could continue my graduate studies toward a Ph.D. outside of hours. That was really what I was working towards from the time I took the job in Albany to the time I took the job in Washington on June 1, 1929.

MEIKLE: So you did Ph.D. work at the University of Maryland at the same time you were working at the Bureau of Standards?

KLINE: Yes. That's right.

MEIKLE: Was that difficult or did the two fit together?

KLINE: Of course that work was done mostly in outside hours or on hours of leave. I would go over to College Park, Maryland, to the campus, for eight a.m. classes. Then I would make a dash back to the Bureau of Standards, which these days would take maybe three-quarters of an hour, but I usually accomplished it in about 20 minutes, to get back to the Bureau after an 8:50 adjournment of class. Also, there were classes scheduled at 4:00 p.m. on the campus so that by taking leave, it was possible to attend the late afternoon class. I can assure you it was a strenuous schedule, but I finally received the Ph.D degree in June 1934.

MEIKLE: You had recently been married at this point, too?

KLINE: Yes. I was married, actually in 1926, before I moved to Albany, and then, of course back to Washington in 1929. By that time I had been out of university classwork for over three years and I found it very difficult to regain the skills in note-taking and preparation for examinations that I had previously had. One problem I immediately encountered was with Dr. Nathan Drake, who was in charge of the chemistry department at the University of Maryland. He got his Ph.D. at Harvard and had just recently come to the University of Maryland. He was an excellent lecturer but was very rapid fire, both with the tongue and with the chalk on the blackboard. It was a real speed method of trying to take adequate notes. He wasn't using a text, he was using a self-prepared series of lectures, and when he would finish the 50-minute lecture, he would walk to the exit door and look back and smile, and all of us were still scribbling to high heaven to try to get down the last details of what was on the

blackboard. But it worked out. As for the Ph.D. thesis work, I originally had envisioned having some of the work done at the National Bureau of Standards credited for the thesis work. But I quickly realized that Dr. Drake was not particularly interested in following such work and he was much more interested, of course, in the projects which were being done under his supervision at the College Park laboratory. Fortunately the topic was related to the polymerization processes dealing with decenes, primarily, so that really fitted in with my later work on polymers and polymerization. Prof. Frank Whitmore at Penn State University had developed an explanation of why certain reactions resulted in certain by-products, based on the formation of a positive or negative ion on a hydrocarbon radical or a hydrocarbon derivative. In the course of this work on the decenes, we were able to add a link to Prof. Whitmore's picture of how these reactions occurred. In the case of the decenes, there was a particular mechanism that he hadn't previously covered in his theory. This work was all really related to what today is known as free radical initiation of polymerization but at that time they were referred to as ion-initiated reactions. So it was an interesting project and resulted in publications both in the NBS Journal of Research and in the Journal of the American Chemical Society (16).

MEIKLE: At the same time, what were the early projects that you were involved in at the Bureau of Standards? You mentioned a farm waste program.

KLINE: Yes. The primary work was on the use of surplus sugars as a source of making xylose, which is a five-carbon carbohydrate which could then be subsequently converted into furfural which was at that time becoming of increasing importance as a raw material - a monomeric material. In the course of that work, we had to have a method of determining the residual aldose sugar that was left after the oxidation with nitric acid. It soon became apparent that the reliability of the method being used at that time had an error probability of 5% or more. So I developed a refinement of that method which led to a method for determining aldose sugars that was accurate within the 0.2 to 0.3% limit of error required. This was another contribution that was published both in the chemical journals and the National Bureau of Standards publications (17).

MEIKLE: It was right after you arrived at the Bureau that you were assigned to cover synthetic resins and plastics? How did that come about?

KLINE: Yes. One of the NBS responsibilities was to keep in touch with industry needs for analytical methods. The synthetic resin industry was just developing. At that time there was only really Bakelite (the phenolic resin) and Celluloid (the cellulose nitrate material) that were classified as plastics. There were some products produced from casein, a protein material, by reaction with formaldehyde, and some materials in which coal tar pitch was used and shellac was used, but those were natural

substances rather than synthetic. Nonetheless, research in the industrial laboratories indicated that there were going to be many other types of materials developed. A fellow working with Dr.Acree was asked if he wanted to work in this field of synthetic resins and he took it on in 1929, but, for some reason, he quickly lost interest and transferred to the Department of Agriculture. So they asked me early in 1930 if I was interested in taking over. I was never reluctant to tackle any such new field. At that time there were only three magazines in the field. One was the predecessor of Modern Plastics, which was at that time headquartered in Chicago and another was Kunststoffe in Germany. Then there was the third journal, Revue Générale des Matiéres Plastiques in France. So I only had three journals to review to find out what was in the literature. Again, there were only about four or five books at the most. One was Carleton Ellis' Synthetic Resins and Plastics (18), there was a book on Cellulose Esters by Edward Worden (19), and Emile Hemming had written a book on Cold-Molded Plastics (20). Those were the three that I can think of offhand.

MEIKLE: So it was a pretty easy field...

KLINE: Yes, to catch up on the literature. But I did that conscientiously. The German I couldn't read at that time, so that wasn't much help to me, but I could read the French, and of course went back through the magazine Plastics to find out what was going on and what had been done in the field. But it was a good place to start because it was right at the beginning of the plastics industry.

MEIKLE: Well, I guess we've come to the end of the interview. I would like to thank you Dr. Kline for providing what I think is a very full and complete record of your career and the founding and the growth of the industry and your place in it and the place of the National Bureau of Standards. Thanks very much for doing this for me and for the Center for History of Chemistry.

KLINE: Thank you, Jeff, for doing this. I want to say that I have been very happy with the way you could supply me with some names and events that slipped my memory during the course of your questioning. I think you had a remarkable set of facts that you had accumulated which should make a very interesting book on the history of plastics and I hope that it is not too long before you can finish that.

MEIKLE: So do I. Thank you very much.

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