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Interviewee: Roger Adams

Interviewer: John Mellecker

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Unknown: The following recording is a December 8th, 1971, reproduction by the university of Illinois archives of a series of edited tape-recorded interviews of Dr. Roger Adams made by Mr. John B. Mellecker, historian of the Chemist's Club New York City. The recording sessions were in New York City on November 20th, 1964, and on February 12th and March 15th, 1965. And probably in late July and perhaps October 1965. The edited tape was secured from Mr. Paul B. Slaughter[?] Jr. on December 1, 1971.

John Mellecker: Yeah, I would like to first introduce Dr. Roger Adams, honorary member of the Chemist's Club New York. And today's date is the 20th of November 1964. We are in historic Room C, and now, Dr. Roger Adams.

Roger Adams: The number of questions you've asked me it would be difficult for me to answer in a short period, but I will say a word about the club. To me, it has been valuable because it's given me the opportunity of meeting other chemists whenever I've come here to New York or on business. I have always enjoyed my stay here and since I joined the club in the 40s, I have seldom stayed any place else in New York.

In general, the club is a gathering place for chemists and like scientific meetings their chief value is in the opportunity given for chemists to discuss their activities with each other. I would say that the club has played an important role in the development of chemistry in the United States because New York is a headquarters for practically[?] all the chemists.

JM: Do you feel of the chemists sooner or later come to New York?

RA: I feel that they all come here sooner or later. The American Chemical Society meets here every three years so that brings thousands here from the outlying districts and anybody who's in industry as soon as they got to a level that involves contacts with other concerns[?] always comes to New York.

JM: Dr. Adams, I've done a little checking in American [inaudible] science and have something to talk about with you. I find that you were born in 1889. Is that right?

RA: That's right.

JM: And you started in the profession of chemistry by getting your bachelor's in Harvard at 1909.

RA: Right.

JM: Now, very soon after that, the records show you went over to Berlin. And did you come into contact, would you just mention the names of some of the world-renown chemists, or who would later be world-renowned, that you came into contact with then?

RA: Well, I went to Germany immediately after I received a doctor's degree with a fellowship that was awarded me by Harvard University. The person, the laboratory where I worked at the beginning was the University of Berlin, which was headed by Emil Fischer. I didn't work with him personally but with one of his assistants, Professor Otto Diels, who later moved to Kiel and received a Nobel Prize for the Diels-Alder reaction discovery. After a time, I then moved to the Kaiser Wilhelm Institute so called at that time, now the Max Planck Institute in Dahlem. And there I worked under Richard Willstätter. Now the people whom I saw in action and became superficially acquainted with them at the time were Nanst, distinguished physical chemist, with Hoffman, with Beckmann, Beckmann thermometer and Beckmann rearrangement. And their colleagues [inaudible] were still alive and I remember seeing him once in Munich.

JM: You came back to the United States, 1914, was it? About 1914, about the time World War I was getting underway.

RA: I came back. Yes, that's right. And then I taught at Harvard for three years and incidentally and Radcliffe[?] as well until 1916, when I was offered a position at the University of Illinois and I decided to accept it. And I've been there ever since except for war period, the two war periods. First World War I was in charge of an offense[?] chemical research unit and D. James B. Conant, who was a well-known chemist, was in charge of another one. And W. Lee Lewis of a third[?]. And that was the total of the offense research program.

Then there were defense research units, one of which I recall was headed by Bob Wilson, who was later chairing the board of the Standard Oil of Indiana. Then I've really[?] taken an active part in chemistry ever since. I was made head of the department in 1926 and went on until I retired in 1954 from headship of the department. Continued for three years as a research professor before I retired completely. Second World War I was out for five or six years then as a civilian as a member of the National Defense Research Committee.

JM: You were over in Germany as a Deputy Military Governor, weren't you?

RA: No, no, I was scientific advisor to General Clay who was Deputy Military Governor. And he had three top advisors, a political advisor, an economic advisor, and a scientific advisor. And I held that position. It was rather interesting but General Clay actually didn't quite know what a scientific advisor was supposed to do and I had to tell him. [Laughs] But that is to be expected in the military.

JM: Did this give you an opportunity to see much of applied German science?

RA: Well not very much, no because Germany was pretty badly destroyed at that time and this was right immediately after the war. Well, I had the opportunity of going around Germany and seeing the destruction and that’s about all one could say. It was all reorganization then, butting[?] up the IG and forming smaller units that could operate in competition with each other.

JM: Then you went on to Japan, the record shows.

RA: Well then, I came back and General McArthur requested the State Department to send over a scientific group to advise him on how best Japanese scientists could be democratized. That was a pretty big order because it was [inaudible] Americans who were familiar with the scientific programs that were underway in Japan. But the State Department went to the National Academy of Sciences, and they asked me if I would go over as chairman of a committee of seven. And we did so. Went over in ’47 as I recall in the summer.

Not being informed by the military government about what the Japanese were actually doing, we had to spend most of our time in finding out what Japanese science was and how individuals operate. So, we went from one city to another in a private train and had formal meetings usually twice a day until we learned just what the situation was. At the end then we came back to Tokyo and wrote up a report for the military government telling them what we thought should be changed in order to instill a little more democracy in Japanese science then had been there previously.

JM: Did you find much difference between Japanese science and German science for example?

RA: Well of course German science was much more developed than Japanese science.

JM: Oh, really [crosstalk] no organization between people?

RA: Oh, between people it was more or less the same except, well, in Japan it was pretty compartmentalized. That is, one professor, distinguished professor would have an area where he would place his students and that area never hides students from any other professor. It’s only been in recent years that that’s been broken down so that the big concerns will hire from any university. In 1947, that was not true. But I’d been over there, I was in Japan last spring, and the situation has completely changed over there. Although their educational system is now a kind of composite of the old German system and the American system. Their viewpoint on science and the way they operate is about the same as it is in any other modern country which well-developed in science.

I consider the Japanese very able and I think in the next decade or so you’ll hear from them and see that they do have originality. They’ve always had the reputation of being copiers. And we were copiers until about 1930. In chemistry we’d just copy everything the Germans had until we finally had these items manufactured in this country. And then after that we started out on

our own looking for new things. And the Japanese [inaudible] in that category at the present time. They now have practically everything they need manufactured within the country, ninety percent of the things. So, they have the research laboratories now available now for looking for [inaudible]. It’s a good illustration is what they’ve done in the way of transistors.
The transistors were discovered in this country by the Bell Telephone Laboratories but within six or eight years a physicist in the Sony company discovered a model occasion[?] which is so important that the Bell Telephone Laboratories had to adopt it over here.

JM: Has much change taken place in the German system of education since you were over there as a student.

RA: Well, I’ve been over to Germany of course a good many times in the last thirty years and I would say that the general system has not changed materially. Each professor is a little king in his own area, and he has assistants and so on. And it’s just now that they’re trying to break this down in the sense that there would be more than one professor at a university. And there are several that have, let’s say, or make them professors at the equivalent of an associate professor. And I think there’s one university that has two professors in the same field, but that’s very exceptional. But Switzerland operated on the German system but at the Technische Hochschule in Zürich they have I think three full professors at the present time, which is rather extraordinary for Europe.

JM: You have contacts all over the world [inaudible] people that you know.

RA: Well, I’ve met people around the world because I’ve gone to the international meetings so that, even though I haven’t seen them at the home bases I know them and correspond with them occasionally.

JM: How many students have you turned out as Ph.D.’s in your career? Now I heard you say this I think before the [inaudible] of the chemists.

RA: I had 182 who took their doctor’s degrees with me.

JM: That’s quite a few.

RA: Well, it just happened to work out that way.

JM: Well Dr. Adam’s you’ve had a long history of rather tremendous accomplishments. People have given you the record shows, and this is not an up-to-date record, I know at least twelve medals. I should say societies have, including the Perkin Medal. How can you characterize your accomplishments?

RA: Well, hard to say. I’ve always worked pretty hard and got the most out of my innate ability and then I had a lot of friends, and it’s always your friends that get the medals for you.

JM: Well, I'm sure you're being very demure about this. But what can you claim to be your major contribution, the things that you feel are most important that you were able to achieve in organic chemistry.

RA: Well, my chief achievement I believe is in the training of students. I've had many students who have been very successful, and I think it was because of the attention that not only I but my colleagues in the early days gave to these students to help them out and to advise them how to get along and to succeed. So personally, I consider my achievements will be chiefly in the training of the graduate students. As far as researches go, well, I worked in many fields and contributed something in each, but nothing of peculiar importance, let's put it that way.

JM: Oh, how many papers have your name attached.

RA: Well, I've published over 400 scientific papers. I don't know the exact number.

JM: That's an impressive number.

RA: Well, you must remember that I had at one time, I had 20 to 25 men working with me over the years. If they are capable individuals, they accomplish a lot.

JM: How do you go about organizing the support for programs where you get a lot of money to put in to the employment or financing-

RA: I had practically no money because my active days were the time when you couldn't get money. It's only been in the last 15 years that you could [drag?] on the federal government money. So I never had that experience.

JM: [Crosstalk] Oh, you had to get it yourself.

RA: Yes, well, I, most of my work was done with graduate students who were assistants or had small fellowships from the universities and some industrial fellowships, but they were relatively modest. But it was not until in the 30s that I had any post-doctorates which now [in the phase?] of most of the work that I'm in in this country turn out because most of the young, aggressive professors have 6, 8, 10, 12 post-doctorates with them. And of course when you have trained men like that you can turn out work very rapidly and you can work on much more difficult problems than you can if you are training the student at the same time you are trying to accomplish results. But in the olden days we never put the research goal first. We put the training of the student first and then if we could get the results [so much?] the better. That's the difference in what it is now. I think that too many of the professors today are thinking about research results and not too much about the students who are doing the work.

JM: You have decided upon and you have gotten your graduate students to work upon programs that have turned out to be very important. How did you make those decisions? How did you do it in the times when you were doing those things? How was it done then by you?

RA: You mean how did I happen to select those problems? Well, that's from reading and while I was reading something in one field, I would see something in another that interested me. And I'd mull it over for a few weeks or a few months and then decide to have a student step in and see what he could do. And then it gradually expanded into an area where [I worked?] for eight or ten years. And that's about the way things develop.

In the case of marijuana where [we?] worked out the structure of the [acting?] principle. I had done some natural product work first and then the director of the narcotic agency in Washington asked me if I wouldn't undertake this to see what I could do with marijuana. And I told him I would provided he would furnish the material and see that I got a license and not be hounded all the time by federal agents. And so we undertook the work and within two or three years we had a constitution and one of the things we did do is to demonstrate that the test that had been used by federal agents for detecting marijuana was false. That is the test that they used was indicative of an intermediate which was non physiologically active and we actually isolated the product and showed how that intermediate could be converted to the active principle.

JM: Was this a pretty complex synthesis as of the time?

RA: Well again it wasn't so much a synthesis as a determination of structure. No, it was, we didn't know how, what it was, and we found out what it was. It's a three-ringed molecule and I wouldn't say it's complicated as the [unintelligible] and some of the things being studied today. But, well you study polymers and very large molecules, the complications are much greater than when you're working with relatively small molecules like this marijuana.

JM: [Would you step over here?] [Unintelligible] Do you mind that we continue while-

RA: No, go on. What do you do, cut this off, cut this up till you get something?

JM: That's right. [Laughs] You didn't have the instrumental advantages that are enjoyed today-

RA: No, you're absolutely true and that's the big difference between what the [hims?] can do in the university [work?] today and what they did in, when I was most active.

JM: Absolutely the biggest difference, huh.

RA: Oh, tremendous difference. Because even before I stopped chemistry, I was making use of IR and MMR and the other tools which had become available in the last ten or 15 years. And I could see how if I'd had those tools ten years before I could've solved some of the problems in at least months instead of years or weeks instead of months.

JM: So it was necessary to go through a number of grueling steps to be short circuited with a new apparatus.

RA: Oh yes. One can get so much information from infrared data that you can't get easily by chemical manipulations and reactions. And when it comes to [material?] chemical things there simply weren't any methods that can compare with the physical instrument [unintelligible].

JM: What would you say would be the first time you might have accomplished [unintelligible].

RA: It's hard for me to say because I don't recall but I'm sure it would be back in about the 20s when I was invited to come here my [friends?] and did. But I never came regularly until after the early 40s.

JM: Maybe I better shut this off.

RA: Wasting a lot of good tape there. [Unintelligible] as I'm eating. They don't put it on now talking about… 210 dollars [Laughs].

JM: Testing. Would you just say something to test so I can regulate-

RA: Oh yeah. What you want to have him talk about now?

JM: Well-

RA: You're wasting an awful lot of tape on me, I can tell you that. [Both laugh]

JM: Well Dr. Adams, you were telling me a moment ago while we were eating about having received a telegram. Would you mind repeating that for the record now and what happened.

RA: Well, I was at a meeting of the advisory board of the Robert A. Welch Foundation in Houston, Texas. I received a wire which was sent via Urbana, Illinois where I lived to Houston, indicating that I was the recipient of a national medal of science, which is given by the federal government. And the telegram was signed by Lyndon B. Johnson. The award will take place sometime in January at the White House.

JM: Would you mind reading the telegram?

RA: It says, "It gives me the greatest pleasure to designate you as one of the 1964 recipients on the National Medal of Science in recognition of your outstanding contributions in the physical sciences. You have my warmest congratulations and gratitude for the service to science and to the nation, which have merited this award. Presentation of this award to the medalists will take place at the White House [probably?] shortly after the first of the year. You will be informed later of details regarding the ceremony. Lyndon B. Johnson."

JM: Will this be the first time that you received a medal in the White House?

RA: Yes, this is the first time. I did receive the medal for merit, which however was delivered to me by one of the generals in Chicago under very fancy circumstances in that it was a military occasion with, and set up as a military affair.

JM: Where are some of the other places you received medals that you can recall? Just tell me [one?].

RA: Well, the first one I ever received was the Nichols Medal, the New York section. Then I’ve also received the T. W. Richards Medal of the Boston section, the Willard Gibbs medal of the Chicago Section. The Midwest award of the St. Louis section. The Hofmann Award of the German Chemical Society. The Davy Medal of the Chemical Society of London. The medal of merit, which I mentioned a moment ago. The appointment as Honorary Commander of the Most Excellent Order of the British empire, normally called CBE for short by the British government. And then the Perkin Medal and the Franklin Medal and the Elliot Cresson Medal. And I guess that’s all.

JM: [Laughs] You guess so. Why do you feel you were given the CBE? That’s a very high honor of the British Empire.

RA: That came because of my war work on the National Defense Research Committee, and I was closely associated with the British during that period. My counterpart in London was Sir Robert Robinson.

JM: Also an honorary member.

RA: Of the?

JM: Of the Chemists Club.

RA: Yes, yes. He and I visited back and forth during the war and he held a similar position in England to the one I held here in the United States. As a member of the National Defense Research Committee of which James B. Conant was chairman, I had the general supervision of the [unintelligible] and chemical engineering research related to the military services.

JM: Well at this time this involved the support of a great deal of research that had either direct or indirect relationship to defense? Was it not?

RA: It was all defense.

JM: Was this really the forerunner of what was later to be supported by NSF and so on?

RA: Yes, it was the nucleus of the research during the war. The military services did very very little. It was all done by the National Research Defense Committee. And most of the important discoveries were made by the investigations supported by the National Defense Research Committee, not by the military services.

JM: So you might say that through that activity, you were really in on and you were helping get started what was to become a vast program of government supported research.

RA: That's right. That's the way the NSF later started, through hearing about [Bush?] indicating about how the National Defense Research Committee should be continued in substance after the war and it developed into the so-called National Science Foundation.

JM: Now you were in charge of all the chemistry and chemical engineering research?

RA: Yes. Of course, when you say in charge, I just had general supervision because there were many many people working under me in different, around the country, different laboratories.

JM: Wasn't this a vast effort and operation?

RA: Yes, of course, the physics operation was bigger but the chemistry and chemical engineering operation was amounted during the last period of the war. About 18 million dollars a year for research in chemistry and chemical engineering at that time. Of course, that will buy lots more than it will now.

JM: Yes, at that time that was a very large [unintelligible] amount.

RA: The physics, however, was very much larger because it involved, of course, the [approach to?] the atomic energy [efforts?] and then before that was split off from the National Defense Research Committee.

JM: Well at the time did you have kind of this feeling that you were involved in such a historic operation?

RA: Well, I didn't think of it as a historic operation. It was getting the job done and winning the war, so. We did make a number of significant contributions to the chemical field. The ones that come to mind immediately are the finding a suitable method for production of RDX, which as you [unintelligible] with TNT would very much appear in anything they used in naval shells [previously?] and torpedoes and the aircraft fire. And it made a tremendous difference in the effectiveness of the fighting from submarines and [any?] aircraft [fire?] till the end of the war. And the other thing is, the incendiary bombs that were dropped on Japan were developed by the National Defense Research Committee and had to be pushed down the throat of the military before they [accepted?].

JM: How about the quinine substitute?

RA: Well, we never got anywhere on that particularly because we did get [adabrine?] and [plasmaquine?] and so on. Well, that didn't all come from United States efforts. That was an international effort. And I don't recall just who got the drugs that were actually used in place of quinine.

JM: Well now you did liaison with England on this. With Sir Robert.

RA: Well not on [adamine?] because that was under the Medical Research Committee. You see, there were two committees in Washington under the Office of Scientific Research and Development, which consisted of [Bush?]. Then there was the Medical Research Committee and the National Defense Research Committee and all the medical things-

JM: All the medical chemistry was over-

RA: Was over there, yes, was not under my supervision. It was under the supervision of Newton Richards who was the vice president of the University of Pennsylvania.

JM: Now do I understand though that you did have a liaison with Sir Robert Robinson on matters of chemistry-

RA: Yes, on matters of chemistry [crosstalk] in the same area that I was interested in, he was interested in over there. But he also was I think included under his general supervision also the medical end. But medical end here was in a different unit.

JM: Were they able to achieve much in the way of chemical research efforts or results during this period in England?

RA: Oh yes, certainly the British made a real contributions. Of course, they were the ones that demonstrated that RDX was really a potent explosive and tried to sell it to our military people who said, "Oh well they had it before and it wasn't any good." Finally, in desperation they came to the National Defense Research Committee and said, "Well, we will provide you with material if you just make the test and demonstrate to the military government in the United States that it is good." And so we did that in a place called Brusden outside of Pittsburgh. And as soon as our results were completed, then the military was willing to go along with the manufacture and before the war ended, they had a plant down in Tennessee developed by the Tennessee Eastman Company that was producing a million and a half pounds a day of the explosive.

JM: Well now this work took extensive liaison between yourself and research leaders at various universities and industry?

RA: That's right.

JM: Was that right?

RA: Um hm.

JM: So, you on one hand helped formulate what the program should be and on the other hand [crosstalk]-

RA: Yes, administered it. That's right. And it was of course a question of so much going on, it was a question of coordination, more than a question of direction.

JM: Would you tell us something about what kind of a man Sir Robert Robinson was as you saw him?

RA: Well, he not only won but is, and I think even today he hasn't, although he's 77 or 8 years old and not in as active condition as he was years ago, he's a man with an unusual memory for facts. He has contributed much in many fields of organic chemistry.
And he's very ingenious in determining whether a program was the right one to follow or not. His chief asset was his, the prestige that he held with all the chemists in the world. And still does. I would say that if we disregard what is now being actively done because of course he is more or less retired, if you pick out a man of the last fifty years who would stand out in organic chemistry, I'd be inclined to pick Sir Robert as the most distinguished.

JM: Well could you tell us what he was like when you went over to Britain and meet with him and that sort of thing. Sometimes it's difficult to think of a man at a various, certain stage of his life. We tend to think of him as he grows older, and we know him as he-

RA: Well, I couldn't see that he's any different from what he was at any time. In fact, he's just about the same as if he was here discussing things with me and he would ridicule making a wreck of hearing just as I do. He's just a normal human being. But he's pretty opiniated, but he's usually right.

JM: With a tremendous memory.

RA: With a tremendous memory. I don't know how good it is now, but it certainly was remarkable when he was a younger man.

JM: Well, you went over across to see him [crosstalk]-

RA: Oh yeah, several times. And he came over. We went over on the naval, what do you call them-

JM: Flying boats?

RA: Yeah, flying boats. And that was the way you got there in those days.

JM: Long trip.

RA: Yeah, 16 hours to London.

JM: Where would you meet him in London during the war?

RA: I'd meet him different places. Even the hotel or some research laboratory where work was going on. When I was over there, we usually met in a group rather than as individuals. He came over here more often than I went to England. It's a little easier for him to come than for me to go over there.

JM: Is there any anecdote that you could tell us about yourself, him, how you were in a meeting one day and something happened?

RA: Well, I'll tell you an amusing story that has nothing to do with chemistry but it's about him.

JM: Good.

RA: He came to Washington for one of these meetings and he had a good many meetings where I wasn't necessarily attending. And this particular day he had a lunch engagement. He wasn't accustomed to wearing a hat at home, but coming to the United States in the winter time he thought he ought to bring a hat along. Well, this hat had been, let's say, 15 to 20 years old. It was pinched in the front so there was a hole in it and then the band was marked with grease spots, ringed with grease spots around. So it was a pretty terrible looking hat. Went out to lunch one day and took this hat with him. And after the luncheon, he'd hung it on the side of the wall. And after luncheon he got up, went out, forgot his hat. Went back to the hotel and about two passed before he thought of the hat again. And then he was going out another function, he thought, "Well, I better wear my hat," and he couldn't find. And then he thought back, "Well I took it to that restaurant." So he went back to the restaurant about 4:00 in the afternoon and there was nobody eating there, but he looked up to the side, and he saw his hat still there. So he went up and took the hat off and walked out to the middle where there was one waiter standing there doing nothing and he gave a broad smile to the waiter. And he said, "Well he's been there Wednesday and forgot to take his hat so now he was coming back to get it." And he was certainly impressed with the honesty of you good Americans. And the waiter replied, "Well sir, you don't think any good American would swipe that hat, do you?" [Both laugh]

JM: Have you ever run into, not anything equally as funny maybe, but equally as entertaining around the chemist's club in your years of being in and out of here?

RA: Oh, it's hard for me to recollect now. I'm sure there have been things, but I just don't think of anything right in the moment.

JM: Yeah. Well I have a note here to ask you about your attitudes towards basic versus applied research, which we hear a lot about.

RA: Well of course being an academic man I am very much in favor of basic research because if you don't have plenty of basic research coming along, the opportunity for doing new applied research begins to dry out because usually new things along applied lines are based on new discoveries in basic research. And of course, many of the companies today are engaged in basic research just which might well be done in universities. There whole [inaudible] these things that they find will be able to fit into a program [of?] finding something practical. Bring in dollars. So it's just inconceivable that basic research will ever stop as long as we have progressive countries that expect to achieve new things.

JM: Do you think we’re going about it in this world the way we should today?

RA: Well, it’s pretty hard to say. In industry for example, there’s been such a demand for young chemists, chemists I’d say with a Ph.D. degree, that the initial salary has been raised to what I consider, little too high a level. And as a consequence, the smaller companies can’t afford to run research laboratories anymore. It’s only the big companies that can afford to carry on research.

JM: Is this a major disadvantage that you’re referring to of paying such high starting salaries?

RA: Well, that’s one of the reasons certainly and the other reason is that the overall cost is so great for making discoveries in science.

JM: Isn’t the cost of instrumentation today a very high factor too?

RA: I wouldn’t consider that as too significant in the industry. It’s very high for university because these instruments costing several hundred thousand dollars, the university can’t raise that much money. Well, an industry can raise that much money, but it’s the constant cost of the scientists and the assistants and all the other things and the overhead and the laboratory and so on that runs [them?] into money.

JM: Well Dr. Adams you’ve had a lot of experience inspiring and energizing men, young men who you bring into chemistry and bring up in chemistry. Men who go on in their careers, and then you’ve had many occasions to bring them together as experienced career men. What would you say about the problem of getting scientists to work with scientists?

RA: Well scientists often have idiosyncrasies just like everybody else so that the man who is a specialist in science who can also fit into society pretty well is a very valuable individual. Often you find scientists who are such introverts that they can’t work with other people. And although they’ll make their contributions, their contributions can never be as great as if they can get along with people. And that of course is in every category, every field of endeavor. It’s necessary for a man to fit into society and then fit into the development even though he may not like many of the things that go on. He at least does his best and works with a group to succeed. And you can’t do it individually. The genius who works alone, there are so few of them that [not worth?] considering today. The geniuses who can just discover something alone, everything is group. The initial idea is usually from an individual. The idea doesn’t come from a group, but the development has to be by a group. I still am convinced that it’s a very rare occasion when a novel discovery is made by a committee or a group of men. It’s always the idea of a single individual.

JM: But he doesn’t work alone [inaudible].

RA: No, but he doesn’t work alone. And you can’t. There are just too many different disciplines involved and you have to have help.

JM: So this is a matter of necessity, not a matter of-

RA: I consider it that way. It’s just absolute necessity today.

JM: Well now, you’ve sort of answered this question from the standpoint of the scientist. How are the scientists [but?] there are such people, there are administrators, you yourself have years of experience in administering. Do you find that it’s necessary to really administer scientists or is this such a highly individualistic, this research, such a highly individualistic matter that you merely select the right men and give them the right support [inaudible]?

RA: Well that’s the most important thing, to find the right man and give him the right kind of physical facilities and atmosphere in which to work. But in the academic field the scientist can be a little bit more individualistic than they can in industry. But the administrator there has to see that he does fit in and doesn’t disturb the other people too much and will cooperate in the teaching program at least.

JM: And produce results.

RA: And of course produce results. Well, I’m talking about the man who is able to produce results but produce results without interfering with the work of others and [inaudible] different colleagues. And that happens sometimes, you see there’ll be a man in a group and he’s always complaining or something and it just irritates the others so they don’t do as good work. That type of scientist isn’t so valuable.

JM: Selection of people therefore is very-

RA: Very important. For example, I know that industry would much rather have a man with less scientific ability in the laboratory but one who can get along with the rest of the people so they can work in a group then to have one or two really distinguished scientists but who can’t work with a group and they are always upsetting the whole laboratory.

JM: Have you found in your experience that there is really very much that the leader of the scientists can do about this besides selecting the right men?

RA: That’s pretty hard to say. Some of the characteristics of the scientists are innate and you can’t change them or they’ve been brought up under such circumstances that they had ingrained certain characteristics that can’t be modified.

JM: [Frankly I have one of those you’re getting at?].

RA: Oh really. So, you know I think that in a group effort the selection of your individuals is extremely important.

JM: You, with all your experience, and you would really say that there is not a science of managing scientists that is important beyond selection of the man.

RA: No, I believe you’ve got to do more than that because you have to see that the scientist is given the proper opportunity to advance and that his efforts are appreciated otherwise he doesn’t do as good work. And therefore, the director of the laboratory must be able, one, to talk with the businessman to persuade them that his effort under his general supervision and responsibility is worth supporting. And then he has to have the respect of the men under him. Now the men under him will not respect him unless he’s also a good scientist and also knows how to deal with the businessmen. So that the reason that we find so few scientists that are good administrators is that they don’t have that duel capacity of being able to understand economics and talk with businessmen and go with them socially and also have the full respect of the scientists down below who are not interested in business and are just interested in scientific results.

JM: Well, you certainly have a distinguished record of associating with and advising and keeping sole government leaders for example and I assume business leaders as well. Is there any special knack to this beyond what you just said.

RA: I don’t think so. The students who come to you to study, the men are [luck?] that you get somebody that is born with a lot of ability. For example, Wallace Carothers, who discovered nylon, took his doctorate degree with me back in 1923 or 4. And he came from a small college in Missouri that I’d never heard of. But as he studied in the graduate school and learned more, I considered that he was a most unusual man because he could do physical chemistry, mathematics, physics as easily as he could organic chemistry and he was also had mechanical ability. And so he was one of these very unusual people and very modest and quiet. Also interested in music and [labor?] problems and philosophy. He was quite an exceptional person. But I had nothing to do with him, with his coming except that he applied and we gave him an appointment and then I did encourage him of course and I helped him a great deal after he got underway.

JM: Well, you’ve always had very broad interests beside your chemistry. How can you explain.

RA: Well, it’s just-

JM: Starting way back now. Where did all this start with you, would you say?

RA: Well, I really don’t know. One just lives on and something comes up, you do it, and you get into something else and you do that. That’s the way I’ve lived. It’s more like happenstance.

JM: What would you say-

RA: It wasn’t organized.

JM: What would you say that you first started to come into contact with people pretty high up and you were able to interpret organic chemistry to them and stir their imagination and confidence in that sort of thing.

RA: Well, I think I developed quite a bit during the first World War when I was in Washington and all, most of the chemists came down to Washington and there I had contact with all of them and learned how to get along with people and later on it was easier. Before that time, I didn’t know so many.

JM: Wasn’t that a particularly fortuitous time. There weren’t’ very many then, were there?

RA: No, just in the hundreds, that’s all. Chemists.

JM: Got to know them.

RA: Yeah, got to know them all and really that’s what developed the American Chemical Society because prior to that time, to get two or 300 chemists coming to a American Chemical Society meeting, that was it. But after the war these people went rather systematically and methodically and maybe because they wanted to see each other, they’d been together for a couple years during the war and so they wanted to see their old friends. And that, the American Chemical Society started having larger meetings and larger attendance.

JM: Well, the Institute of Chemical Engineers was very small during that-

RA: Oh well during that period I’m not sure it existed.

JM: During the war? [Crosstalk]

RA: No, no, that came after the war I’m pretty sure. I’m not a member of the Institute of Chemical Engineers so I can’t say but my impression is that that didn’t come into existence until immediately after the war, the first World War.

JM: Did you ever have a feeling that the very fortuitous period that you lived through for getting your doctorate before 1912 or so at 1912 at the beginning of so much expansion-

RA: Well, of course it is fortuitous because I had no control over when I was born and no control over the fact there was a first World War and that the United States had to develop chemicals because they were cut off from Germany and needed these chemicals very badly. And I just happened to be around and had a chemical training at the time so that was surely just accident.

JM: So, the United States needed organic chemistry and you were-

RA: And I just happened to study organic chemistry in college. I went to college not knowing what I wanted to do. I started in medicine more or less and then I thought I’d go into mining and finally ended up by deciding on chemistry. I had no idea ahead of time what it was going to take and one of the things that probably persuaded me to go into chemistry was that when I graduated I was offered an assistantship in chemistry to go on into the graduate school and when I received that, that just made up my mind for me to go on. Then I’d had no intention of going into academic world until I took my doctor’s degree and then it was the custom in those days if

you were going to finish your education to go to Europe. So, I applied and went to Europe and got post doctorate training. Then I came back and I had no job and couldn’t get a job while I was in Europe so old professor C.L. Jackson who had retired at Harvard wanted a post doctorate assistant and offered me a place with him. And I was very happy to get it because 800 dollars in those days looked pretty good.

JM: A year?

RA: A year. So I accepted that position and before the year started in August, G.L. Kelley who was an instructor in organic chemistry at Harvard at the time decided to leave and go into industry and they were pinched to find somebody to give the course. And so they asked me to give one of the courses, the graduate course, and Jimmy [?] who was a professor at MIT was asked to come over and give the undergraduate course. And so that’s how I happened to get into teaching. The following year then they offered me a full time job giving both courses and so that’s how I got started in teaching and what I’ve continued through my life.

JM: Now what happened then between you and the development of the organic chemical industry in the United States.

RA: Well, nothing between me and the organic chemical industry particularly but they needed chemists and-

JM: You provided the chemists.

RA: We provided some of the chemists. One of my closest friends was [E.K. Ballton?] who was a student at Harvard the same time I was then went to Europe and studied with Bill Sturin and worked on the [kerring?] matter in petals, flowers. And then when he came back he was looking for a job, could have gotten an academic job incidentally, but DuPont was at that time deciding to expand into chemicals. They were previously in explosives alone and wanted to go into the dye fields and picked him as a man who would be good to start it up because he had done this matter on the [kerring?] matter in petals. Well, it shows you how little the administration of the DuPont Company knew about chemistry at the time. But that’s the story of how they happened to hire [Ballton?] because later he became director of the research of the company and it was under his administration when neoprene and nylon and a variety of other major discoveries were made in the DuPont Company.

JM: Well did you have consultantships?

RA: I started a consultantship with DuPont in 1928. They offered me a position to come, a very high position to come with DuPont in 1928 but I had by that time acquired some reputation in academic work and I decided that if I left and went into industry it would kind of look as though I was selling out for money and I was enough interested in academic work so I decided to be poor and stay in academic work.

JM: A matter of principle. Well have you had consultantships though with industry over the years?

RA: Yes, I’ve been with, I don’t have any further contact with DuPont because I dropped out last year on my own accord, but I was until last year a consultant from 1928 until then and I’ve been a consultant with Abbott Laboratories since 1917 I think and still there.

JM: 1917.

RA: At the time they were doing 300,000 dollars worth of business. Now they do,
I don’t know, 175 million or something.