

An Interview with

JAMES HENRY WAKELIN, JR.

OH 104

Conducted by Arthur L. Norberg

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Abstract

The interview covers Wakelin's career, including his education, work at the Navy Department and Engineering Research Associates (ERA), and later consulting work. The first part of the interview reviews his education at Dartmouth, Cambridge, and Yale, and his first job at B.F. Goodrich. He discusses his work in the Navy department during World War II, where he was involved with their first use of modern computers. Through this Wakelin came in contact with William Norris and others who founded ERA. Wakelin discusses his own plans to establish a consulting company after the war and his decision to join ERA. He discusses his work in ERA's Washington D.C. office from 1945 to 1948, where he was primarily involved with securing Navy contracts. Other aspects about ERA in the late 1940s are discussed, including his relationships with John Parker, C.B. Tompkins, and others. He concludes with a review of his later work with a textile institute affiliated with Princeton.

JAMES HENRY WAKELIN, JR. INTERVIEW

DATE: February 27, 1986

INTERVIEWER: Arthur L. Norberg

LOCATION: Washington DC

NORBERG: Dr. Wakelin, can I ask you first how you came to choose Dartmouth as an undergraduate institution?

WAKELIN: My family came originally from New Hampshire. In about 1892 my father was offered a position as organist in the Rollins chapel at a full scholarship, and his father wouldn't let him go because he said, "You're a businessman's son, and you don't have to go to college to be a businessman, so you're not going to go to Dartmouth College, even though it's not going to cost you a thing." So my father was prevented from going. I always thought since New Hampshire was a home family state that I would like to go to Dartmouth. That's why I chose it.

NORBERG: I see. What sort of business was your father and grandfather in?

WAKELIN: It was a department store in Nashua, New Hampshire.

NORBERG: What sort of course of studies had you intended when you went to Dartmouth?

WAKELIN: I didn't know when I first went, but I majored in physics when I was there, with one other in a class of 582.

NORBERG: Who was on the faculty at the time?

WAKELIN: Gordon Ferry Hull; Charles Proctor; Norman Gilbert; Joe Tanch; and others you may not know.

NORBERG: Hull and Proctor I recognize. You left there in about 1932?

WAKELIN: Yes, '32.

NORBERG: What were your prospects at the time? What were you thinking of doing; what could you have done that perhaps you didn't do?

WAKELIN: Well, I wasn't thinking about doing anything. I spent the summer in Europe -- the summer of '32 in Europe. When I was in England, Howland Sargeant, who was in my class at Dartmouth was chosen as a Rhodes Scholar. He was a great friend of mine and my family. He spent some time with my family when I was abroad when they were up in Maine. He convinced my father that I should go to Cambridge University while he was at Oxford. Sargeant recommended that I go to Cambridge and continue graduate work in physics there.

NORBERG: In 1932! What a fantastic year to be there!

WAKELIN: It was amazing. I was in Scotland at the time, and in September went down to Cambridge, and was admitted to Downing College.

NORBERG: And studied with whom?

WAKELIN: Well, I studied with quite a number of people. Rutherford was there and J. J. Thompson; also eminent physicists such as Blackett, Chadwick, Cockroft, R. M. Fowler, Walton and C.T.R. Wilson were doing research and teaching there.

NORBERG: Were there other Americans at the time you were there that you came into contact with?

WAKELIN: Yes, there were quite a few. Jim Fisk was there from MIT; he later became president of Bell Labs. He was there during one of my two years. I knew Jim pretty well; we spent Christmas vacation skiing in Austria.

NORBERG: You received a degree from Cambridge?

WAKELIN: Yes, a bachelor's degree there, and then five years later after keeping out of jail and so forth, I received a Masters degree in '39.

NORBERG: From there or from Yale?

WAKELIN: From Cambridge.

NORBERG: From Cambridge?

WAKELIN: You have to pass a comprehensive examination to get an honors degree. Then if you stay out of jail and have no criminal record, a Master's degree is awarded you five years later.

NORBERG: I didn't realize it was that automatic?

WAKELIN: Yes, it's automatic. You've already earned it by graduating with an honors degree in '34, but you don't get it until 5 years later.

NORBERG: That's when you came back to the United States in '34.

WAKELIN: Yes, I came back to Yale.

NORBERG: Had that been a design earlier, or was that as accidental as going to Cambridge?

WAKELIN: No, I tried in '35... after living with a German family in Austria to catch up on my German, I came back to the United States in December '34 and tried to look for a job around New England and New York, but I couldn't find

anything. So my father decided I had better go to graduate school. He knew Charles Seymour, who was provost at Yale. I went down to see Professor Seymour, and he took me up to the physics lab and said, "I hope that you can admit this young man." It was so easy, it was wonderful.

NORBERG: What was the program like at Yale in 1934?

WAKELIN: They were just starting to build a cyclotron that was left over from parts of E. O. Lawrence's cyclotron at the University of California.

NORBERG: Who was doing it?

WAKELIN: Ernest Pollard.

NORBERG: Did you work with Pollard?

WAKELIN: No, I didn't work in the cyclotron field, I worked in magnetism under Louis McKeehan.

NORBERG: What sorts of problems did you work on?

WAKELIN: I worked on surface magnetization.

NORBERG: Can you be a bit more detailing about your Ph.D. dissertation?

WAKELIN: Yes. I was trying to find out whether in the close proximity of two magnetic materials there was a magnetic flux from one penetrated the other and circulated around, and whether there was any external evidence of or measure of the magnetization on the surface of the material. I couldn't find any positive result with the equipment I had.

NORBERG: What sort of equipment were you using?

WAKELIN: A magnetometer that I built myself.

NORBERG: Based on what sort of design? Is it standard magnetic theory that you were using?

WAKELIN: Yes.

NORBERG: Do you remember the theory text that you might have been consulting?

WAKELIN: Yes, Francis Bitter's text on ferromagnetism.

NORBERG: How many were in the group?

WAKELIN: I was the only one working in magnetism under than Professor McKeehan. There were a number of other graduate students, about ten in number, but not in magnetism.

NORBERG: What was his principal field? I don't know his work.

WAKELIN: He came from Bell Labs. He's a magnetics expert. Spent a great deal of time in the field of magnetics.

NORBERG: It's interesting that you were the only graduate student working on magnetism while the other several students were not.

WAKELIN: That's right.

NORBERG: You left there in '39 and went to B. F. Goodrich. What gave rise to getting an offer from B. F. Goodrich? Had you sought it?

WAKELIN: No, they sent people on to interview those of us who were completing our graduate work in the physics department, so I decided I'd give it a try.

NORBERG: What sorts of people were they looking for?

WAKELIN: They wanted physicists. They wanted to build up their physics lab, which was rather small compared to their chemistry lab. So, I went to Akron in the fall of '39.

NORBERG: What were the early projects you worked on at Goodrich? Can I stop you for just a moment?

WAKELIN: Sure.

NORBERG: I'm fishing for relations that will be important at ERA and other places later on. That's the reason for going through all these early things. What sort of background you brought to the company, and therefore, why the company settled on certain things.

WAKELIN: Right. Well, I was put on to a problem of trying to study and learn more about the mechanism of vulcanization, from a physicist's point of view. So I was very active in the field of x-ray diffraction, actually, the x-ray diffraction equipment, which I used to discover what was going on during the period when you heated up a body with an accelerator and/or sulphur and so forth, what went on in the various stages of the reaction to the final completion of what they call vulcanization. So I unraveled -- people worked with me too -- I unraveled a whole category of reactions that were related to the business of getting the sulphur combined with the rubber in a quick reaction, which they call an accelerated reaction.

NORBERG: Were you part of a larger group doing this?

WAKELIN: No, I was working on this myself. I was working with some people on the chemistry part of it, but I was the only physicist working on this end of the problem.

NORBERG: Yes. What were the chemists working on at the same time?

WAKELIN: They were working on devices and chemicals that would accelerate the vulcanization reaction.

NORBERG: Did you people get to the stage of designing any sort of production equipment?

WAKELIN: No.

NORBERG: Was that turned over to someone else or was the project dropped when you left Goodrich?

WAKELIN: Well, they turned it over to people who were doing production modeling, small scale production modeling.

NORBERG: So you were in Akron all this time, I think I heard you say.

WAKELIN: Yes, four years.

NORBERG: Were you working on this problem that whole time?

WAKELIN: No, I was working on a number of other applied problems. One of the things we were interested in was what the Germans were doing with synthetic rubber. They set up a laboratory under my direction to find out what we could about captured equipment that was brought back from North Africa -- tank equipment, rubber tank treads. We

did a lot of x-ray examination of those in the production phase, and found out a great deal about what they were doing, which helped us create our own synthetic rubber program at the Goodrich Company.

NORBERG: I see. Was anything published about those developments, either in the normal scientific journals or in some description of war-time activity at Goodrich that you know of?

WAKELIN: I published with several others some papers on the theory of vulcanization, which didn't concern the war effort.

NORBERG: Did you publish these papers in journals like *Chemical Physics* or *Physical Review*, or something like that, on the theory of vulcanization?

WAKELIN: I have forgotten what publication that was in.

NORBERG: But it would be something like that. It would have been a professional society journal.

WAKELIN: Right.

NORBERG: Not a trade journal is what I was trying to distinguish.

WAKELIN: Right.

NORBERG: In 1943 or so, this project was still in full force. Obviously it was a scientific rubber program. It did not end even with the end of the war. How did you come to select to -- I assume -- volunteer for military service?

WAKELIN: Well, that is very simply explained. Tom Wilson who was a graduate student with me at Yale was in the Office of the Coordinator of Research and Development in the Navy department, but of Mr. Forrestal's office. He

wanted to go to the Pacific; he wanted to go to the war front. So he asked me to relieve him in this office. I went to see his admiral, Rear Admiral J.A. Furer, and he asked if I would come, so I did.

NORBERG: Were you already a reserve officer?

WAKELIN: No.

NORBERG: So you were taken in at that time.

WAKELIN: That's right.

NORBERG: And into the Navy for the first time.

WAKELIN: Right.

NORBERG: Then you worked for Admiral Furer for the remaining portion of the war?

WAKELIN: Yes, I was in that same office, that became the Office of Naval Research.

NORBERG: Can you tell me something about the office? How it ran; who was in it besides you; what sort of projects they were interested in?

WAKELIN: The Office of the Coordinator of Research and Development was principally the connection of the Navy department with Dr. Bush's organization in research and development.

NORBERG: OSRD?

WAKELIN: NDRC and OSRD. We were interested in finding out how they could help us in either doing research or doing testing on equipment and weapons in which the Navy was interested. My interest was in the rocket field. I was in rockets and underwater warfare. The NDRC did a lot to help us on various projects in these fields and many new weapons they helped to develop were used in combat.

NORBERG: Who was doing the actual work on these projects? Certainly not the Navy, I presume.

WAKELIN: No, Cal Tech was in the rocket field for us, under Charlie Lauritsen and then Willie Fowler at the Naval Ordnance Test Station, China Lake, California, in the Mojavi desert, and also scientists at the Harvard Underwater Sound Lab were doing a great deal to help us in submarine detection important to antisubmarine warfare.

NORBERG: Under whom?

WAKELIN: Professor Frederick (Ted) Hunt, an acoustics expert and very practical in his approach to research and development work. I knew him very well. He just passed away.

NORBERG: Did you visit these sites with any frequency?

WAKELIN: Yes. Of course. I spent a lot of time in California, and in Boston.

NORBERG: Doing what?

WAKELIN: Seeing how they were getting along on Navy projects and what the Department could do to help them.

NORBERG: Do you remember the transition from those laboratory situations to the Navy at any time during the war? Did they finish their work basically is what I'm asking.

WAKELIN: They never finished their work, but they spent a lot of test material into the war front, advanced service testing, they called it, against the enemy.

NORBERG: You mentioned to me earlier today that at least at one time you had some interaction with Navy security people on Nebraska Avenue during this time.

WAKELIN: Of course I knew Howard Engstrom very well, because he was a math professor at Yale when I was there as a graduate student. And I also knew him and his family in Maine. So it was quite natural that I should keep up my contact with him as a representative of Admiral Furer's from Mr. Forrestal's office. I was sent out there by Admiral Furer to find out what was going on, and what we could help them with, what the Secretary could help them with.

NORBERG: Without going into any classified material - I'm not trying to fish on that sort of thing particularly - what sort of projects were they working on that you felt your office could help them with, if any?

WAKELIN: We weren't sure.

NORBERG: But when you went out there to visit them, what did you find out?

WAKELIN: Well, we found out the most we could do to help them was to get them more money. Allocate more money from the Secretary's office.

NORBERG: I want to follow up on the statement you just made about Engstrom. Had you taken courses from Engstrom at Yale?

WAKELIN: No.

NORBERG: But you had known him before you went to Yale?

WAKELIN: No. I met him at Yale. But I met him through his family's friends at Deer Isle, Maine.

NORBERG: That's what I was trying to find out whether he went to Deer Isle, Maine, or...

WAKELIN: No. He went to visit his family. I didn't know him before I went to Yale.

NORBERG: Before you went out there for business at the Admiral's office, had you been in regular contact with Engstrom?

WAKELIN: Not in terms of Navy work, no.

NORBERG: How about on personal terms?

WAKELIN: On personal terms, yes. We visited each other back and forth with our children, wives, and so forth.

NORBERG: So that would be several times a year?

WAKELIN: Oh yes.

NORBERG: Through Engstrom, did you meet other people at Nebraska Avenue in that period 1943, 1944?

WAKELIN: Yes, I remember meeting Bill Norris, and Andy Gleason from Harvard, and some mathematician from Cal Tech; it's hard for me to drag these things up automatically.

NORBERG: I want to say Reed.

WAKELIN: No. Marshall Hall.

NORBERG: Yes.

WAKELIN: He was in the field of statistics, I think, but in combinatorial theory.

NORBERG: Did you become equally friendly with Norris and Gleason.

WAKELIN: No, I didn't know them well at all.

NORBERG: I have learned that in mid-1945 that there was some discussion about two different things: one of them was related to Navy needs in computation, and in fact, there was a memorandum which I've seen that was dated 9 June 1945. The Naval Research Laboratory, the Taylor Model Basin, and the Naval Ordnance lab all participated in the development of a memorandum called Professional Employees in the Naval Service. I have the idea you wrote that document, or at least you were responsible for its generation. Do you recall it at all?

WAKELIN: It might have been through a memorandum I wrote to the admiral about the fact that we should get a national Navy program in computation going, whether you've ever seen that memorandum or not.

NORBERG: I have. I'm coming to that next. Do you recall the circumstances about how you came to write such a memorandum?

WAKELIN: I was interested in some early work at the Bureau of Standards that was going on, and I just thought that the Navy needed to get to know what the computing equipment field could do for them, and what modern computing could do for them.

NORBERG: Why did you have that desire, or feel the need to do that sort of thing?

WAKELIN: I thought that the computing field was very interesting to me, and I thought the Navy ought to formalize its interest in it, and I worked out several agreements with the people at the Bureau of Standards to get together to form a program that would help the Navy in understanding computing techniques and how to use computing.

NORBERG: How did you learn about computing? What sort of projects did you know that were going on in the wartime period?

WAKELIN: I knew Howard Aiken pretty well at Harvard. And I followed his work with the Mark I computer. He was the one that sort of instilled my interest in computation. And of course, his interest was also in the Navy, because he was a Commander in the Navy. He had a Commander billet at Harvard as you know.

NORBERG: Yes. Yes. When did you first come into contact with Aiken?

WAKELIN: 1945.

NORBERG: Because these documents were written in the middle of 1945. So...

WAKELIN: Late '44...

NORBERG: Yes. Yes.

WAKELIN: It was building up slowly. I began to feel that high speed computation would add a great deal to the technology of the Navy in the weapons field and make training devices available for people that use complicated equipment.

NORBERG: One could argue that that's a very sophisticated position to take in 1944 and '45 on certain grounds.

There were many, many Navy people who knew absolutely nothing about computation. Just as there were many other people in both civilian and military life. How was it that you developed the interest?

WAKELIN: Farther back than that, I'm just trying to dredge down into my stalled memory system... I was the officer in the Office of Coordinating Research and Development in the Navy that was concerned with following the Applied Mathematics Panel of the National Defense Research Committee, and I was very active in finding problems that they could work on for the Navy that would help the Navy in operations. One of the problems that came up was a problem that Maurice Biot had asked me to look into for him with the Applied Mathematics Panel about Navy aircraft wing flutter. The problem concerned Mathieu functions--and non-periodic, non-linear functions. So I was interested enough to generate a memorandum describing why it was important for the Navy, what it would serve if the Applied Mathematics Panel came up with the required table of Mathieu functions, and how it would help Commander Biot and the Bureau of Aeronautics solve the problem of wing flutter. I had to give a reason for everything I wrote to Dr. Bush's group on, because they wanted to know, they didn't want to work on things that didn't have a real Navy interest and objective.

NORBERG: That I know from other circumstances is quite correct. The Applied Mathematics Panel, you mentioned a few moments ago too that...

WAKELIN: I guess the connection I had with Harvard followed the AMP business, because we were only interested in generating tables of functions. We couldn't compute them with computing equipment then, so we were interested in documenting tables of mathematical functions to use. Now of course you just calculate them.

NORBERG: Do you think that you met Aiken through the Applied Mathematics Panel?

WAKELIN: Yes I did.

NORBERG: I think he served on it toward the end of the war.

WAKELIN: Yes.

NORBERG: In fact that was a very important panel as you well know. But you also said that -- if I understood this correctly -- that this activity of yours in the Navy, and possibly your own stimulus, was responsible for the National Bureau of Standards developing a program in computation.

WAKELIN: I'm not sure it was responsible. I think we came up with the same idea simultaneously, and -- I have forgotten the fellow's name that was there, he was a very nice guy, we had some very serious talks together about how we could combine our efforts to produce some activity in the computing field that would help the Bureau of Standards and the Navy Department. I was off on another tangent for a minute there... I was going to review for you my responsibility toward the end of the war delegated to me by Admiral Furer. The NDRC and Dr. Bush decided in the early part of February or March -- '45, to fold up the NDRC. Admiral Furer said, "Now, would you review all the Navy work done by NDRC, and tell me what work should be continued with universities, and in what fields?" So, one of the important assignments I had was to look into all the projects that we had with Harvard, and Cal Tech and the other universities that we were working with, to keep their interest in and concern about Navy problems. So, the mathematics field came up automatically as the sort of field that's been very useful during the war. How do we keep this research going after the war ended? That's one of the reasons for that memorandum.

NORBERG: That memorandum though, on the face of it, doesn't play any role it seems to me, necessarily at least, in development of something like ERA, unless you combine it with this document called "Professional Employees in the Navy." Those two things I found almost together in ERA records, and attached to letters that you had sent out to other people. One letter I'd like to remind you about in all of this, was sent out...

WAKELIN: Your history is better than my memory.

NORBERG: That's the reason for checking these things against contemporary records to make sure we all know what

actually happened. On 16 June 1945 a letter was sent by Hedly Crabtree to Graham Friese Greene. And that letter to Greene talks about Wakelin's plans as if the ERA design was your idea. Wakelin's plans for an independent company to do business for the Navy.

WAKELIN: The company referred to was not ERA, but a company to do research and contract work in the fields of chemistry, physics and engineering. I discussed that quite a bit with Hedly, because we were students at Cambridge together.

NORBERG: I didn't know that.

WAKELIN: He was a student friend of mine from Cambridge University. He was a very close friend of mine in England. And Graham Friese Greene's father claimed he invented motion pictures, before our friend Mr. Edison.

NORBERG: But, what was Hedly Crabtree doing? Was he in business in the United States or Great Britain?

WAKELIN: No. He was just a very close friend of mine. And of course Graham Greene was a very close friend of mine.

NORBERG: These people seem to be in a position to invest in a new company, it seemed to me by reading the letters. Did I misunderstand that?

WAKELIN: They would have been in a position to do it, yes.

NORBERG: They would have? That's what it seemed to me to be the case.

WAKELIN: But ERA wasn't the only thing I was interested in. I was interested in gathering a few people of my own to form an R and D company and consulting group, and that's how the whole ARC came into being after moving into

ERA.

NORBERG: Can you be more specific about that for me? I don't know the details of that. Two things there: we'll talk about ARC later on.

WAKELIN: That's an analogy.

NORBERG: Yes. Yes. I appreciate that, but in this sort of thing they're calling Wakelin's plans...

WAKELIN: It didn't concern ERA. It did not, because I was interested in forming a company with some people that I had known at Princeton, and some people that I had known in the Navy, to do something quite different than computing. So it didn't concern ERA at all.

NORBERG: Well then, what's the distinction between the planning for a company like ERA, which I assume also came out this...

WAKELIN: The thing was, they had an operating group that they could translate into some company or get some financing to build a facility to keep the group together. I didn't have a group to keep together. I was just assembling one.

NORBERG: If that's the case, then what you're telling me is that people like Engstrom and Norris and Meader were trying to establish a company independent of what you were doing in the Navy to say that professional employees need to be retained in order to exploit computation ideas and so on. Those two things were separate.

WAKELIN: Right. Quite separate.

NORBERG: Did you talk to Engstrom about these things, either their plans or your plans?

WAKELIN: Yes, I did.

NORBERG: Very early? Say in the same period?

WAKELIN: Yes, very early.

NORBERG: What sort of help did you offer to give the Engstrom group? Let me call it the Engstrom group for shorthand.

WAKELIN: In what context?

NORBERG: In helping them to find a backer.

WAKELIN: Well, I was looking around among the people in the financial field -- very a few of them I did know in the large operating financial groups -- to find out who I knew that I could call on to talk to Howard and Bill Norris.

NORBERG: Do you recall who you thought about to send the information to?

WAKELIN: I can't right now, no.

NORBERG: Was Ralph Damon one of those people, president of American Airlines at the time?

WAKELIN: No.

NORBERG: There was also a letter to the Socony Vacuum Oil Company in the same period. That got a response directly to Engstrom, so he may have sent the original letter.

WAKELIN: I think he probably did.

NORBERG: There was a whole list of companies -- I don't have that list of companies, which went out in November 1945: C. Russell McGregor to Ralph Thompson. Do those names mean anything to you?

WAKELIN: No.

NORBERG: I think that McGregor was in one of these companies like Socony Vacuum. Did you think to approach Goodrich at all?

WAKELIN: No I didn't.

NORBERG: You did not.

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NORBERG: Let me take a moment so I can understand what has been said.

WAKELIN: Have I clouded your whole historical record?

NORBERG: Nope. Not at all. What you've helped to clarify is that I was seeing these two things; that is, your activities in the Admiral's office and Engstrom and Norris and their activities as somehow trying to do the same thing, trying to develop some sort of context for generating an ERA contract.

WAKELIN: I remember that. Howard kept saying to me, "If your plans to get your people together don't work, why

don't you think of joining Bill and me."

NORBERG: What was your knowledge in the end of 1945 about their success with approaching people? Did you know when Parker came into the game?

WAKELIN: I did, yes.

NORBERG: You did. Do you remember when that was?

WAKELIN: I think it was the end of '45.

NORBERG: Is that something you knew afterwards, or something you knew at the time?

WAKELIN: Something I knew at the time. Because Howard discussed this with me, and said that there was a possibility of help from a man by the name of John Parker.

NORBERG: But you had knowledge of Parker before that?

WAKELIN: No I didn't. I thought he was a partner in the firm of Auchincloss, Parker and Redpath, investment people here in Washington. But I guess he was partly connected with them.

NORBERG: What happened to you, then, at the end of '45? What plans were you making?

WAKELIN: Well, I wasn't making any plans. During the spring of '46, I had a number of offers I was considering. One was to go to Yale as an assistant professor, spending half time in the Astronomy Department and half time in the Physics Department, which I turned down. Then Phil Morse was interested in getting Brookhaven started, and wanted me to come up and join him at Brookhaven on Long Island, which I also turned down. Because I had this

idea of starting this company of my own which we could all enter into with joint ownership in the research and consulting field.

NORBERG: How did those things develop? Who did you approach?

WAKELIN: I went to see a few people in New York that I knew in the financial field, Dillon-Reed, and so forth. They were aghast at my plans.

NORBERG: What did you approach them with? Were there specific details that you were telling them what you and your group were going to do?

WAKELIN: Yes, there were.

NORBERG: Can you tell those?

WAKELIN: We were going to work in the field of physics and chemical engineering, and we had some projects in those areas to work on, and I wanted to do some work in computing, too.

NORBERG: And they weren't buying it?

WAKELIN: Well, they didn't think it was a very smart thing to do.

NORBERG: Did they say why?

WAKELIN: I said we were going to pay ourselves very low salaries, because we wanted to spend a lot of money getting going. Then they said, "Well, what do you mean low salaries?" And I said, "Well, I'd like to get 10 or 12 thousand dollars a year.

NORBERG: That was pretty good.

WAKELIN: In '45.

NORBERG: Not necessarily a small salary by those standards.

WAKELIN: But the people in New York thought it was too small.

NORBERG: That's interesting.

WAKELIN: You wouldn't want to sell yourself for that salary just to get going? And I said, "Yes, I'd consider it."

NORBERG: You didn't get the backing, I assume, because the company was not formed.

WAKELIN: That is right.

NORBERG: How did you come then to join ERA?

WAKELIN: I didn't have anything else to do, so Howard said why don't you come with us. I said I'd come if I could head a Washington office for him, because I wanted to stay here in the Washington area, and be in the Washington group here to interface with the Navy Department and other organizations.

NORBERG: There was already a Washington office there at that time? Was there not?

WAKELIN: Yes. That's right.

NORBERG: Who was in the Washington office?

WAKELIN: A friend of Johnny Parker's was head of the office -- Jim Clifford, a lawyer.

NORBERG: I think of Jim Miles, but he came to the company later. Who else? Was Engstrom there, or had he gone to St. Paul?

WAKELIN: He was there part of the time; he was mostly in St. Paul.

NORBERG: Was Norris there still?

WAKELIN: He was in St. Paul.

NORBERG: Was the office downtown?

WAKELIN: On Vermont Avenue.

NORBERG: On Vermont Avenue still?

WAKELIN: Yes.

NORBERG: Was Tomash there at the time?

WAKELIN: Yes. Charlie Tompkins was there. Rubens was there.

NORBERG: That's interesting. Are you sure Rubens was there, or was Rubens still at NOL?

WAKELIN: No, he joined us after he left NOL. What do you mean, was he there at the start?

NORBERG: No, when you went there. It is my understanding that he was at NOL until he...

WAKELIN: No, I think he came after I did.

NORBERG: Yes, because it's my understanding that he went right to St. Paul when he left NOL. There was a three month overlap between when he had the offer from ERA and he actually came on board.

WAKELIN: He was back and forth from St. Paul to the office.

NORBERG: Yes, that was frequent with a number of those people, right. Did you join that office before the contracts came from the Bureau of Ships and Naval Research, which is June of 1946?

WAKELIN: Oh no, I didn't join them until September.

NORBERG: So you were not there.

WAKELIN: I spent that summer in Maine.

NORBERG: You had already left the Navy by that time, I assume?

WAKELIN: I left in the middle of July.

NORBERG: Middle of July? Then in September there were already two major contracts under way with the Navy, plus all those other things that I mentioned at lunch that Parker was bringing in to keep cash flow going. What exactly were your responsibilities in the Washington office? I'd like you to think of that in two senses: one, the

Washington office as an operation, as an organizational part of ERA; secondly, your own responsibilities within that office. Why don't we take the larger one first. When you joined them, what was their task in that office?

WAKELIN: What did they task me to do?

NORBERG: No. That's the second part of my question. What were the responsibilities of that office?

WAKELIN: I'm not sure I understood it completely when I joined them. The office was to keep a very close contact with the Navy Department in view of the starting of the contracts they were engaged in. I wasn't concerned with that at all. I was asked to come into the office to be part of a group that would represent ERA in Washington, but not necessarily with the departments of the Navy and other things. I was supposed to go in and... I was really using that connection in ERA to set up an office that I could operate in myself with a number of people like Art Sloan and Art Scurlock, who joined because I was there. This sort of became a nucleus of mine in a sense. A little bit remote from ERA -- the computing field -- but we were forced to find contracts of our own to keep our own organization going. So, a number of us, except those that were working in ERA in the Washington office, and had connections with St. Paul, most of us were out looking for other work to do, such as the Boom contract.

NORBERG: Can you give me a couple of examples of the types of contracts that you and your smaller group were able to obtain, because I'm interested to know how the specs were developed for those contracts?

WAKELIN: Well, one of our principle jobs that concerned Scurlock and Art Sloan and Roy Bryant and George Vaux and myself was a Navy project at Princeton called Project Squid, which was concerned with the research and development ongoing in the nation in jet propulsion. We were tasked to go around the country and find out all the people and organizations working, either academic or corporations, on jet propulsion devices to see what they were doing; what they intended to do in terms of jet propulsion mechanisms, and how they looked at the future of jet propulsion. So we prepared over the next couple of years that I was there a field survey report on jet propulsion in the United States, which was Project Squid in Princeton.

NORBERG: Squid was funded by the Army was it not?

WAKELIN: No. By the Bureau of Aeronautics.

NORBERG: So, this was a sub-contract to ERA?

WAKELIN: That's right.

NORBERG: Now, in that circumstance, the Project Squid people would have understood what they wanted to know, so they were just giving you a job to do basically, is that correct?

WAKELIN: Yes.

NORBERG: The reason I'm asking those questions is because when it came time to develop something like Atlas, that there was a large interchange, an interaction between the Navy people on the one hand, on Nebraska Avenue for the most part, and the ERA people. There was discussion back and forth until the specifications had been developed, and in fact there were some people in ERA who believed the Navy's role was very substantial, others who don't want to see it quite that way. I'm trying to sort that out. That's the sort of thing I was looking for in the contracts that you were dealing with, but it's clear in the case of Squid that would not be true. If you were set up sort of independently in the Washington office, so to speak.

WAKELIN: Highly independent, yes.

NORBERG: What was your interaction with people like Norris and Parker, and Meader for that matter?

WAKELIN: We had a very strong relationship with Johnny Parker, because he went up with us to Princeton to

formalize the contract with the University. He was interested in seeing us develop into other fields than just computing at that time, and the Army contract, the explosive equipment contract that we had with the Army; Jim Fitzgerald had led that effort. Johnny Parker was interested in a number of other fields that we were in, and formalized with Dean Taylor our contract to undertake Project Squid and other contracts with the Army. Johnny Parker was very helpful to us.

NORBERG: This suggested that you worked together closely in a sense.

WAKELIN: Not particularly with the computing part of ERA, but with other parts.

NORBERG: I meant with Parker in this instance. What sort of a manager was Parker?

WAKELIN: I'm not sure he was a manager at all.

NORBERG: Then let me ask the question differently: what did he do for ERA, besides provide the financial wherewithal in the beginning?

WAKELIN: He provided the financial foundation for ERA and also the plant foundation for ERA in the form of Northwestern Aeronautical Corporation in St. Paul. But when things began to get into motion in the company, his particular value was in keeping contact with the contractors that we had to work with as a business man. But he didn't really do very much, I don't think, in organizing, or improving the organizational structure of ERA. He left that to the people in St. Paul. What little effort I had in Washington, which was a very small office then, no management at all. I remember that Bill Norris was the principle organizational man in the group in St. Paul. Howard didn't do much organizational management, although they all got together, Ralph Meader and Bill Norris, Howard Engstrom and Johnny Parker, usually on problems that required a group solution. But John wasn't always there in the management structure saying do this, that, or something else without having a group meeting with Norris and Engstrom.

NORBERG: Did you mention a Paul? Paul who?

WAKELIN: No, I shouldn't have. I didn't mean to.

NORBERG: Because I don't know any Pauls.

WAKELIN: Neither do I.

NORBERG: I think that's what I heard.

WAKELIN: Well, you did; I don't know why that name came up.

NORBERG: Did you have to report then, to say Engstrom or Norris, or did you report directly to Parker?

WAKELIN: We reported to Parker.

NORBERG: So, in spite of the fact that you were part of the Washington office, there wasn't any chain of command that needed to be reported through?

WAKELIN: No. No. There were people like Charlie Tompkins who was there, but mostly connected with the ERA work in Minnesota, St. Paul. He was there because he wanted to do his work there for St. Paul.

NORBERG: He wanted to do his work there in Washington?

WAKELIN: Yes. He could have done his theoretical work just as well there. He was a genius, a true genius. We enjoyed having him there.

NORBERG: Did any of your earlier contacts in the Navy or the military generally prove useful in the ERA days, either to you or to the others?

WAKELIN: I think they did because the contract we had formalized with the Office of Naval Research was with Mina Rees, who was formerly with the Applied Mathematics Panel, as you probably know, and she was director of the mathematical group in the Office of Naval Research. I knew her through the AMP. I don't think I was the reason for it, but she was very much impressed with Engstrom and with Charlie Tompkins, and thought they could help her in getting a book started on the whole computing field of that era.

NORBERG: In your *Who's Who* listing, it gives a title for your period with ERA as director of research in Washington. What did the title director of research mean here? You just described a situation where you were in a more confined area of the company. I would interpret the title director of research to mean some broader responsibilities for...

WAKELIN: I didn't have anything to do with research in St. Paul. They quite often brought down problems for all of us to look at that could help us. Rubens would bring down problems for us, too, to look at and say, can you help us with this, can you help us with that.

NORBERG: Did other people then report to you at the Washington office?

WAKELIN: Yes.

NORBERG: Who?

WAKELIN: Art Sloan and Scurlock and Roy Bryant, for particulars, reported to me.

NORBERG: Were they working on Squid or were they working on other problems?

WAKELIN: They were working on Squid and on other problems of a chemical nature.

NORBERG: Were the other problems connected to computation at all?

WAKELIN: No.

NORBERG: Was there more than one director of research then?

WAKELIN: I think there was. I don't know.

NORBERG: Because there are no organizational charts for this period, so you don't see this sort of thing.

WAKELIN: I know it. I think when Howard asked me come, he said you will just be director of research.

NORBERG: Well, it's nice in a small company, but look at it from my position 40 years later and trying to understand what those titles might mean.

WAKELIN: Exactly.

NORBERG: In terms of the development of the organization itself. In 1948 you left ERA. Was that because you were still thinking of starting your own company?

WAKELIN: No. I was thinking that Princeton would be a good place to bring up my boys rather than Washington, and I wanted to get out of Washington for awhile. In fact for good, if I could help it. No, I was very much intrigued with going to Princeton because of John Dillon whom I knew at Firestone when I was at Goodrich. He was director of physical research there, and I was in the Goodrich lab, I didn't have any big title, I was senior physicist or something

like that. Dillon went to Princeton to be head of the Textile Research Institute. I happened to meet him at the Princeton Inn when I was driving home from Maine, and he said he had just come up to see Hugh Taylor, Dean of the Graduate School, who was also interested in supporting the Textile Institute with graduate students at Princeton University. So Dillon said, "Why don't you come up and join me?" And I said fine. My title-to-be was Associate Director of Research.

NORBERG: What was the fascination with Princeton? It seems to me there might be many towns you could bring up the boys in?

WAKELIN: We didn't live in Princeton, we lived in Lawrenceville, which is very small, a town of about 1500 people. It was a good place for the boys to grow up in because the Lawrenceville School (private) offered a very fine educational program and facilities through junior high and high school.

NORBERG: But still, that doesn't answer the question. What's the fascination with that region? Did you want to join the Princeton faculty, say? Lots of friends in the vicinity?

WAKELIN: No, I didn't have any friends up there at all. I just thought it would be a good place to settle down and do some research of my own. Very simple. No overreaching sort of implication at all.

NORBERG: I'm also fishing here to see whether or not there's any connection with ERA, because '47 and '48 were essentially good years for them; the best years they had, to be quite blunt. Then, in '49, '50, obviously, it began to go downhill for lots of reasons, not all connected with them.

WAKELIN: Tell me more about that, because I wasn't there then.

NORBERG: I was thinking of it in terms of gross receipts and in terms of profit.

WAKELIN: But their scientific work and technological work kept going pretty well.

NORBERG: Yes, but it was not clear in 1948 that was going to happen. They were -- I'm speaking now just -- of the computation group. They had the contract to do the survey that resulted in the report and the book on *High Speed Computing Devices*. They had in 1947...

WAKELIN: I don't think anybody looked at that in ERA.

NORBERG: Which, the book?

WAKELIN: Yes. Charlie Tompkins was the one who outlined the whole text. He left to go to George Washington University, and I took his place with the work and it was completed and published by McGraw Hill Book Company.

NORBERG: Completed the work on the survey?

WAKELIN: On the survey.

NORBERG: Which was then submitted to Mina Rees. Then it was she, as you know, that suggested that it ought to be published in book form for everybody else, but that was two years still in the making. In 1948, two things happened in ERA, that I think are highly significant for the '49, '50, '51 period: one of them it seems, people in the Navy became very interested in particular machines like IAS and Whirlwind, the Princeton machine and the one at MIT, and decided maybe that there was something in this for them. They then promoted the idea for the Atlas or what became the Atlas. That was one thing. But that contract wasn't let until very late '47. The second thing that occurred, I think around the same time, was an interaction with the National Bureau of Standards, which resulted in another contract, which is where Engstrom and Tompkins played a big role in defining many different kinds of machines based on word length, principally, and using a magnetic drum for memory. I don't want to deviate too much into that, but that came about through a misunderstanding. The Air Force wanted one thing and the Bureau of

Standards wanted another thing and ERA wanted to do something else. They were all talking by each other for a long time, but then they finally got a product out of this. In that period, it's clear that the Washington office is playing a substantial role as a mediator between St. Paul, which is doing some sort of design work -- lots of different kinds -- and the military, principally the Air Force and the Navy are interested in having machine designs. By the middle of '48, those designs are fixed; Atlas is fixed; the NBS paper computer has gone somewhere else. The NBS decided they were not going to wait for ERA to build it and they'd go out and do their own building. It's at that point that ERA begins its downhill slide from the financial side. They're technologically important in the development of Atlas and the 1101. What I don't need to ask you about, because I'm not sure you know anything about it anyway, is where the ideas for Atlas came from. I mentioned the Navy people were interested in the MIT and the Princeton machines. The people back in St. Paul were taking the reports that the Navy were sending to St. Paul and picking and choosing among the different techniques available from those reports, and then sort of ginning up their own design.

WAKELIN: You mean the contractor proposals of the Navy?

NORBERG: No. The reports of progress of the IAS machine and the MIT machine.

WAKELIN: That's what I mean.

NORBERG: Yes. Yes.

WAKELIN: But from the contractors?

NORBERG: Oh yes. I'm sorry. The progress reports of the contractors rather than the contractors proposals.

WAKELIN: But that was privileged information.

NORBERG: Yes, but the Navy was circulating this quite regularly with the knowledge of the contractor.

WAKELIN: I see what you mean.

NORBERG: No question of that. There are records of site visits by both sides in each of those cases, both ERA and the other contractors. So, there was a general sharing of information. That's not an issue. The Washington office itself in 1948 seems to be playing some sort of critical role. It's at that time you left. Did you sense that significant role or not or did it not matter, even if you did sense it?

WAKELIN: That's difficult to say. I don't think I did sense it at all. I sensed a growing encroachment on what I felt was my contribution to ERA and the Washington office by Johnny Parker and I talked with them at length about it, and we each got mad at each other about it and so I said, "Well, I don't think you need me anymore Johnny." And he said, "Well, I guess I don't."

NORBERG: What was the point of contention between you and Parker?

WAKELIN: The point of contention was who was going to run the scientific end of it. Was Johnny going to dictate to us what we should do or was I. I felt I could be a great help to ERA if I was left alone to decide these things on my own. Not that I had any better ideas or judgment than anybody else, but I had a feeling for the scientific end of it. I was very close to Charlie Tompkins, and when he left I felt the very intellectual core of ERA had gone.

NORBERG: I have in my notes an organization plan in December of 1947. This is what I gleaned from documents. Under a vice-president for engineering research, who was Bill Norris, there was a technical committee made up of Norris, Tompkins, Howard, Steinhardt, and Wakelin. Under that committee there are five directors of one kind or another. The director of research was Tompkins. Your title then seemed to be director of consulting services. Do you remember that?

WAKELIN: No. I don't think I ever knew it.

NORBERG: There was a description of these offices in October of 1946 in which all of these things are defined. I haven't found that document so, what a director of consultant services is, I don't know. In this period, Tompkins leaves too. Did Tompkins say why he was leaving?

WAKELIN: I never knew why he was leaving. He never told me.

NORBERG: He never told you.

WAKELIN: I remember he went to GW.

NORBERG: GW. But, but...

WAKELIN: Then he went to California after that, rather quickly. University of Southern California, I think.

NORBERG: So the Washington office is falling apart, in a sense, in the middle of '48.

WAKELIN: I had a feeling it was falling apart, but I couldn't tell you why.

NORBERG: But I sense from what you say, Parker had a role in this. How about Norris? Was Norris making demands of the Washington office?

WAKELIN: No, he wasn't. No. Neither was Howard. Bill wasn't making any demands. Not that I knew of. He may have been making demands that weren't fulfilled.

NORBERG: Do you remember this Technical Committee at all?

WAKELIN: I remember it was set up, but I don't remember if we had any meetings.

NORBERG: Because there was then a director of engineering, who was Norris; director of laboratories, who was John Howard; director of special projects was Steinhardt and yourself and C. B. Tompkins. This looks like, on paper at least, a fairly substantial organization. I would see it as rather hierarchical, and therefore I would have expected you to be reporting to someone other than Parker. For example, I would expect some sort of reporting to Bill Norris. But you said a little while ago that you did.

WAKELIN: No, I didn't.

NORBERG: You reported only to Parker. I think you clarified a number of things, but I don't know what I'm going to do with it though.

WAKELIN: Well, neither do I.

NORBERG: Given that it is not really clear that there are records for this sort of thing, when an additional organizational plan shows up a year later -- by the way you are director of research -- so that must be after Tompkins is gone -- because he's...

WAKELIN: Well, frankly, if you want to look at a director of research of ERA in its full sense, Charlie Tompkins should have been.

NORBERG: Apparently he was in the earlier period, but by the beginning of '48 he had become a technical advisor, that's what it was.

WAKELIN: It was very hard for anyone to work with Charlie, because he's so damn bright. He is so much of a genius you can't work for him. I only used him to help me understand things I couldn't understand myself, you

know? I was very fond of him; he's a very fine guy.

NORBERG: Arnold Cohen said a similar sort of thing, that he went to him with that sort of problem he couldn't solve himself, that it required somebody with a different approach.

WAKELIN: With a superior mind. I think he had a very superior mind.

NORBERG: I just want to ask you a couple of more questions, not about ERA, unless you can think of things that I haven't mentioned. About the Textile Institute, what was it? How many people were involved? What was Princeton's association with it?

WAKELIN: Yes, it was a very interesting organization. It worked for the textile industrial field. We had company members, [from companies] that supported the research, doing basic research. There were about 50. I came as associate director of research and then I became director of research, and then I set up my own consulting business in Princeton, and became a research associate, where I spent half the time at the institute and the other half at my own business. I still had the urge to be my own boss, you know. The reason why it was so interesting to me was our connection with Princeton University through Dean Taylor. He was the chairman of the Chemistry Department and dean of the Graduate School, and a very, very important figure in Princeton. A very senior guy intellectually and management wise. We were very fond of each other, and he was very kind to me when I first came there. He was the one that engineered the connection between Princeton and the university by saying, "We'll have graduate students in the various departments elect whether they want to do textile research as their thesis. If they do, you and Dr. Dillon will be on their examining committee. You will report to Princeton University..."

END OF INTERVIEW