ORAL HISTORY INTERVIEW
OF
JOHN B. WACHTMAN

February 4, 2010

MR. LIDE: Good morning. We are in the Standards Alumni Association Office in the Administration Building of the National Institute of Standards and Technology which will be referred to as NIST throughout this interview. It's February 4, 2010. This interview is part of the oral history series conducted by the Standards Alumni Association in conjunction with the Information Services Division of NIST. Today we're interviewing John Wachtman who had an active career in the Material Science area of the National Bureau Standards, the predecessor of NIST. The others taking part in the interview are Edwin Fuller, Sheldon Wiederhorn, who has not arrived yet, but will hopefully come in shortly, and Hans Oser.

I'd like each of these to say a few words in order to help the transcriber identify voices. So I'm David Lide, I'm currently in charge of the Standards Alumni Association Oral History Program. I spent thirty-four years at the National Bureau of Standards and NIST as a research physicist and manager of the Standard Reference Data Program. Ed, do you want to identify yourself?

MR. FULLER: I'm Ed Fuller and it's my pleasure actually to be here because when I first came to NIST, Jack was my supervisor two levels up. So, he's somebody who I've always admired and it's a real pleasure to be here and to be able to interview him.

MR. LIDE: Hans?

MR. OSER: I'm Hans Oser; I'm actually the sound technician here. My encounter with Jack Wachtman was actually during the NBS graduate schools days. He was a student of mine and later I happened to be on the committee when he got his PhD from the University of Maryland.

MR. LIDE: I meant to say that I've known Jack even longer than Hans; we were freshman together at Carnegie Tech many years ago. So Jack, I'll now turn to you and ask you to give a brief resume of your career at NBS and NIST and beyond that.

MR. WACHTMAN: Thank you David. I'm John Wachtman, but I'm always called Jack. That's been my lifelong nickname.
I spent thirty-two years at NIST starting in 1951, but I'd like to say something as this interview proceeds about the rest of my career as well. I went on to thirteen years at Rutgers University and then several more years as a part-time work, and all of it has a tie to NIST, even the period afterwards. So I will discuss the whole matter as it seems appropriate here.
Another aspect I want to address is the progression of roles. I started out as a bench scientist. Also a project leader, so it was very much hands-on personal research. As time went on, I made a transition more and more into management and wound up as Director of the Center for Materials Research.
I would say my NIST career was roughly in two equal parts. One predominantly personal research and the later half predominantly management and then the later part of my career after NIST involved university teaching and management of a research center there, as well as being the editor of a technical journal and writing textbooks. But it all tied into my experience at NIST and so I hope we can bring out some of that as the interview goes on.
MR. LIDE: Alright, so Ed, do you want to start with any questions you have?
MR. FULLER: I guess I've always been intrigued about when you first came to NIST. What really brought you to NIST and how did you find your way here and your early career here?
MR. WACHTMAN: Well, I came here because I needed a job. You know it was not a grand design on my part. I was a scholarship student as an undergraduate at Carnegie Tech, now Carnegie Mellon University in Pittsburgh and I stayed on there on an ONR contract working as a graduate assistant and a research assistant. I was there about three more years and was on my way to a PhD or so I thought.
I had gotten a masters degree fortunately along the way and then my father had a stroke and my mother was there at home with a sister about three years younger than I was and with a brother about eleven years younger and needing support so I needed a job and I inquired around and found one that seemed to fit the

work I had been doing. It turned out to be at NBS in mechanical properties. Professor Koehler, under whom I had been working, was leaving to follow Fredrick Seitz to the University of Illinois.
So two events precipitated my leaving. One was my father's illness and the other was that I would've had to follow Koehler to Illinois and in a sense start over again, and I simply wasn't in a position to do that.
But through the contacts and the Navy program that I was working on, I heard about NIST or NBS as it was then.
So I wound up at a place and in an area of work in a way that illustrates what a role chance and circumstance play in our lives. The job happened to be dealing with the mechanical properties of ceramic materials, a field very little worked on from a fundamental point of view at that time. As you get into a field and as you know more and more about it you become in a sense self trapped, because it's to your advantage to stay in a particular field.
So Ed, it was basically a set of circumstances that lead me here and I have always been glad that I came.
MR. FULLER: That's interesting because when I was at Illinois, I worked with Andy Granato, who you may have known.
MR. WACHTMAN: I know his name.
MR. FULLER: He worked very closely with Koehler.
MR. WACHTMAN: Yes.
MR. FULLER: In fact, the circumstances might have brought us together in a different life if we hadn't met here.
MR. WACHTMAN: Yes.
MR. FULLER: So it's sort of interesting.
Should we stop now and let Shelley introduce himself? Shelley just came in.
MR. LIDE: Yes, Shelley Wiederhorn has arrived and I'll ask him to just say a few words so his voice can be properly calibrated. Shelley.
MR. WIEDERHORN: Oh hi, this is Shelley Wiederhorn. I'm glad to be here to have a chance to hear Jack's story and to interact with him in this little adventure.
MR. LIDE: So do you have a question?
MR. WIEDERHORN: Not right now. I'm sorry I'm late, but the traffic was bad. I don't know how Jack got here on time. That traffic on the beltway was awful.

MR. WACHTMAN: Well, I didn't come by the beltway. I came out 355, but I wasted about twenty minutes because things have changed getting from 355 over here. I miscued a couple of times, but I eventually made it.
MR. WIEDERHORN: Yes.
MR. WACHTMAN: And David's email about where to park and how to find this place was very useful.
MR. LIDE: Good. Well let me just come in with a question. In some previous discussions you had mentioned several of your mentors at the NBS in your early years. Do you want to say something about the people who helped you and influenced you most?
MR. WACHTMAN: I do, indeed. That's a very important subject and I feel a lot of gratitude to a long list of people at NBS/NIST. The people who influenced me most at the beginning were my section chief and my division chief. We had sections in those days. A man named Roman Geller, who was a ceramic engineer, and a good one, was my section chief. I think that strangely he had a tie to Pittsburgh too. I believe some of the early NBS work was done in Pittsburgh. I haven't checked the history on that. And his boss, the head of the Mineral Products Division, as it was called in those days, was Herbert Insley. They were both good men, real gentlemen and technically able. I was fresh from university research and I must have needed a good deal of direction, I suspect looking back on it. They must have wondered what they had on their hands at times, but they persevered and were very helpful to me.
There're so many other mentors, David, I can't remember them all, but a pair of them that came along were Irl Schoonover, who at one time was Director of Planning at the Bureau of Standards and Alan Franklin, who came in to upgrade the basic science aspect of the Division's work as I remember. This assignment ties into the ADX2 battery additive story, the difficulties of which I'm not going to repeat here, since it's so fully described in the excellent official NBS histories. But the result was the Kelly commission and the recommendation of redirection of the Bureau away from testing and toward more measurement standards and data and strengthening the science here. And I believe it was Schoonover who had a big role in planning for that change and that one of the things that resulted from his planning was that a group

of several outstanding young scientists were recruited and came into materials work including Alan Franklin, Elio Passaglia, and Lawrence Kushner. I had helpful contacts and mentoring from all of them in varying degrees. The experienced staff of the Mineral Products Division helped me with many practical experimental techniques. For example, there was a good deal of expertise in the section in building high temperature furnaces and I think this story is worth telling too.

It happened that the mineral products division had a large stock of platinum and that resulted from a World War II project which was a revival of a World War I project. This was the making of high quality optical glass, which was a German specialty and of course when we were at war with Germany, that supply was cut off. And so they had had an operation in World War I and they set up another one in World War II.

And my understanding is that the Bureau Standards pioneered continuous glass casting and melting technology. This was an important advance on what had been a batch process. When I came here they still had a huge facility for making pots, very large ceramic pots, in which they would make batches of optical glass. But they were also pioneering continuous flow using a large platinum lined tank with different temperature zones in it. The raw materials went in one end and the glass mixture came out the other. And as a result of that defense project they accumulated a large stock of platinum.

The project was closed out at the end of World War II, and the industry took up the making of fine optical glass using the continuous process, but the Bureau somehow retained all that platinum. So we had access to platinum which could be reworked into wire. I suppose some of that is still a legacy; it probably continues in some part of NIST today.

Anyway, I had to learn to make furnaces and people were generous about that, and that touches on another point. We didn't have the safety standards or concerns in those days, and so I handled a lot of asbestos in my time. Looking back on it, I feel very lucky because I didn't wear any mask. I handled this stuff and made furnaces with my own hands. We also had in our Division work funded by a predecessor of the Department of Energy on oxides of nuclear interest. Some of those were not good things to handle,

especially in powder form. The group handled beryllium oxide, for example, in the early days and we had to handle powdered thorium oxide. I fortunately had no contact with the beryllium oxide although I was in the laboratory where it was handled. But I did have contact with the thorium oxide and again I escaped unaffected. One of my friends there came down with berylliosis. It didn't kill him but it sure gave him a bad time. Nothing like that would happen these days with today's higher level of safety precautions. I think it was just the times. It wasn't that the Bureau was bad at it, it's just that today's understanding of the hazards and the corresponding safety precautions hadn't developed.

But anyway, I got onto that by talking about mentoring. So the practicalities of experiments was one form of mentoring, and then the handling of data was another. A statistician named William Youden helped me. I had a little bit of statistics at Carnegie, but certainly not enough. And Youden introduced me to experimental design. Laying out a pattern of measurements so that you could extract the most out of data. It was a very fundamental thing for me to learn the analysis of variance procedures for estimating precision and assigning the variance to different causes. That stood me in good stead. In some of the work I did, it was difficult to disentangle the effect being studied from the scatter in the data. Strength data have a lot of scatter typically, and Youden's statistical designs that he put me on to were very useful. That characterizes I think a form of mentoring, but there were many other examples of that.

MR. OSER: Jack there was a short interruption when the telephone rang. You had mentioned Franklin and John Hoffman and Lawrence Kushner, but there was also a reference to the Kelly commission. What was the context that you were referring to when you mentioned the Kelly Commission?

MR. WACHTMAN: Well, it was the difficulties over the ADX2 battery additive. When I came here I was a person with no concept of Washington politics and I was down at the bench level. I was certainly not aware of most of what was going on, although I read in the newspapers about the trouble. All of you know the story very well, but here is a little bit of it.

The Bureau of Standards tested this additive, ADX2 I think it was called. Apparently this was largely Epsom salts. It was supposed to extend the life of batteries and they found that it did essentially no good, and they said so and the manufacturer had political connections, including a connection to the Assistant Secretary of Commerce. This caused a great uproar that lead to Dr. Astin, the Director, actually being fired. And then the scientific community rose to his defense and within a few months he was reinstated. But the whole thing was a very wounding episode for the Bureau of Standards, and the Kelly Commission I think looked into what the Bureau should do. I'm sure I'm not stating this complicated situation very accurately, but essentially it lead to this redirection of the Bureau away from testing of commercial products, and the farming out of commercial product testing to independent laboratories. NBS was limited to providing measurements standards, so commercial and State laboratories could do product testing with good measurement techniques traceable to national standards. I hope that's roughly right.

MR. FULLER: An interesting aspect of that though, which you did contribute to, was the redefining the direction of the Bureau of Standards. Certainly you helped to guide that through the '60s and the '70s in your management role. I don't know if it's the appropriate time to comment on that, but I find we all are struggling to define the mission.

MR. WACHTMAN: Yes, as I was gradually moving up, I became aware that we seemed to be constantly talking about what is the appropriate mission of the Bureau of Standards and how does the work that you're doing support that mission. And of course, I didn't know much about the mission in the beginning. I had little to do with those things in the early days, but as I became first a Section Chief and then later on a Division Chief, I thought I really had to take this quite seriously and it seemed to me that I had to know a lot more about the materials we worked on. How they were used and the manufacturing and technology and commerce involved, and what a Bureau scientist might to do support that national use or that commercial use, or that economic use through measurement, standards, and data, that sort of thing. And that lead me to a great deal more interest

outside of my narrow field. I could have read only papers relating to mechanical properties in a broad sense but particularly after I became a Division Chief, I felt I had to become knowledgeable about the whole range of activities in the Division and how they related to industrial, commercial, and defense needs and so that lead to a lot of effort.

I eventually was, at one time or another, a member of at least eight professional societies. The Physical Society was my home society, but the Ceramic Society actually became the one I spent the most time on because of the class of materials I was dealing with. The Section and then later the Division were dealing with inorganic materials and the principal society, but by no means the only one dealing with them was the American Ceramic Society which itself was undergoing a revolutionary change from being an engineering, practically oriented society and toward being a much more science-based society. That was a major trend that I think is worth remembering.

This interest in finding out what was going on, lead me eventually to a fellowship with the Office of Technology Assessment of the U.S. Congress. That was a formative experience to me in several ways. I found myself learning about materials in a broad sense: the whole concept of the materials cycle from the earth through processing and use and back to the earth and the energy flow that goes with it. The materials cycle, the energy cycle, the interrelated chemical and physical cycles, and the environmental effects were all involved. We were aware of them in those days though not as much so as we are today. I don't recall that in those days we were talking about Global Warming, but much of the rest of it was already present in the thinking of the materials community. My experience at the Office of Technology Assessment is a subject in itself, but I think I better stop and let you pick up.

MR. WIEDERHORN: I was very interested in your memoirs where you talked about the management structure of NIST, of the Bureau of Standards, when you first came, and then it gradually changed. I don't know if you're willing to elaborate a little more on that.

MR. WACHTMAN: Well sure.

MR. WIEDERHORN: This change has continued and I'm just wondering whether it's for better or worse.
MR WACHTMAN: Well, I have some opinions on that. I grew into the management system in the 1950s and 60s and perhaps because I did, I liked it. I respected the line management structure, and I very much respected Dr. Astin. He was my director for many years after he was reinstated. He went on to a notable career as a manager. He gradually was able to get funding increases. I believe he was the man who was responsible more than anyone else for the new laboratories here. I think, I can't remember exactly when he left. Hans?
MR. OSER: '69.
MR. WACHTMAN: Yes, well my part of the Bureau, moved here in '66 so the construction of the new laboratories certainly came under his regime. Remarkable you know, to recover from being fired, and reinstated, to building an enormous new laboratory.
MR. OSER: You were talking about the laboratory buildings, not the laboratory structure that came later?
MR. WACHTMAN: Yes, yes, I meant to say the physical facilities not the management structure.
MR. OSER: He had nothing to do with the reorganization into laboratories or institutes.
MR. WACHTMAN: Yes. Well, so I was accustomed to this system where I dealt with my immediate boss and to some extent with his boss, but mostly through my immediate boss and you could go and discuss things, and you could get an answer, and you could proceed. It was not an enormously complex and difficult business selling programs. At least to your own management. It was to selling them to other agencies of course. But when Dr. Astin left, Lou Branscomb came in as director, a brilliant scientist, but he didn't stay very long. I forget. Just a few years.
MR. OSER: He left in '72.
MR. WACHTMAN: '72. Richard Roberts came in from the General Electric Company in Schenectady. A young man marked for high things in industry and he came down, I think, with a sort of mission I think he was supposed to bring in modern management techniques to the Bureau of Standards. And he did indeed install a system, which relied heavily on a show-and-tell kind of thing. I don't recall whether zero-based budgeting was an aspect

of this in the background.
MR. LIDE: That came later.
MR. WACHTMAN: That came later. That was Carter wasn't it?
MR. OSER: It was him, from Georgia.
MR. WACHTMAN: But Roberts's concept was to put up the various levels, certainly at the Division Chief level in a public review in which they presented their programs and were critiqued annually. Base review, it was called. Roberts only stayed about three years, or three and a fraction before going off to the Department of Energy.
Incidentally, here's a side light that I don't think many people know. Initially I got on pretty well with Roberts because I knew a number of people at the GE research lab, and I think he had heard of me before he came here from them, and he had a superficially engaging personality, and I could talk to him, and while I wasn't adverse to doing that sort of thing. But I began to change my view of him as this review situation proceeded.
I'll go on with this story, but I've got to loop back and talk about a non related subject. The question of line versus program managers which bears on us too, but let me go on with Roberts. When he left for what was presumably going to be a major position in the Department of Energy, and I think he had identified energy as a coming thing and a thing that he wanted to pursue and build his career around. Not unreasonably for General Electric, but also because of the oil cut-off and the focus that energy had then in our national thinking. He, Roberts, invited me to go with him as his assistant or to consider it anyway, and I asked him for the weekend to think it over.
And I went around and talked to various friends about this and they warned me about what they knew about him and what they knew about the Department of Energy situation. And I thought some more myself about Dr. Roberts who, I had begun to realize, was very success oriented and he would readily discard anything that didn't succeed and anybody that didn't succeed, or so I began to understand. And his management system was designed with that in mind. Put them up, test them, reward the ones that make a good impression on you and scorn the others. That's a harsh statement, but I think not essentially an incorrect statement of

the essence of that system.
Well I went back that next Monday and declined the job, and Roberts changed character. He told me something, I forget the exact words, they were essentially that I lacked the courage to arise to a real challenge.
So that was my Roberts story, and I've been so thankful ever since that I didn't follow him because he didn't stay at the Department of Energy very long and it's a sad story and you all know it, but I think it needs to be repeated here. He stayed at Energy only a short time I think.
MR. WIEDERHORN: Yes, there was a change in the political administration.
MR. WACHTMAN: Yes. Well, for whatever reason he went back to GE, and I think they gave him a major job there. Something like planning the corporate strategy for the future. And the following story I heard, not in print, but through contacts up at the Schenectady lab. I used to have some pretty good contacts there. He worked very hard on that and drove himself. He was now working in a sense under a system of the same sort he had installed and supervised at the Bureau of Standards. Now he had to deliver. He was staying up all night working, and finally came a night when he was staying up all night and he wanted his wife to stay up with him and she rebelled. She had to get some sleep. He worked through the night, went out in the morning, doused himself with gasoline from the car, and committed suicide.
And I've always thought that this awful thing that shouldn't have happened in some ways reflects the pressure of that system when not run decently as applied to the people who have to appear before it. It doesn't necessarily have to be run in a demeaning way. It depends very much on the person on the top. The character of that person and the way they run the system determine how well that system functions and what damage it may do to good people. So if you want a controversial statement Shelley, there's my controversial statement.
MR. WIEDERHORN: You said you were going to go back and talk about the line management. I guess you just did.
MR. WACHTMAN: I have more to say about that. I am going to describe the obligations of line managers and make some

criticism of the situation in which many line managers were placed by the Roberts management system. In this discussion of management there is an important distinction that needs to be made between describing the proper responsibilities of line managers, on the one hand, and criticism of the situation in which they were sometimes placed, on the other hand. Describing the proper obligations of line managers and resulting problems is one thing; criticism of higher management techniques that unnecessarily increase these problems is another. Line managers have many legitimate obligations. People who take line management positions are obligated to face and carry out these responsibilities. Such people have no legitimate complaint about these obligations. Accordingly, I believe that line managers have an obligation to recruit and nurture staff, to recognize (or originate) good programs, to support them, to terminate not-so-good programs, to operate within financial limitations, to take appropriate measures when funding is insufficient, and (of great importance) to do ones best for staff including listening to them, showing respect and fostering their self-respect. This description of the fundamental problems of line management is not a complaint or an attempt to argue for an exemption from these fundamental line management responsibilities. It is an attempt to say that such responsibilities should not be made unnecessarily difficult by higher management as I believe that they sometimes were under the Roberts management system that replaced the traditional management system in 1974.

MR. OSER: Let me add an anecdote to the story of Dick Roberts. At one time he assembled all the section chiefs, which was the level below the division chiefs, in the Senior Lunch Club. And he said, for management purposes, if I want to start a new program, I have to take it from one of you and you better become competitive because I can only give something to one section chief if I take it away from the other, so you better be alert. This is going to be a fight. So that adds to that philosophy of dog eats dog.

MR. LIDE: Watch your back.

MR. WACHTMAN: Yes, Hans, I think the system was designed to force competition. I'm not saying that competition is wrong, but it should also be balanced with a sense of teamwork, with a sense that if you lose one fight, you haven't necessarily lost respect or utility, complete utility. That at the moment it may be better to put the money somewhere else. But the person who didn't get the

money can still be useful to the organization. That's the aspect that I think is very important for a top manager to reinforce.

And Roberts did not, in my view, do that. I believe that He omitted to do so deliberately. In this connection I want to say about program managers versus line managers. One way to start a new activity, to build on Hans' comment, is to put a tax on existing programs. That is, to give the current line units less than they received the previous year for their own programs. Put on, say, a three or four percent tax, thus creating a fund in the director's office. Give it to program managers and charge them with carrying out some activity.

They can farm the money out then and essentially act as contractors, contracting into the line managers units rather than accumulating staff for themselves. It has the advantage that as one piece of work is finished, the program manager can withdraw the money and move it somewhere else. At least in theory. In practice, I think, it may not be so easy. I don't think a program manager's life is an easy one and this is not an attack on program managers, but it is a statement of a fundamental difference in the way that a line manager and a program manager are required to operate.

A line manager has responsibility for the people in his or her unit. He or she has to hire them, put them up for promotion, or criticize them. He or she has to nurture them and to build a functioning unit of people. The program manager doesn't have to be concerned about the people in the same way. Well, I don't want to belabor this point, and again, I'm not criticizing program managers, they have their own set of difficulties. But it is a profound difference I think, and tied to my respect for the line managers. Maybe it's self serving since I was one, but I believe that what really carries the Bureau of Standards, or NIST, is the scientists and the line managers one or two levels above them. That's where the technical strength of the organization is. That's what makes the thing of national value, that's what makes it able to respond to new work. It's the core staff and the core immediate management that makes whatever is of value here rather than just some inspired top leadership. So there is another controversial statement by me.

MR. OSER: Another aspect of the problem with

management was the influence of other agency money. You may want to say something about the influence of other agency money.
MR. WACHTMAN: I lived with that. I went to graduate school on other agency money. I came here on an Air Force contract. I was very lucky to get, sometime later, some STRS money [funds directly appropriated to NBS], on which I did my thesis and finished up my PhD. And many thanks to a number of people, another digression here, the people whose courses I took at the NBS, University of Maryland Graduate School. It included Hans Oser and his course on Fourier Series. Hans, what I learned from you then came back to me years later when I was writing a text book on Characterization of Materials at Rutgers University. I was trying to understand Fourier Transform Infrared Spectroscopy and computed tomography. The former requires Fourier analysis and the latter also, at least in one of its versions. I got out the old book I studied from with you. I still had it at that time, and looked at it, so that was very useful.
I also benefitted from other people in the NBS Graduate School. Karl Herzfeld taught a course in advanced classical physics, and Hans Frederikse taught a state physics course, and Larry Bennett taught a course in nuclear magnetic resonance. I think there may have been one or two others. Those are the ones I remember. I had to go out to Maryland to take most of my courses. And the Bureau was good to me in the sense that they allowed me to shift my schedule and work off hours here, so that I could get out there and take courses. They didn't provide any financial assistance, tuition or anything like that. But I was very grateful for the variation in working hours that allowed me to go there. Well now, let's see. I think I've digressed.
MR. WIEDERHORN: You mentioned Herzfeld, you also mentioned in your memoirs how he was instrumental in your PhD work, or was that just peripheral that he happened to be there as a adjunct professor.
MR. WACHTMAN: It was really peripheral. He was a great, brilliant man, but he was not at all in the field I was working in. There was some common tie and he was interested in the mathematics of relaxation phenomenon, and so he was able to have some sympathy with my thesis which was about mechanical and dielectric relaxation, and it had an experiment side,

and it had a theoretical side working out defect motion.
Attempting to deduce something about the geometry of the defect motion from the orientation dependence with stress and with electric field, and the little theoretical model that I had the good luck to work out.
He did not direct the thesis. He did read it critically, which is a very important thing to do. Unless you want to call that direction. He didn't originate the problem, he didn't tell me how to do it, but he was a smart man and he read the material and he evaluated it. So yes, I owe Herzfeld quite a bit.
MR. FULLER: So your thesis was basically self directed?
MR. WACHTMAN: Yes, it really was. It came out in the Physical Review. It was my 13th publication. It took me a long time. When I first came to the Bureau of Standards I was really up to my neck in just making that project go, and I had no time to do a side thesis problem. It was about years before I could really get into that.
MR. WIEDERHORN: It was very good work.
MR. WACHTMAN: Well, thank you Shelley. It did get into at least one textbook.
MR. FULLER: Going back to when you first came here, you really were in a mode that a lot of us, I essentially did the same thing when I first, I had another agency contract and that's what sort of got me into the whole process. I don't know if you want to comment your first days here, I mean your first years here, not days, but first years in terms of that other agency contract and how that then worked into your career. Because it sort of changed later, especially as you started going into your thesis work.
MR. WACHTMAN: Yes, well the Air Force was interested in high temperature materials for several reasons I think. It all had to do more or less with propulsion systems at that time. They wanted to get higher thrust and they wanted to get higher efficiency, and wanted to raise the temperatures in engines. And there was even a project that I had some peripheral contact with, in which other people were handling these dangerous powders I talked about earlier. The Air Force was actually considering powering a bomber with a nuclear reactor.
One of the schemes was that air was going to come

in the front of the engine and it was going to pass through ceramic tubes, that were heated by the reactor, so you'd have the air coming out hot. It was an afterburner to end all afterburners. So that had the side product of driving a lot of money into various places, including the Bureau of Standards. Of the group at NBS handling these powders, the man I remember is Harry Parker, a wonderfully good practical scientist. They were working on the processing and the shaping of these tubes. They developed processing techniques that are, as far as I know, still pertinent today for oxide fuel applications. The whole thing, quite rightly, I think, was eventually discontinued for reasons unconnected with the quality of the NBS work. Imagine that flying reactor. There were major possibilities for trouble there. But it drove some high temperature research.

Well, I'm digressing a bit. The Air Force interest in my project was in the mechanical properties at high temperature, and this meant strength in several different senses. Fracture strength, creep resistance and so on. This leads to the question of fundamental mechanisms and what controls strengths of ceramics at high temperatures, what are the limits, how can it be influenced. Little was known then, and so I wound up trying to imitate metallurgists and doing it on ceramics, trying to investigate plastic deformation of polycrystalline and single crystal ceramics. Fortunately, for other reasons, having nothing to do with this project, a technique had been developed for growing long, single crystal rods of ceramics of aluminum oxide. Sapphire we called it. I was able to get those and do creep tests. Another nice piece of serendipity here. There had been some creep work going on at the Bureau of Standards, and was able to adapt some of that to doing this work. It was not up to your standards Shelley, in the creep work you did. But we had a laboratory where we had these long rods and we cemented platinum string gauges on them, optical platinum string gauges, where one arm of the gauge would come down and slide over the other arm and you had a grid on each one and you could observe this with a microscope, and you get about one micron accuracy or precision anyway.

MR. FULLER: That's pretty good.

MR. WACHTMAN: That's pretty good, and you could put the attachment points as far apart as you

could get a uniform temperature zone in a specimen, so you could get a pretty fair strain sensitivity there. And you could observe it with a microscope with a relay lens on it so that you could have the microscope furnace wall away from it and not right on. The microscope didn't have to be hot in other words. This was a nice technology, but a touchy one to use because you had to make those specimens.

An interesting thing developed, I have to tell this story. At first it looked like these rods were quite creep resistant, but I knew these engines would run for thousands of hours, so it seemed justified to look at them for at least some hundreds of hours. What developed was that they had been resisting and not creeping at all measurably over the first ten hours and sometimes later. Over 50 or 60 hours you would begin to see some creep, and then the creep rate would increase. So the creep accelerated, and then in some cases it then diminished again. Here was a phenomenon I had no experience with and that led on to a lot of dislocation dynamics picked up in other places.

UNKNOWN: It was nice.

MR. WACHTMAN: It got a lot of attention. It was picked up in other labs including the GE lab and that was one of the reasons I had some connection there. There was a theorist up there who worked on it from a point of view of dislocation dynamics multiplication interaction.

MR. FULLER: Was Jack Gilman with that group?

MR. WACHTMAN: Yes, he was, but he was later. There was a man whose name I can't recall now, who worked out another aspect of this problem and developed a model in terms of dislocation multiplication and velocity as a function of stress that was qualitatively successful. I found that the single crystal sapphire had a great anisotropy of mechanical properties. Favorably oriented crystal would begin deforming measurably about 900 °C over a time scale that we could afford to measure. Another crystal with a different orientation could be raised to 1400 or 1500 °C before it deformed, an enormous anisotropy, owing to the fact that it was a hexagonal or more accurately a trigonal structure, and slip was much easier in one way than on other slip planes. So working out some of that, I started

some of that and other people carried it on.
MR. WIEDERHORN: It had to be really interesting because you had no idea that this was going to happen.
MR. WACHTMAN: No, I did not.
MR. WIEDERHORN: You got the data and then gradually, you and other people figured out why.
MR. WACHTMAN: Yes, yes. It was a lot of fun.
MR. OSER: You mentioned the name Gilman.
MR. WIEDERHORN: Yes, Jack Gilman.
MR. FULLER: J. J. Gilman
MR. WIEDERHORN: Yes, John J. Gilman, and he was very well known for his work on dislocations.
MR. OSER: Just wanted make sure of the name.
MR. WACHTMAN: I recall the name now of the scientist who did some of the earlier work before Gilman got into it. Merritt Kronberg.
MR. WIEDERHORN: Oh yes.
MR. WACHTMAN: Kronberg, you know that name.
MR. WIEDERHORN: Classic paper, yes.
MR. WACHTMAN: Classic paper, yes classic paper. He also did some dynamic work instead of doing static creep; he did strain rate experiments.
MR. WACHTMAN: So there was the single crystal aspect and then there was the aspect that strength of polycrystals measured in conventional bending tests held up pretty well up to about 900 °C and then dropped off. I thought then that it was grain boundaries softening. That lead me into thinking, how could you study grain boundaries. Well one way was to use a torsion pendulum, but making a torsion pendulum out of these samples didn't look promising to me. Another way was to work at higher frequencies with a vibrating bar, so I tried to do that, and there was a fellow at NBS who knew something about vibrating bars, Sam Spinner.
MR. WIEDERHORN: So that's how you got into developing standards for elastic measurements by volume techniques.
MR. WACHTMAN: Yes, later on.
MR. WIEDERHORN: Those are now standard techniques?
MR. WACHTMAN: Oh yes. I think that vibrating bar technique became an ASTM standard.
MR. WIEDERHORN: It's an ASTM standard, and they have a high temperature one in which you have the

bar suspended and then just hit it with a hammer,
MR. WACHTMAN: Yes.
MR. WIEDERHORN: -- and get its vibrations and you get commercial equipment that is sold to do this. So what was very difficult for you now is easier.
MR. WACHTMAN: Yes, that's right.
The technique has its tricky aspects. You've got to contact the bar to drive it but you don't want to contact it so much that you affect the resonant frequency, and even more, the relaxation behavior. So anyway, that was not always an easy technique to use, but I guess they've greatly improved it since my day.
MR. OSER: In our previous conversations you had a lot of good things to say about the manuscript review system at the Bureau. I would like you to repeat those comments.
MR. WACHTMAN: Oh yes, the Washington Editorial Review Board. When I came in as a green young person, I had to start writing papers, and I wrote them up, and my section chief, Roman Geller, didn't understand this particular science particularly well. His expertise was in other areas, but he was a meticulous man, and I would write these reports, and he would look at them, and they would come back all bloodied up with red pencil. He had very definite views on grammar and ways of expression and that was good for me. You know, that helped.
Then there was the Washington Editorial Review Board situation where, as I recall it, the paper was reviewed as a division level and then at a Bureau level, by the review board, so it got two reviews before it got published. And some of those were pretty tough, but again, that was a very good experience that was good for me. And I think that's a great strength of the Bureau of Standards, that they have that system. I thoroughly endorse it, and hope it's continuing that way.
MR. OSER: I'm glad you say that because there are some very harsh critics on the Washington Editorial Review Board, and some people made every effort to avoid it if they could, by sending papers out without review, and they got into trouble.
MR. WIEDERHORN: Yes, someone got fired because of that.
MR. OSER: Right, right.
MR. FULLER: One of the things that I noticed you talking about, which I don't see a number of

the young students doing today, is the statistical analysis and the precision. And you sort of commented at one point about how you worked with the statisticians here, and how the Editorial Review process sort of drove you to put in precision uncertainty statements.

MR. WACHTMAN: Yes. There was another driver for it too. There was considerable scatter in some of the data, the strength data. I was looking at the bending strength of single crystal sapphire loaded rapidly, so there was no time for creep, and it scattered quite a bit when I was trying to measure it as a function of temperature.

Now, I was surprised to find that there was apparently a minimum in the temperature dependence of the strength of sapphire single crystals. I had expected that it would just smoothly drop of off, but there's a minimum. But how much of a minimum. So I went to the statisticians to tell me how to get the most out of this data, and the answer was, design the experiment before you do it, don't analyze it afterwards. Plan the analysis before you do the experiment, and that was very, you know it was an elementary concept, but it was news to me, and very helpful. They had all sorts of designs, and I think that tightened up what you could say, what you could attribute to the systematic variation as opposed to the random scatter in it. And that enabled me to publish a curve which was called a bird shaped curve. The strength went down and up, and then it went down again, and there were different effects. I think Shelley, your work bears on the minimum in it.

MR. WIEDERHORN: Yes, I think that minimum was real.

MR. WACHTMAN: I think it's real, and it probably has to do with water in cracks I suppose, and water coming out of the cracks as the temperature goes up, maybe. I don't know.

MR. WIEDERHORN: I know that happens in glass, and it has to do with water, and then the higher temperature you're driving the water off so now the water's not so important.

MR. WACHTMAN: After you did your work, I suspected that was going on in sapphire, but I wasn't aware of anybody ever demonstrating it.

MR. WIEDERHORN: Of the sapphire, I don't

know.
MR. WACHTMAN: I don't know.
MR. WIEDERHORN: I know of the glass they have.
MR. WACHTMAN: That's the only explanation I could think of for it, and that came years later, of course.
MR. FULLER: The question I've wondered about too is in the early '50s, Gumble was doing some of his classic work here at the Bureau of Standards on extreme values statistics, and since they are viable distributions for strength. Did you ever interact at all with Gumble?
MR. WACHTMAN: No, I don't even know the name. The Bureau was a big place.
MR. WIEDERHORN: Yes, that's right, and there is always someone, some expert you could talk to.
MR. WACHTMAN: You know, a comment here. Years later when I was putting together this textbook on mechanical properties, I struggled with Weibull statistics and working out examples for students. And it struck me then that really, Weibull statistics rest on an assumption, just a power law assumption. Quite a bit of mechanical properties theory rests on representing behavior through power laws.
MR. WIEDERHORN: Because the math is easy.
MR. WACHTMAN: And those power laws don't derive, as far as I know, from any fundamental physics. They're just experimental fits.
MR. WIEDERHORN: That's right.
MR. WACHTMAN: Yes.
MR. WIEDERHORN: And they still use them. They use them for design.
MR. WACHTMAN: Yes. Oh yes. Oh, the other statistician, if I haven't mentioned him yet, was John Mandell. Another fine man.
MR. WIEDERHORN: Oh, absolutely. He was the person I used to go to all the time.
MR. WACHTMAN: We got into that with him, another little side story, late in my 15 years of research here, it was with Webster Capps -- Webster Capps collaborated with me on it, and this illustrates another thing. It illustrates trying to know the industry and at least shape your work somewhat in support of industry.
I had a friend in the AC Spark Plug Division of General Motors. Morris Berg that was his name.

I talked with him at conventions. The spark plug is a remarkable technical achievement by the way. He was trying to develop other ceramic parts. I think he was probably involved in the oxygen sensor, the zirconia tube for the oxygen sensor and the strength of that, and so he wanted a practical test method for it. Strength measurements that he could make rapidly. The direction that strength measurements tended to take in the ceramics community, the fundamental ceramics community, was toward very careful, uniaxial tension tests.

People spent a lot of money making exquisitely machined tensile specimens and very carefully aligned grips. The whole thing became a very expensive business, and to get good statistics, you had to have a lot of specimens. So the price of doing that really mounted up.

MR. WIEDERHORN: About $500 a specimen as I recall in the early days.

MR. WACHTMAN: Just enormous costs, and Berg wanted something, in his industrial research, he wasn't going to get that kind of support, or have that kind of time. He wanted a simple way of testing and we thought about it.

I talked with him, and another issue of strength testing is the surface conditions of the ceramic. If you machine it, you inject flaws, or you put in cold work, you do things to it, and you've changed the surface. And if the fracture begins at the surface you've changed the strength. So it would be nice to have a test that would require no surface machining, and a simple specimen. And it would be nice to have one where the specimen didn't have to be so exquisitely dimensioned.

And I forget how it happened, but I came across a mathematical analysis that somebody in the Applied Math Division at the Bureau of Standards had done. If you have a disk supported on three points, and loaded on the center, what the stress would be on the lower surface. It's a three ball test.

Capps and I decided we'd try it, and we -- largely Webster-- designed a simple loading system; a fixture that you could put in a loading machine. The purpose of the fixture was to just keep the load in the right place. And you could press little discs in the shape of a quarter say, or half dollar, and fire them at very

little cost and effort, and then you could test hundreds of them. Pop them in, load them up.
MR. WIEDERHORN: And it didn't matter if they warped slightly.
MR. WACHTMAN: It didn't matter if they warped slightly.
Yes, and so, now this thing has its limits. It has its inaccuracies. It's not a uniform stress field. Various people have proposed improvements on it, and pointed out limitations in it, but I think it has a place for a certain kind of work, and it was a useful test at General Motors. General Motors in those days amounted to something.
MR. OSER: Jack, you mentioned Webster Capps.
MR. WACHTMAN: Yes. He had worked in the Glass Section. He was a very good experimentalist and a very fine fellow. One of those people it was a pleasure to work with.
MR. WIEDERHORN: Is still alive?
MR. WACHTMAN: The last I heard was years ago. He was from Maine, and he retired to the coast of Maine, and bought a sailboat, but I've long lost touch with him. I don't know.
MR. WIEDERHORN: Okay. I liked him.
MR. WACHTMAN: Oh he was a nice fellow.
MR. WIEDERHORN: He really knew a lot about --
MR. WACHTMAN: And he could write doggerel poetry too. He had a gift for that.
MR. FULLER: I guess I was wondering if you would comment on your activity in professional societies. And I know you were president of the American Ceramics Society, and I was wondering how you got involved with that, and the other societies you were mentioning. How they related to NBS.
MR. WACHTMAN: Well, I was a member of the American Physical Society and my boss, Roman Geller did me another of several great favors. One of which was to tell me, "You better go to the American Ceramics Society meeting." I was only vaguely aware of them. He sent me off to their annual meeting and said, "You better join." I joined. I became aware of the meetings, and I went to my first annual meeting of the society in Cincinnati, Ohio. It must have been about 1954, I guess. And I'd like to tell you a little story about the kind of meeting it was.
The society was organized along the industrial

lines. There was a structural clay products division which was largely brick and tile sort of thing, and the glass division, and a refractory's division, a white wares division for porcelain, and so on. This had a rationale, this was where the industrial support action was. And so a lot of small business people would come, and the convention was not primarily scientific. It was quite different from a Physical Society meeting with all these intense intellectual people focused on science. These ceramic fellows wanted to have their drinks and other things as well. It was a meeting place for good old boys among other things.

The executive director of the Ceramic Society of those days, Charlie Pierce, always put on a floor show which was a great hit. Did you come along Shelley in time to see any of those floor shows?

MR. WIEDERHORN: No.

MR. WACHTMAN: He would hire talent to do that in those days. Television you see hadn't made the impact that it has today. There was still room for that sort of thing. I guess it was already a dying art form, but we would have singers, and acrobats, and dog acts, and comedians, all there at the annual banquet. I must say, it was a lot more fun than listening to the president of the society give an hour address.

MR. WIEDERHORN: You got your opportunity.

MR. FULLER: We had a magician this year.

MR. WACHTMAN: Did you get a magician?

MR. FULLER: No, he was called the experimental mentalist.

MR. WACHTMAN: Oh.

MR. FULLER: And actually Rustum Roy brought him in, but he was just an illusionist.

MR. WACHTMAN: Well, this first meeting, they were having this floor show, and I was sitting in the audience quite new to all this. And there was some very obstreperous gentleman a couple of rows down ahead of me and his wife kept trying to hush him up, but he wouldn't be hushed up, and he obviously had a bit too much to drink and he kept popping up and heckling people, and security walked down the aisle and glared at him, and he'd sit back down, and as soon as they left, he was up and at it again, shouting insults at the people on the stage. This went on and on, and people were nudging each other. "Who is that man?" "Oh he's the president of a small company", someone said. Somebody had spread

that story.
Well, by the end of the program, it became obvious he was a plant. He was part of the act you know. But nobody for awhile suspected. Well, all right, this is the way the president of a small ceramic company is expected to act.
MR. WIEDERHORN: Well, when I started at the society which was in like '62, this was all gone.
MR. WACHTMAN: Yes, I think it died out very rapidly. I probably caught just the very last end of that sort of thing.
MR. WIEDERHORN: Maybe that would help balance the budget, Ed.
MR. WIEDERHORN: When you were in the society, did you find it a bit lonely since you didn't know anybody?
MR. WACHTMAN: You know, I don't remember it that way. I seemed to have made friends pretty quickly there, and I can't remember a lot of difficulty about that at all.
MR. FULLER: Insley was very involved in the society wasn't he?
MR. WACHTMAN: Insley was involved in the society, yes.
MR. FULLER: And at that time there was a local Baltimore Washington section --
MR. WACHTMAN: Yes.
MR. FULLER: When they used to hold their meetings, they would go out to dinner and then come back to NBS to have their technical meeting.
MR. WACHTMAN: Yes, yes. We alternated meetings between the Washington end and the Baltimore end.
MR. WIEDERHORN: So you would've known those people.
MR. WACHTMAN: I would have known those, yes, yes indeed. That probably eased it Shelley.
MR. FULLER: There was a series in the Bulletin of the American Ceramics Society in 1950 about NBS. There were six issues over six months which were all about NBS at the time.
MR. WACHTMAN: Yes.
MR. FULLER: Talking about the materials research at NBS.
MR. WACHTMAN: I think undoubtedly that the NBS reputation in the ceramic community must have been a

big help, whether I knew it or not at the time, to getting into that society. The Basic Science Division was formed, I think, by Carl Schwartzwalder of the AC spark plug division of General Motors. I don't recall exactly when it was formed, but I think in the late '50s. That began to change the divisional structure, and that remained a subject of controversy for many years. Whether to organize the society one way or the other.

MR. LIDE: Jack, do I remember correctly that you made a trip to China in connection with the Ceramics Society?

MR. WACHTMAN: Yes. I think that was after I left NBS. I think that was -- it must have been at about the time I was leaving NBS, but I think it was after. That was when the gang of four had been displaced, and China was opening up. You remember that era? I think it must have been about 1984.

MR. LIDE: That's probably right.

MR. WACHTMAN: Yes, and that was a very interesting thing. Amazing to me to see the situation, it was nothing like China is today. We got out into the back country. I remember we were taken out for a weekend. Another one of these little stories about how things were like. I was riding in a very rough vehicle, and of course there was a bench across the front and there were more of us, and we were sort of in the back of what amounted to a pickup-truck with a sunshade over it, but open sides. And we had some rough seats around there, and we took turns standing up because the vehicle bounced so hard on those roads that it was hard on your bottom and you were better off standing up. You could take the bouncing with your knees as springs.

I remember that episode sharply. We took turns as to who would have enough space to stand up while we were driven around the countryside. And you'd see people sitting by the countryside squatting, not sitting, with some kind of a little table in front of them, and glasses of tea with a little saucer over it to keep the dust from falling in the tea. And people would come by and buy a glass of tea to drink. It was on that level.

This was out in the countryside, and I saw pottery out there. Brick making works. We stopped to see it. It was a really early type of kiln. It was essentially

built into a hillside. They had tunneled into the hillside, and then tunneled up, several stories up. That was the smoke stack. They piled pottery in the back of the cave. Piled the wood in the front of it, and set off the fire, and the flames and the combustion products went up those several stories and out the hole on top of the hill. They had a good draft. The quality control wasn't very great, but they got brick out that way.
MR. WIEDERHORN: That sounds interesting.
MR. WACHTMAN: They didn't have a very high investment in instrumentation there, but they made brick that way.
MR. WIEDERHORN: They knew exactly what they were doing, I'm sure.
MR. WACHTMAN: Yes, they had centuries of experience doing it, you know. But it was interesting to see. I met people who had been sent to the farm during the Cultural Revolution. People who had been sent to the west, educated in the west, went back, were intellectuals, and suddenly under Mao they were just swept up and put on the farm for several years. And they were very bitter about it, and very thankful to have survived it. Not all of them did. It's astonishing to think how China has progressed since then. How they recovered from that period.
MR. WIEDERHORN: Yes, it is. You know I saw some of the same things you saw. I was there a few years later, but very primitive. And then I went back maybe 15 years later and I couldn't believe the progress.
MR. WACHTMAN: Yes.
SHELLEY: And now it's even changed in the north. It's just an amazing country.
MR. WACHTMAN: Yes, you could see water buffalo, and people barefoot driving the water buffalo along, and that sort of thing. Also, the Shanghai Institute, there was somebody there, I can't think of his name now, a scientist who had survived all that period.
MR. WIEDERHORN: A graduate of the University of Illinois.
MR. WACHTMAN: Yes, that's the man.
MR. WIEDERHORN: He was a member of the communist party, and he was a big shot.
MR. WACHTMAN: He was a big shot, and he

managed to keep his institute going and not get sent to the farm.
MR. WIEDERHORN: When I was there, they were just starting to buy equipment. They didn't have proper scientific instruments, and you saw a few of them had just been purchased from the west, and I guess now they must have very modern laboratories. That's interesting.
MR. WIEDERHORN: Was everyone wearing Mao jackets when you were there?
MR. WACHTMAN: I don't remember about the Mao jackets, but they had clean shirts on. What struck me was, as we rode through the countryside so you could see these buildings, and they looked like they had dirt floors. They were living in buildings with dirt floors, but they would come out and their clothes looked clean. My understanding was that they were very poor, but that everybody got fed. There was no starvation in China, and whatever else communism did badly, apparently the people got enough to eat there. I remember the cities being full of bicycles though. Not many had a car, but there were throngs of bicycles there.
MR. WIEDERHORN: Throngs of bicycles there.
MR. WACHTMAN: And there were also ox carts, or bullock carts, or whatever they were, and I remember the Chinese were producing some kind of a crazy looking little engine and pair of front driving wheels. You've seen those things?
MR. WIEDERHORN: I saw the same thing. Those were the only private vehicles they could have, so the farmers would come into town from the countryside on those vehicles.
MR. WACHTMAN: This pair of wheels and engine out in front of it, pulling whatever you hooked on behind it. You know it was a rather practical thing.
MR. WIEDERHORN: It was like a little tractor.
MR. WACHTMAN: Yes.
MR. WIEDERHORN: And all of the other vehicles were government.
MR. WACHTMAN: Yes.
MR. WIEDERHORN: No private vehicles. That changed very quickly.
MR. WACHTMAN: The big luxury item when I was there, was a fan. An electric fan.
MR. WIEDERHORN: Interesting.
MR. LIDE: I read in the Washington Post this morning that China now manufactures more automobiles

per year than Japan does.
MR. WACHTMAN: Yes.
MR. WIEDERHORN: Really? Amazing.
MR. FULLER: About that time is when you were thinking of leaving NBS. You want to comment a little about what made you decide to leave NBS and pursue another career afterwards? You had a very successful career after you left NBS.
MR. WACHTMAN: Well, okay. Well, I was Chief of the Inorganic Materials Division, the new name for the Mineral Products Division, for ten years, and afterwards I was Director of Materials Research, working under John Hoffman, whom I respected very much. He was a very remarkable personality. Something needs to be said about what an extraordinarily able person he was. Not always easy to live with, he had his own views about things. But at the same time, he was willing to listen to you. He would tell you off, but he would listen to you, and it wasn't personal. Afterwards you could be friends with him. You know, I liked the man very much. I respected him.
MR. FULLER: I remember when I first came here as a young scientist, I would come up in the elevator with him, he'd be putting on his tie, but he would always talk to me and ask me what I was doing. He was interested about my research.
MR. WACHTMAN: Oh yes. One side of him, I have to tell a little story here. Hans can correct me if I get it wrong. Jack liked to interview people, and he had a lot of theories about how to interview people, but I won't go into those particularly. This candidate told me about it later. He was pretty nervous; he'd heard about Jack and went in there quite apprehensive. This interview went on for two hours. Hoffman did almost all of the talking. The man hardly got to say a sentence. At the end of it Jack Hoffman said to him, "Well, now that I know you better, I'm offering you the job."
Then he moved up to be whatever it was, National Measurements Lab Director, I guess it was.
MR. OSER: Yes.
MR. WACHTMAN: I got his job of Center Director of the Materials Center. I think I had it for about five years working under him, and his excellent deputies, Howard Sorrows and Manny Horowitz. I think Manny left the Bureau shortly before I did. Maybe a

year before I did. And then Don Johnson I think came in. I believe that was the succession.
But anyway, we had this system. The principal way to defend your unit was to propose initiatives. We all seemed to be obligated at the Center Director level to propose initiatives. I had a very fine person helping me. Next to hiring Shelley, probably the next best thing I did was to get Elio Passaglia as a Deputy Director. A brilliant man.
We had some success with materials fracture and corrosion. People could understand the importance of failure, and essentially things that related to failure and use. We had some success at selling that. I forget the details.
MR. FULLER: I remember the failure avoidance proposal.
MR. WACHTMAN: Yes.
MR. FULLER: An initiative that didn't avoid its own failure.
MR. WACHTMAN: No, no, it didn't.
MR. FULLER: Bad choice of words. Liability would have been better.
MR. WACHTMAN: We struggled with several of these. I tried one on composite materials measurements. That didn't fly either. Elio and I together worked a great deal on an initiative on measurements related to processing.
The argument was, materials properties depend very much not just on their composition, but their structure, and their structure depends on the processing. So you have to control the processing, especially if you're dealing with highly optimized materials, you have to control the structure through control of processing. And that means you need in processing measurements of various things. Temperature being a prime example, but other things as well. We tried to sell an initiative on that. Well, it was a game you couldn't decline to participate in it, and you couldn't win it at that time, it began to seem to me. Materials I thought fell between two schools. They weren't manufacturing, there was a program -- at least there was a recognizable and supported effort in the Applied Technology Division on electronics, and on manufacturing in general. And on the other end there were the basic standards work. That was understandable,

and there's a certain justification for that.
But materials in a sense, I began to think, fell between these two schools, and what I saw ahead I thought was a difficult future for materials and that it might be gradually ground down.
I want to acknowledge the mentoring I received from Jack Hoffman above all. And Manny Horowitz, a wonderful gentleman. Still a good friend. Howard Sorrows was another mentor I want to acknowledge. He had come in from Texas Instruments, and had a long career there, and knew a lot about industrial research, and taught me a lot about how they looked at things. Howard gave me a couple of aphorisms. He would describe the process of hiring and firing, and he said, "breathe in oxygen, give off CO_2. Hire good people, fire the ones who are no longer useful".
And I remember arguing with Jack Hoffman once, and saying, you know we were having to have another reduction in force, "These are good people we're firing." He would make these broad statements. He would say, "Look over the list. Are you running a turkey farm? Find the turkeys, fire the bums." He'd adopt language like that at times, and that's not an expression of Jack's true character. It was his flamboyant use of words. I mean he was a decent man. He was not that sort of man. And I said, "Those were good people, they aren't bums," and he says, "we make bums."
Now think about that statement. That's an awful statement, but in a certain sense, people get used up at the Bureau of Standards. If they're in a field where, when the interest moves away from it, to use Howard Sorrows expression, "Without changing yourself, you can turn into CO_2, change from oxygen into CO_2 in the perception of the Bureau because they don't need you anymore for what you're good at."
And I saw that process happening -- I thought I saw myself turning into CO_2 in the perception of some in top management and I also saw that the things I had tried to sell hadn't sold very well. If I wasn't smart enough, I was sure Elio was smart enough, so it wasn't lack of smarts I thought. I concluded that the Materials Center's best chance was to be lead by somebody that the Director had great respect for, and fortunately we had such a person in our center, and that was the new Chief of the Metallurgy Division, Robert Mehrabian.

A very dynamic, energetic, and able person.
So I thought it would serve me well to get out of the Bureau before I turned completely into CO_2, so to speak, and partly because I thought that it would serve the interest of the people in the center better if they had a person who was highly regarded and could probably get something from the Director. So I decided it was better for me to retire from NBS.
The deal was that I wanted to stay until I had reached my fifty-fifth birthday because of the retirement system. It was advantageous to do that. For my last six months I took a staff position, so that Mehrabian could have the leadership. I was happy to do that. I came to the Bureau of Standards gladly, and I thought I could do better somewhere else. And it turned out I could, so I was happy to leave. Am I really addressing your point?
MR. FULLER: Yeah.
MR. WIEDERHORN: It's a very interesting story Jack.
MR. FULLER: Yes, I had no idea.
MR. WACHTMAN: I think there's something more than just my story in it. I think it happens, a lot of other people got out about that time too under those conditions.
MR. WIEDERHORN: It's a continuing story.
MR. WACHTMAN: It's a continuing story yes.
MR. WIEDERHORN: And I think that you put your finger on it. It's the interests of the organization that change.
MR. WACHTMAN: Uh, huh.
MR. WIEDERHORN: This happens all over, you know. I've spent a lot of time in Germany now, and I think that places like the Max Planck Institute have ways of doing it so that it's less painful. Which, I don't know if I should go into, but it's a better system than what we have here.
MR. WACHTMAN: Yes, I wondered what would happen to materials afterwards, and I took the trouble here a little while ago to look at the excellent NIST website, which has a lot of detail on it. At my peak, that is, in the sense when I had the most responsibility for the largest number of people, I think I had responsibility for about 300 people, including the Reactor Division staff, but if you set aside the reactor I think it was about 260

people. And today the reactor is no longer in the Materials Center which makes a lot of sense I think. For me to be responsible for a reactor never made any sense except administratively, because of course I had no competence in that field.
What had been about 260 people seems to now be about 150. So it has shrunk I think. And I can only say that I have the greatest respect for the people who had to manage that process because it appears to me that the remaining staff and programs are of very high quality. It is fine management at the materials laboratory level to have managed a reduction of this size and at the same time hired good young people and have excellent programs. That's a very considerable accomplishment and the people who did that deserve a lot of credit.
MR. LIDE: Jack, I'm amused by the way that one's own perceptions can sometimes be very off base. During the period you were talking about, the late '70s, early '80s, when you felt Materials was in a period of decline, I was trying to get more support for the Standard Reference Data Program, without any notable success. In my perception Materials was the golden boy of the Bureau and the Materials people got all the increase in their budget, and data was down at the bottom of the priority list. Clearly that was not the case.
MR. WACHTMAN: Well you know, I certainly sympathize with the problem of selling data to the management in the time we're talking about because I had the task for years of standing up in these reviews and explaining why phase diagrams, phase equilibrium compilation, was a good thing to do, and explaining why powder diffraction standards were a good thing to do. Both are widely internationally used and, in both cases, money comes into the Bureau to support people to work on those programs. Howard McMurdie had a long career in the Powder Standards Program including many years of outside support. David, perhaps sometimes you had the same feeling that I did. When I got up to defend those two data programs I felt that I was damaging my reputation as a manager, even though these two programs had wide industrial support and the resulting compilations are used internationally to this day
MR. LIDE: There was very little sympathy for data programs.
MR. WACHTMAN: Yes, I'm sure you're right.
MR. LIDE: Ambler was never convinced it was

the type of work the Bureau should be doing.
MR. FULLER: But I also remember at about that time, on a personal level, you were very involved with physical property databases and the PC had just come in.
MR. WACHTMAN: Yes.
MR. FULLER: I remember a time where you said you put your disk on top of the television and wiped out your database.
MR. WACHTMAN: Yes, that was a bitter lesson you know. I had an old television with big magnets in it you know. One of these things you could hardly lift.
MR. WIEDERHORN: That's funny.
MR. WACHTMAN: All the PC revolution where we had to go back, you began to feel very inferior when all the younger scientist knew so much more about computing than I did.
MR. LIDE: Well, the internet has changed the whole picture. I mean now, databases on the internet are a big thing.
MR. WACHTMAN: Yes.
MR. LIDE: And it's recognized that this is a way to bring innovation to science and technology. That was certainly not true back in the '70s and '80s when there was largely paper dissemination of the results rather than electronic.
MR. FULLER: I can remember convincing Shelley that rather than paying a lot to a commercial agency, for the same amount of money, I could build a computer for our own use. So I built one, and we had enough memory to run three lines of code, until I doubled the memory to 64K. Not M, but K.
MR. WIEDERHORN: Well, the world has changed things.
MR. LIDE: Well, are there any other questions for Jack?
MR. WIEDERHORN: I just want to make a comment. You know there's a reorganization going on at NIST.
MR. WACHTMAN: I don't know.
MR. WIEDERHORN: And in that you may see the breakup of the materials lab. With different divisions going to different parts at NIST.
MR. WACHTMAN: Is that really going to happen, you think?

MR. WIEDERHORN: We don't know yet, but there's talking. I think it would be a mistake. I think the divisions have certain things in common so that they actually complement one another rather than subtract. I found that in my own career, I would as easily go to metallurgy to find an expert to help me as I would to ceramics.
MR. WACHTMAN: Oh, yes.
MR. WIEDERHORN: The science is the same.
MR. WACHTMAN: Oh, yes.
MR. WIEDERHORN: Polymers was a little further distant.
MR. WACHTMAN: Yes. I'd sure hate to see that happen, but who knows. I read with interest, the new Director's confirmation statement. The things he talked about have been published in the Standards Alumni Association Newsletter. And it seems like those statements were made for the public purpose, and it's hard to read what they really mean.
But his background is hardly in materials. His concept of materials I would think would be viewed through the lens of what you could learn by neutron diffraction. Rather than through the whole business of processing and quality control and so on. So he may not have the same sort of attitude towards materials as many of the rest of us do.
MR. OSER: I understand, at least the rumors seem to indicate, that the number of laboratory units will be substantially reduced, and that even EEEL, the electronics laboratory, will also vanish..
MR. FULLER: EEEL and MEL.
MR. OSER: And MEL, Yes. I guess the number of units reporting to the director is to be minimized. I guess that's the philosophy.
MR. LIDE: You know that was Jack Hoffman's philosophy too. I remember he told me once that he thought there should be no more than five people reporting directly to him.
MR. WACHTMAN: He said something like that to me too. The number I remember is six as a maximum. It was a number along that order, yes. And it was some management theory behind it supposedly.
MR. OSER: Yes.
MR. WACHTMAN: Jack was interested in management theories. He told me that he had had a role in insisting that at least the outer part of the buildings of

the Bureau of Standards have windows in it rather than just be blank walls. And the argument that he gave, maybe you remember this, was that when you are thinking, you need to be able to relax your eyes and focus them on infinity, and having windows, you could look out the window and focus your eyes on infinity, and that psychologically, he said, this was very important. I'm inclined to agree with him. It sounds sort of strange at first.
MR. OSER: Makes me feel distressed, allocated to the basement.
MR. WACHTMAN: Yes.
MR. FULLER: We'll paint you a window.
MR. WACHTMAN: The next best thing to do is put a landscape picture on the wall.
MR. WIEDERHORN: I don't know if you've got a point to infinity or not.
MR. WACHTMAN: Yes.
MR. FULLER: If you had to think of one thing at NBS that you would think was a major success, or your biggest success, what would it be.? You brought up so many things?
MR. WACHTMAN: Well, I suppose it was the Physical Properties Section and what became of it. That wasn't just my personal doing, it was the people that were hired into it. Notably Shelley and those who followed; Tony Evans, Brian Lawn, that whole progression of people. I had something to do with getting it started, and I guess I'd have to say that, more than anything else, I value doing this.
MR. OSER: What would be on that plaque, the monument, dedicated to Jack Wachtman? What should that plaque read? What's your proudest accomplishment?
MR. WACHTMAN: I don't know.
MR. OSER: Don't be modest.
MR. WACHTMAN: I survived, I guess.
MR. WIEDERHORN: Brian Lawn once said something about you. That you were the most decent man he'd ever met.
MR. WACHTMAN: For heaven's sake.
MR. WIEDERHORN: Yes, that's sort of a nice thing to say.
MR. WACHTMAN: I appreciate it.
MR. WIEDERHORN: I'm sure he'll never tell you that.
MR. WACHTMAN: No. Is Brian still here now?
MR. WIEDERHORN: Oh yes.

MR. WACHTMAN: Yes.
MR. WIEDERHORN: He asked to be in today, but he's retired and he comes in infrequently.
MR. WACHTMAN: He kept kind of griping, and talking about how he's going to go back to live in Perth, Australia.
MR. WIEDERHORN: He is. What slowed him down is --
MR. FULLER: Selling his house.
MR. WIEDERHORN: Is selling his house, and then the unfavorable exchange rate.
MR. WACHTMAN: Oh.
MR. WIEDERHORN: So he's got hit economically. To the point where it would be very unfavorable for him to leave Washington now.
MR. WACHTMAN: So, I do want to say one more thing, for the record. I have this memoir that I wrote. When you talked to me, Ed, I had simply not been thinking of my time at NBS particularly and thought that if I've got to do an interview, I really should sit down and think about, and try to recall some things. I started writing them down, and one thing led to another and I wrote this document which has now grown to 24 pages.
If it's possible, I would like to have it mentioned in this interview, and I would really like to have it in the same folder, if that's possible, with the transcript of the interview itself. If anybody reads the interview they might want to go on and read the memoir as well.
MR. FULLER: I think it's wonderful. I'm sure that that can be done.
MR. LIDE: Yes, I'm sure that can be done.
The procedure is, after the transcript of this session is edited, a hard copy will be deposited in the archives that are maintained by the NIST library. And I'm sure there's no problem in physically attaching your memoir to that hard copy.
MR. WACHTMAN: Let me have one more thing to say here. I'll read you the opening motto of the memoir and then the closing motto. The opening is a quotation from a newspaper columnist called Don Marquis. He created a character called Archie the Cockroach, who supposedly wrote newspaper columns and gave a cockroaches view of the world, and the quotation I'm offering from Archie is quote/unquote, "Self expression is the need of my soul."

So, that's what drove the memoirs. And then the closing quotation is by Albert Schweitzer, "At times our own light goes out and is rekindled by a spark from another person. Each of us has cause to think with deep gratitude of those who have lit the flame within us." And I like that comment. I feel like I've had so much good luck with being in good institutions, and above all with good people, and that makes all the difference in life. And the Bureau of Standards has had so many good people in it at all levels, and I think it's really what gives the place its value is the quality of the staff, and the direct management above them.

MR. OSER: Now Jack, if your memoir is attached to the oral interview, it may be behind a firewall and may not be discovered. I was just wondering whether we should think of having the memoir, even though it's attached to the interview, also available more publicly.

MR. WACHTMAN: Do anything you like with it.

MR. OSER: Because I could even think of the Alumni Association sponsoring it and send it through the web process.

MR. WIEDERHORN: Does the Alumni Association have a website?

MR. FULLER: Yes.

MR. OSER: Yes.

MR. LIDE: It's under development.

MR. FULLER: There's one internally and we're looking at one externally.

MR. WIEDERHORN: Could be attached. Could be made available.

MR. OSER: It could be attached like Jim Schooley's papers on examples of NBS/NIST excellence that he has been doing. These are typically three or four page documents, each with a specific topic. And I could easily see Jack's memoir added to the same collection.

MR. LIDE: Yes, I think that's something we should look into. Thank you all for taking part in the interview.

End of Interview