

Interview with Walter Rosenblith  
by Eden Miller  
Marstons Mills, Massachusetts

Session 2 - July 19, 2000

TAPE FOUR, SIDE ONE

EM: Today is July 19, 2000. My name is Eden Miller, and I am in the home of Walter Rosenblith in Marstons Mills, on Cape Cod, where I will be speaking with Walter and his wife, Judy. Walter began his career at MIT in 1951 as an associate professor in electrical engineering and eventually became provost of the university before retiring in 1980. Today we will be speaking about Walter's early years in the RLE (Research Laboratory of Electronics) as well as his memories of MIT during the McCarthy era.

When last we spoke, you had just arrived at MIT as an associate professor in communications biophysics.

WR Yes. We talked about the whole business of the title. But what happened when I was there, I had -- in some sense perhaps this is most easily expressed -- I had four offices, believe it or not. I had an office in the main building of MIT on the third floor, which was there in order for my students to be able to come for consultation. I had an office in the Acoustics Laboratory at MIT. And then I had a small office in the Research Laboratory of Electronics, but the fact was I spent my time teaching a good deal of the time and when I say about the fourth office, because the people at Lincoln Laboratory, which had just been established by the Air Force for protecting essentially against various kinds of possible attacks. They wanted me to be also part time in the Lincoln Laboratory. Now what happened is that in this office -- I remember its number, 397 on the third floor at MIT -- I used it very little because what I did in terms of teaching, I taught a course in circuits, which was taught by Professor Guillemin, who was a fabulous professor, and I was advised by the department chairman I should take a section of that to see how things were going. In some sense, since I was trying to develop new fields, I should, however, have contact with the department, and that was a direct contact with the department. And then,

what happened is I taught one of the sections -- there were probably six young professors, assistant or associate -- and we got to know Guillemin. He was really a fabulous teacher. And that's what I did in the first semester as far as teaching is concerned.

The second thing was that people in the Acoustics Laboratory who had known me for what I had done in acoustics wanted me to be participating there. And they offered me an office with a part-time secretary, and that was helpful. And then, as I said, very quickly I said no to the Lincoln Laboratory, where they wanted me to be essentially somebody who was mathematically inclined to work in so-called human engineering. I never thought of myself as being in human engineering, and besides I didn't really want to have anything to do with the military.

EM: Would you have had to have a security clearance?

WB: Yes, of course. And also what happened is Lincoln Laboratory had originally only a little space on the MIT campus, but then it moved out to Lexington. And for me to not only have part-time but to have to travel even -- that didn't make any sense. So here we have the department as such in the main building, the Acoustics Laboratory was at the end of Building 20, the famous Building 20, and they had there a big anechoic chamber. I don't know whether you are familiar with anechoic chambers.

EM: Are those the foam chambers that have cones?

WB: Yes. They basically make no echo, so that you can really study the way the sound travels. And they said, Well, maybe you can do your experiments in the anechoic chamber. There was a big anechoic chamber. And it turned out that they didn't want me to bring animals into the anechoic chamber.

EM: Did you want to bring animals into the anechoic chamber?

WB: Well, I had studied at the Psycho-Acoustic Laboratory the electrical activity of cats, and hamsters, and so on. So I could understand that they didn't want the smell and all that went with animals -- and besides, my experiments were not short experiments, but sometimes they went for several days. We anesthetized the animals because at that time we didn't know how to do it otherwise, so these were experiments that were in many respects final experiments. But they were also... I knew people wanted to come to use an anechoic chamber somewhere in the Boston area. There were other people who wanted to study the possibilities. So what happened is we -- I said we, I think MIT -- and it was probably money that came through the armed services -- paid for the building of an anechoic chamber. A special one for what communications biophysics was going to do. But we did there experiments also on humans. And we found a way of having both going on -- there is a picture somewhere of my daughter in an anechoic chamber lifting her finger when she hears -- you know, psychophysical experiments. You remember that, darling, don't you? And I think that this was a project -- the special anechoic chamber for our purposes -- that took some time and quite a bit of money. The money was provided by RLE and the Acoustics Laboratory, if I'm not mistaken. But I wasn't interested in money -- I wanted to get the facility. The facilities that I needed at MIT for the work I wanted to do where both computers and developments in that area and then they were -- the chamber, the facilities in that respect.

EM: Who else would use the chamber besides you?

WR: That chamber was only used by people of my group, as I remember. And it turned out that the group -- I don't know how long it took to build that chamber, but in the interim I was able to use the big chamber for human experiments -- for psychophysical experiments. Now, I will say that obviously when I came to MIT, RLE was a successor to the Radiation Laboratory, as I have said. The Radiation Laboratory made a big contribution to the whole development of radar, and changed the country. Before the war MIT was a place that mainly dealt with industry. During the war it became one of the biggest military contractors. And it had a new pattern of

staffing. These were not staffs for a given discipline in general, but there were 4,000 people in the Radiation Laboratory, enormously, coming from all parts of the country. There were physicists, engineers, biologists, psychologists, I mean it was a major pattern of a multi-disciplinary enterprise, which was quite unusual in the pattern of universities at that time. In industry, some of these may have existed. After the war was over RLE maintained in some sense that pattern.

I think the armed services were very flexible at that time. In recognizing that now the war was over MIT's strength should not be used only in direct implementable things, but that basic research could be done. Professor Zacharias, who was a member of RLE and who had been brought in from outside, always said that they agreed to a formulation that RLE should be working in all the areas of electronics in a non-narrow sense. And this non-narrow sense is what attracted me a great deal. In some sense, in the department there were so many professors and so many special fields, that there wasn't a real community. I mean, it was a great department and a great incubator for a lot of new ideas. The Acoustics Laboratory seemed too much going in the direction of outside interests. There were people who had been physicists and engineers and even architects. The problems of acoustics in architecture had always been enormous, and they got interested in the building of concert halls, and so on. Then out of that there came to be a consulting firm which was probably a pattern of the origin consulting firms in the Cambridge area. It was called Bolt, Beranek & Newman. Bolt was a physicist and he was the director of the Acoustics Laboratory, and Beranek was the technical director of the lab, and Newman was the expert in architecture. And this was a strong group in that field, but it was not really a community. And given the fact that when I came to MIT I think Dr. Stratton was still the first director of the Research Laboratory of Electronics, but he quickly was replaced and Wiesner became the director of the Research Laboratory of Electronics. And Jerry and I were already friends at that time, and he sort of attracted me into the group. Now if you have the little booklet which I put together, the "1946 plus 20," you could see how the Research Laboratory of Electronics was structured into a variety of groups. These were some of the patterns that during the war the Radiation

Laboratory had found useful, but what was more, there was also a community -- the people got together for lunch and they discussed technical questions, but they talked about everything including politics. And this made much more of a community in which I felt comfortable and where people were interested in the things that I brought there because there were lots of queer ideas floating around. And I think the formulation of electronics in a non-narrow sense was a very broad umbrella under which you could do things.

I should also add, on the other hand, that in the department when I came to the first meeting that I was invited to among professors, people came there and we talked about what I was going to do. Well, they understood some of the acoustics that I was going to do. Then I told them that I wanted to study the electrical activity of the nervous system, and people asked me, You are serious? Is there electrical activity of the nervous system? At that time, this was not something that engineers who had been trained in the usual sense found to be a natural part of their intellectual horizon. They were very friendly towards me, and they patted me on the back for taking a section of the circuit course; however, they thought I was sort of a wild guy like later on people would say about artificial intelligence.

EM: Why hadn't engineers before put together electricity and the nervous system?

WR: Well, there were great physicists in the past who had put this together, but at that time the intellectual horizon of classical engineering -- and even though MIT was probably the best of that classical engineering pattern -- but it didn't encompass -- I mean it was many years later that biology became a required course. And though there might be individual engineers who were working with some people at MGH (Massachusetts General Hospital) on something to do with the nervous system, but what I was doing was to study -- for these people the study of animals, which nobody thought belonged in engineering. And people could go through engineering courses with the curricula that existed -- four semesters of physics, four semesters of mathematics, and then you specialized in various fields of engineering. But there was no contact with

bioengineering. The mere fact that I said -- most people thought that I should have taken a title that didn't include bio. They thought that belonged somewhere else. And that was the intellectual horizon. I might add one other thing. After I had been teaching in my section for a while and felt I knew the students, I asked them who did they think were the most important physicists. And I think two names came back: Einstein, and the other one was Oppenheimer. Those were the names that they knew. And when I talked about the history of the field, and so on -- I encountered the view even at Harvard in the psycho-acoustic laboratory that when I said, "Why don't you study the history of this problem," they said, "It's easier to invent the future than to study the past." So as time went on RLE offered a very convenient place for me to exchange ideas and to fit in the kind of things that we were studying, which were sensory communications and we also had some interest in developing computers at that time.

EM: Can you give me a sense of time? How long were you at the Acoustics Laboratory?

WR: Well, I stayed at the Acoustics Laboratory for a long time because I had a secretary there, which was important, and I moved into RLE, which was physically -- I mean they both were in Building 20, which was this incubator of all sorts of things, and which has been now flattened. And have you seen the new building that is going -- that's going to be something very different. Just as there was a change from a gray long hall to a more colorful hall with outright art and so on, this is going to be an architectural switch of an important way, just as Corbusier in some sense was at Harvard. I don't know whether you know the Corbusier right next to the Fogg Museum.

Okay. I don't want to get distracted. But it took probably four to five years to build the anechoic chamber and also to see how the group around me was growing. And then I moved into Building 20 towards the [core?] and started out with several rooms, and that expanded. Now, who were the people? I had first a few graduate students in electrical engineering. But I also, I should say, developed at that time a joint course

with Professor Halle, who had come at the same time to MIT as I. He was a linguist, but there was no linguistics department, so he was in the School of Humanities, and I in the physics department, and we developed a course, "Hearing, Speech, and Language," which was quite ambitious at that time, and also interdepartmental in the sense of -- the electrical engineering department, and I think he was in the department -- I don't know what it was called -- the director of libraries was his boss. But we got students from other departments to take that course. And of course MIT's development in linguistics was enhanced by the fact that later on MIT attracted Professor Jakobson, who had been the teacher of Professor Halle, and who was a world authority. And RLE was interested in these things too.

JR: And Chomsky.

WR: And Chomsky came later. I think came even after -- we'll look it up in the Who's Who. But Chomsky came, and once Chomsky came, the whole reputation of linguistics exploded.

EM: Can you be a little more clear as to how this interdisciplinary course that you taught with Halle related to this eventual work in linguistics, and drawing Chomsky.

WR: Well, that wasn't the only magnet for that. The fact was at that time people started to talk about machine translation. And there was a man called Ingve[?] who did machine translation, and then Ken Stevens was interested in the phonetics part. And in the course that Halle and I taught together, phonetics was in there. So we had machine translation. The director of libraries was very much interested in that. And then the question Chomsky raised was, What was the structure of grammar and syntax? So that pushed this whole field which originally was connected to acoustics via phonetics into a much broader range. And I remember it was still not that well known.

I was not yet chairman of the faculty, but somehow I was the chairman of the committee that presented to the faculty meeting of MIT the new program in linguistics. That came because I had these connections to Halle. And the faculty wasn't quite sure that we should recognize that as a new department at that time. And so we had to go through a certain number of faculty discussions and recognize that these people were honest workers, and they're considered by all the people in the world as being the leaders of the field. Harvard previously had been. And there was always when you went to the faculty for a new program, this was considered, You are trying to take away part of what we have considered ourselves -- the identity was being changed. And faculty people in that respect are quite jealous. When Jerry Wiesner and I brought on the Health, Science, and Technology, which has many other difficulties, what with bringing in Harvard. The only thing that Harvard and MIT did together was during the war they created a joint laboratory in physics. And that was located on the Harvard campus. But this whole question of faculty flexibility is true as far as I experienced in departments, in schools, and in whole institutions. The story was interesting when we talked about -- I think I talked about the fact of Samuelson bringing economics to MIT. I looked it up in Killian's book -- what does he think about this? And he talked about how brilliant -- you know, the question of anti-Semitism -- and Killian in his very subtle way said that Samuelson came to MIT since his future at the other institution was uncertain. There it was.

JR: One might interject about the fact of his talking about institutions being that way -- I spent a period of time in which I had an appointment on the Harvard Medical School faculty, and I used to attend the faculty meetings even though my relation to it was quite remote. And every time any issue in terms of curriculum change came up, you had everyone saying, But I can't have an hour less of something in order to give an hour to that. And I was on the Graduate School of Education faculty for a while, and the same sort of thing occurred there, though with less intensity than at the Medical School -- that's Harvard. So his point is quite generally taken.

WR: Well, the fact is that each time we changed the curriculum, the chemists came to the floor of the faculty and would practically cry and say, We can't have one hour less of chemistry. We can't teach chemistry anymore if that happens. I mean that is a pathology, I think, of institutions that basically belong to the period when they were essentially committing themselves not to the problems of the world, but to a discipline in a narrow sense.

EM: So this was the major objection to the formation of the linguistics program?

WR: Yes. I think there is always, though it isn't spoken about -- I mean, linguistics had grown enough that some government agency was willing to support it. MIT did not have a big endowment, and before the war had very little. I mean there were no people who gave \$300 million or something like that. MIT's alumni, lots of them got out of MIT during the depression and had great difficulties finding jobs, leave alone accumulating capital enough that they could give. One of the first Alumni Association meetings I ever came to, I heard the same debate. The alumni said, "But you only ask us about money. You never ask us about changes in the curriculum." And this is something which is another problem of change. And so MIT, since it didn't have money, had been a big contractor during the war, and I think Dr. Kiillian's genius was that he found a way of keeping the contracts from government coming. Even though they were not of the size that they had been during the war, nevertheless -- and he kept them coming for non-narrow interpretations. And then he developed also a technique to bring people in, and the fact was that...

#### TAPE FOUR, SIDE TWO

WR: So the question was, when you had new adventures, MIT had no venture capital essentially. But the government, because of the reputation of MIT and the results that it had produced during the war, government agencies were willing to endow new ventures. And the way this went in RLE is that RLE had an advisory committee from the three services, though it was mainly Navy directed because the Navy had the historical role of being the more technical service. And the Air Force wasn't yet that

much a sponsor. But these people came every year to meetings -- and that I remember while I was there -- ones at which they came and wanted to know, what have you produced during this time? And it was the art of the director of RLE to produce all the important things that he thought they would be interested in. So you had a whole group of people in the services who were essentially connected to live research and us. They could take whatever we did and formulate it in its relevance to what the services were looking at. Today that story is true also for the National Institutes of Health. People make a new discovery, and I listen in general on TV to the hearings in Congress, where, for instance, Arlen Specter and the senator from Iowa have the leaders of the National Institutes of Health to come and talk about Alzheimer's, to talk about other diseases, and the senators interrogate them and say, "What will this do for the people?"

I think this pattern of having the government involved in things that very often can't be completed in terms of devices or inventions -- (this is what all the fuss about genomics was) -- this is a pattern that probably is quite unique in this country. And MIT in some sense were leaders in this country. And the fact is that after Stratton had been director of RLE, he became provost, and in some sense -- there were deans for each school -- there still are deans for each school -- but the provost had the mission to in some ways, not direct, but keep in real touch with all the interdepartmental ventures because the department said these guys are trying to take away something from us. Interdepartmental is not taking away but adding something -- being oriented toward problems, problems in technology, problems in real society, problems in health -- this was another one of these fracture lines in the university. So I think that in that respect RLE was for a long time the pattern in the country for interdepartmentality.

EM: Did other universities come to the RLE to study the way that it was...?

WR: Yes, we had lots of visitors. And I mean visitors not only from this country, but visitors from Japan, visitors from France, from Germany, and that led me to some

extent -- there were visitors especially in the developing areas of information theories and computers. We had a man who came from England, Colin Cherry, he was such a Britisher. And he spent a year or so. We had a man from France with the incredible name of Marco Polo Schertzenberger. Remember, Judy. (laughter)

JR: Of course.

WR: And he was really a psychiatrist. Now that early gave me the idea that the group that I was developing, which was basically starting out with MIT graduate students in communications biophysics. And if you look at the progress report of the RLE, which is another pattern that glued the institution together -- I brought in people from different countries. And I guess I was maybe among the first to bring in postdoctoral people from Germany, from Japan, and people who later on in their own countries turned out to be quite remarkable people. So we were not only interdisciplinary, but we were also international.

EM: How did you connect with these people?

WR: Well, they found out about some of the papers we gave, and I guess I traveled already a little there. You know probably the thing that really represents in some sense the most of what I have done, and I don't know whether you have seen the book called *Sensory Communication*. Well, for that I traveled in Europe quite a bit in the late '50's and found the best people there. And this sort of radiated back by having their postdocs and others come here. But I had postdocs from other universities in this country because this combination of taking classical fields and putting them with computers and mathematical analysis of some sort and model-making -- that brought people from other institutions. And I think that if you look at the list of people, when you look at the progress reports for this group that I led for a while, it is amazing how they -- we had people from Chile, we had people from -- well, I can't now reproduce all of them -- but we did have people from Switzerland --

JR: You had one from Argentina too, didn't you?

WR: That's right. And Japan -- the man that we had later on became a very famous Japanese physiologist, and he sent his students to us. I mean this is a flow that renews itself. It's recycling in some way. And that's, of course, what made the scientific community what it is today. But it wasn't that big at the time. Today you see the problems of Los Alamos. It turns out that in this country, I think, enormous percentage of the Ph.D.'s in the physical sciences and in engineering are of Asian origin. And the problem is the fact that Los Alamos may be working predominantly on problems of weapons, but the problems are related to not just weapons but to the general problems of science. And if we don't have in this country the Ph.D.'s in these fields, and if we don't have contact with the people abroad, that brings up the difficult thing which it is very hard to explain to Congress people, who think that security ought to be the top consideration and not -- I don't know whether you saw the story that Los Alamos has now tremendous difficulties in attracting people from those areas. And fortunately in the period in which we were at RLE, nobody asked me, Now what did he do and does he have any connections to something else? I told you in some ways when I was in Paris how the Austrian embassy said to me, When you go into the Ecole Supérieure d'Electricité, look at all the things, some of them may be useful. In some sense, this is still the pattern that political people very often consider that way. But RLE was free from that.

EM: No one ever asked you to justify why international visitors were coming?

WR: No. The problem was, do we have support for them? What happened often is the country that they came from supported it because they thought this would be interesting, and it was not people that came to RLE -- RLE was not in weapon things. Lincoln played that role. They could go there and do classified research. And then we had the Draper Lab, which had its problems because they were working directly for weapon things. But in RLE nobody asked me, Why do you bring these people? All I had to do is to document that they were good people and what they were going

to do. So in a sense RLE was an almost ideal environment, and I think people worked in so many fields in that period, and that to me is an important thing. During the war, and that made for a big change in the academic climate, during the war obviously it was directed toward specific products that could be used -- either weapons or communications or detectors of enemy structures -- and after the war was over we didn't have a long period before the Cold War started. And so in some ways the set of things that I and my group were trying to do used the developments of the war -- of the devices, machinery, computers, and so on -- in a field that was health oriented. And I think that made a great difference -- attracted a group of people who didn't want to be in weapons.

EM: Why did you think of bringing computers to the biological sciences?

WR: Well, because I had been influenced by Norbert Wiener, who thought that one can mathematize biology. He had worked with some biologists and had developed a reasonable sense. Have you ever seen *Cybernetics*? There he essentially thinks that you can do things mathematically. Now I thought that you couldn't do it mathematically as such, you have to do some experimentation. Did we talk about Norbert Wiener the last time?

EM: No.

WR: Well, Norbert Wiener is a chapter all by itself. There are some people now who want me to work with them on a biography -- in what they say will acquaint Norbert Wiener with the present generation. I said I am committed at this stage, and I cannot get into that. But Norbert Wiener had a supper seminar, as it was called. And again, one of these incubators of an interesting kind. And it was at the Smith house, which was right next to MIT. And people got together from all fields. And he essentially gave post-postdoctoral examinations. People had to explain what they were doing and why at the frontiers -- I mean Samuelson came there and many others, and you were connected with the real frontier of thinking even though Wiener himself wasn't

always sure that what they were doing -- as long as it was somehow echoing what his ideas were. I'll give you a specific example. We started to study the electroencephalogram, and we were trying to find some mathematical regularities. And we were studying that in a way, and Wiener would come by -- and there are some pictures in which Wiener stands there with a junior colleague of mine who came from MGH (Massachusetts General Hospital) and myself, and I had no competence to do EEG's. And after we had done a certain amount of work, Wiener says, Isn't that enough experimentation, and we said, No, it isn't. And he got unhappy with a postdoc, one of the people in my laboratory, and went to the president and said, Tell Rosenblith to fire that man. And the president said, Well, let me talk to Walter about this. He talked with me and I told him that this was clearly what we had to do. And Dr. Stratton had himself not been an experimenter, but he had a very broad view and as a provost, he felt that it wasn't Norbert Wiener's job to tell us what to do. So finally when Norbert Wiener saw that I wasn't going to fire the man, Wiener went to the president and said, Fire Rosenblith! (laughter) That's the way -- he was a very difficult person. A genius, but a man who had had a long, difficult road at MIT because when he came to MIT, even in spite of the fact that he was a wunderkind -- young genius -- and that he had done enormously good things -- mathematics was considered at MIT -- again, it shows the problem of faculty -- was considered a service department. It wasn't considered something in its own value.

And this problem of having enough flexibility to recognize new things, even if they look crazy, and give them a chance to grow, and if it doesn't work out to somehow find a way. The choices that you make among these new things is a terribly important matter. And I must say, in that respect, MIT has not always been successful, but it is amazing how many good bets it made. And there was this climate at that time that young people should come in. I mean the World War was there, and then the Korean War, which was a real disaster, and then the Cold War developed. And so MIT got pushed more and more into things that were related to the Cold War. MIT, not having any big endowment yet, didn't have much money, and the government became less willing to support adventurous things. And so the role of an

administration, and when I became -- especially after the years of trouble -- when I became a provost, my role was to be sure that I supported new things with whatever one can as a provost -- I'll talk about that later. I was also the chairman of the Space Committee. So I allocated space, and that, of course, is a resource. And if you read the report of the women's group, who said that they -- at MIT, the women faculty -- not only were they disadvantaged in terms of money, but they also didn't get the same space, you see. And those are the resources that one needs, and they didn't get the responsible jobs as far as committees were concerned. And I think this problem of how you work change in an organization such as MIT where you have the sources of financial support -- and for a long time MIT didn't have any endowment, and then Dr. Killian found the thing that related to the government and kept on on a very, I think, flexible basis. And then we got some support from foundations. And in general foundation support during those years was very important because it gave people a little venture capital. And then came the period where the alumni started making enough money and so they could support some, but in general, MIT didn't have the tradition of big sums and within the past few years MIT has had such an increase of gifts, even for endowment that -- from what I understand, the present campaign is doing so well that they may double the amount for it.

EM: I'd like to ask you something about a comment you made earlier. You mentioned that the Acoustics Laboratory wasn't a good community. And you've alluded to the fact that the RLE was a good working community. In your opinion, what are the key ingredients to having a successful scientific community.

WR: Well, I'll tell you. Let me start with the RLE. I think when I came to MIT the Acoustics Laboratory was already draining things off in the direction of the company Bolt, Beranek & Newman. And so there was no lunch. The luncheon at RLE in which a lady cooked luncheon, and you smelled the hamburgers. You knew you would get together with a group of people and sit around and talk about things.

EM: So food is a key ingredient. (laughter)

WR: The food? No, I think the fact was that you had a director who was willing to hire somebody to do that. And then the other thing was that it had a publications office, and that publications office was responsible to get progress reports. And so people had accountability in some sense. All of these things, these RLE progress reports -- just Monday, when we were in town, I got last year's progress report which is now obscenely thick. And when I was there it was maybe one-fifth of that. But these were mechanisms that are mechanisms of coherence and there was both human glue and technical glue. Also, the Acoustics Laboratory had the big anechoic chamber, but that was its great facility. And that was not flexible for other things, but RLE was willing to help get new devices. RLE was willing to bring in people as affiliates, as postdocs, and they were willing to put together a human community that was interactive. And that I think was the great difference, and successful. And Jerry, who, after having been director of RLE, went to Washington as advisor to two presidents, and when he came back, he became dean of the School of Science. And after that, he became provost when Howard Johnson was elected president

The transition of presidents at MIT was always a big thing. And I think there are lots of problems there but that I don't think is appropriate right now to bring up. So then he had the problem of convincing not only -- there is also the Corporation at MIT which plays a very important role. The Corporation played an important role also in connection with the years of trouble. But there was the time after Stratton went from being president to being chairman of the Board of Trustees at the Ford Foundation. So there was a discontinuity there because in general, presidents became chairman of the Corporation. Then after that, it was not easy to get a new president. And the engineers wanted to have Gordon Brown become president. And the others felt that Gordon Brown was not flexible enough. Though I must say, Gordon Brown and I, we differed on many things, but he was basically fair to me, and I mean, he was willing to go, in the McCarthy period, to my hearings in New York City, and he defended the fact that I had gotten tenure. But after Howard Johnson had been president, and Jerry was most likely going to be the next president, Jerry had to

defend the fact that after I had been two years chairman of the faculty, I should still be for the interim period even though -- as associate provost, and that wasn't easy because the Corporation people were well aware of the fact that both Jerry and I were Jewish. And undoubtedly, at that time this counted a lot more, even though it was clear that we belonged to the group of Institute Professors, which is a special device that Dr. Killian invented, and which the faculty resented enormously (they have special privileges) when it started.

EM: The members of the Institute community didn't like the fact that you were both Jewish?

WR: No, the Institute community wasn't the question. It was mainly the Corporation, and not, I think, the whole Corporation.

JR: Obviously not. Or Jerry wouldn't have been elected.

WR: Yes. So there are still -- I think today, I don't think that factor counts much.

EM: Before we leave the topic of the RLE, I'd like to know who you feel were your major influences -- who influenced your research to the greatest extent?

WR: Well, I think this was the -- today people talk about the cyberculture. Well, most people don't know that cyber comes from cybernetics. And in some sense, the post-cybernetic era and Norbert Wiener influenced -- not specific research because as I have told you, he had no feeling for experimental things -- but it oriented the way I and my colleagues thought about things. And it was a combination of responsible experimentation with the hope of not just, put down data -- not one [damn?] fact after another, but trying to make a sensible model if you could, or integrate something. I think that for that period -- and then Jerry, because Jerry and I talked to each other, and though after his years in Washington, he was much more occupied with the problems of anti-ballistic missiles and so on than I was, but we belonged together to

the Pugwash group. We went a few times to the meetings. But Jerry's view of the world I think I often disagreed with in detail, but I certainly agreed in principle and was very much influenced by that.<sup>1</sup> Wouldn't you say that, darling?

JR: I would say that. But I wasn't so sure you often disagreed.

WR: Well, on specific people

JR: Oh, on people. Yes, that's another detail of thinking or philosophy, or what have you.

WR: You probably haven't seen the photograph there of J...

#### TAPE FIVE, SIDE ONE

EM: Today is July 19, 2000. My name is Eden Miller, and I am continuing my conversation with Walter and Judy Rosenblith in their home on Cape Cod, where we were discussing Walter's early years in the RLE as well as his memories of MIT during the McCarthy era.

WR: And I think the phrase -- MIT went from an underendowed institution to becoming one of the world's leading institutions in the scientific sphere and not losing its technological heft. And people always are surprised that there is a School of Humanities, and I suspect that they will even be more surprised when in October they will add humanities, social science, and arts to the title of the school.

EM: Oh, I didn't know that.

WR: Well, I guess this is a piece of information that I thought would be interesting. You see, Howard Johnson brought about the Sloan School. The School of Architecture was the oldest in the country. And then the Lewis Committee was somehow

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<sup>1</sup> Walter A. Rosenblith, ed., *Jerry Wiesner: Scientist, Statesman, Humanist : Memories and Memoirs* (Cambridge, Mass.: MIT Press, 2003).

responsible for the School of Humanities, and it had an extraordinary dean as the first dean -- Dean (John E.) Burchard, who was really the MC for the centennial of MIT -- who brought Churchill. That's a story that somebody ought to put together. And since then, there have been economists as dean, and now [Philip] Khoury is really a humanist. So I think MIT is still developing and filling out in some sense a humanity perspective. MIT also never forgot that there is a country, and we had the Center for International Studies, and so on. And today we have the security things, and we have professors at MIT who disagree with the Pentagon. And publicly so. And I think we have had troubles of a variety of sorts. During the Vietnam War Walt Rostow, who was an MIT professor who worked on the Vietnam War and whose wife was the only tenured woman faculty member when I came to MIT. So MIT at that time was very much under attack that Walt didn't tell [the President to get out of Vietnam]. So when he wanted to come back to MIT, MIT said, not so fast. So MIT, I think, basically, even given its connection to the "establishment" of the United States, is very courageous. And that I think ought to be counted in -- not for the number of patents or the number of products. And this whole thing is a problem that will come again and again as it has come in the area of biology.

EM: I'm sure we'll get back to that when we talk about the McCarthy era after lunch. But I wanted to ask you one last thing, which is, whose research do you think your work influenced?

WR: I would say, let me think over lunch.

JR: One question. You said there was only one joint Harvard-MIT thing for H S & T (Health Sciences and Technology). Not true. There is the Joint Center for Urban Studies.

WR: True. But that came later, I think. Well, I'll have to look it up. I can't remember. I was for one half a year director of that center.

[Break for lunch]

EM: So when we left off, I had asked you whose research you felt you had influenced.

WR: Well, I certainly influenced the people who were doctoral and post-doctoral associates of mine, but again, being a person who was basically looking at contextual views of research, I may in some ways have contributed to giving them a view of how their research fitted into what the whole laboratory was doing. But in a sense I felt -- and this is undoubtedly immodest -- I felt as an ambassador of the way in which research ought to be done. And there I think I had -- to some extent my European background -- even though German kind of research was certainly not that -- but I tried to give people who worked with me the feeling that they were part of a bigger picture. And to some extent this showed up as they went on in their careers. And I guess I was not a person who was going to point to one of my associates and say, This is the man who carries on. I tried to give people independence as long as they were conscious of the fact that they were not alone. But what they did had to be in some relation to their colleagues and to the history. Is this a fair statement, Judy?

JR: Yes.

WR: Does it answer your question?

EM: That answers my question. So now we're going to switch gears, and I know that one thing that you've been anxious to talk about is MIT during the McCarthy era. So perhaps we should start at the time you first became aware of the Red scare.

WR: Well, when I came to MIT, McCarthy had already started his wild accusations, and there were people named in the faculties of universities and especially also in MIT and at Harvard. And I don't know the details of how Harvard started dealing with these matters. I only really know that I got the feeling -- and partly because I knew some of the people who were accused at Harvard and partly because *The Crimson*

certainly wrote about this -- that Harvard tried to distance itself from these people quickly. And MIT, where a few people had been named, said, let's set up a committee of colleagues and let them look into it and suggest what MIT's position should be. And that didn't mean that the Corporation would go along with it, but I think it meant that there was going to be an input of people who were peers. And there was one man whom we knew well -- that was Ted Martin, who was a mathematician, not a great mathematician, but a very good mathematician and a very decent human being. And he was later on chairman of the faculty. But on the whole thing MIT dealt with these people as human beings and they listened to them and not to McCarthy. And then I think the major clash between MIT and the McCarthy envoys came when (Roy) Cohn and (David) Schine appeared at, I think, the Lincoln Laboratory and asked for personnel data. And Dr. Killian showed them the door. And that certainly made an enormous impression. And it made an impression that I wish Harvard had imitated, but they didn't.

EM: Can you say a few more words as to how Harvard reacted to the situation?

WR: Well, there were various different places. I don't think they discharged anybody.

JR: Well, I'm not sure of that.

WR: I'm not sure, there may have been one, one psychologist.

JR: What about Wendell?

WR: Yes. Wendell Furry I think was put on, I don't know...

JR: Long-term leave.

WR: I just know what the feeling in Cambridge was. I don't know the details of that. At least I can't remember them now without looking at the papers at that time. But, of

course, for MIT to be that tower of war research to take that position, that was something different. And I think that Zacharias as chairman of that committee at MIT played a first-rate role. So that was the way in which MIT as an institution felt.

The other thing is that -- I don't know what particular date -- we got a letter from -- I can't remember what that agency was called -- but we got a letter that I had been refused security clearance for the mission that I was doing for the Navy. I had finished my report and had sent it in, and I didn't need clearance any more. But that's the way it was. And so when we got that letter, we consulted with Dr. Killian.

JR: *You* consulted with Dr. Killian.

WR: Yes. I consulted with Dr. Killian, and he suggested -- he wanted to know, am I going to take it just simply like that. I said, no. I haven't done anything that I had any problem with, I hadn't violated any security sensitive information or anything, and I think the letter had already in it a lot of charges -- not yet, no? Okay, why don't I let you talk about this since you were much involved...

JR: No. Go back to finishing the story with Killian.

WR: So Dr. Killian suggested that we talk to the lawyer of the university, and that he would suggest a lawyer that we could get to give us legal advice in my case. And so we got in touch with the lawyer of the university, and he suggested that we go to the lawyer whose name was Roy, who was a Beacon Hill lawyer and later judge. And we came to see him and when we told him the story as to what had happened, he just couldn't believe it. He had never been exposed to -- he hadn't read about this, he hadn't heard what was going on. He was the one who got the particulars of the charges -- and we wrote out in a lot of detail what the answers to these particulars were, and we, I think, contributed to his education in a very significant way.

EM: What were the particulars?

WR: Well, we'll come to that.

EM: Were you a U.S. citizen at that time.

WR: Oh yes. This was in the '50's. And I can only say that if I look at my list of publications for a couple of years, it's rather thin because we spent our time trying to debate the particulars by detailing answers to them.

JR: In that connection, one might want to note that a lot of jobs that normally would be done by the lawyer -- but we basically couldn't afford the lawyer, to begin with, let alone support his doing any legwork on the particulars -- and so we took all of that work onto ourselves, which both left a lasting impression on our children, especially our daughter, and left an impression on Walter's scientific output as well.

WR: So this was the time, fortunately, that the anechoic chamber was being built. Particulars, as far as I was concerned, they started with my father. And I think we didn't talk about this before.

JR: Did they start with him in the particulars or only in the hearing.

WR: That may well be. Honey, your memory of that is probably more vivid than mine.

JR: The particulars included things like, you attended a meeting in such and such a locale of suspicious nature. You had a meeting of a Communist cell in your home. So what you had to do is try to remember who was at these things, try to get them to check their schedules to see what their view of what kind of a meeting it was, and all of this kind of thing. And, fortunately, there were Harvard professors who were at those meetings too. And, fortunately, some of them had very good calendars.

EM: What kind of meetings were they in reality?

WR: These were meetings -- this was the period in which the National Science Foundation was being considered -- to get into existence. And people in the community in Cambridge were strongly pushing in the direction there should be a National Science Foundation. So the particular meeting at our house that I think was the cause included two presidents of the American Academy of Arts and Sciences, Harlow Shapley, the most famous astronomer at that time, and another one who was Kirtley Mather. And this was interpreted that I was involved in trying to push the National Science Foundation, creating it, so we had invited other people who were interested in this to talk about it.

JR: But one meeting also did involve a discussion of Dirk Struik's case.

WR: Yes. Okay, that's two particulars. Then we should deal with the particular that you...

JR: I wasn't -- at least I don't recall being in the original list of particulars, but when you investigate the particulars and you go with your lawyer for a hearing in appeal of this decision, then you send letters, or you get people to send letters, both as to your general character kind of thing, such as my father did, for example, and so on, but also about what was in the original particulars. And at the hearing people also come to testify. And my father being in California did not go to New York for the hearing, but wrote. Walter's father went. He lived in New York. And I don't remember whether the committee demanded that his father be present or whether he came voluntarily.

WR: I think he came because he was terribly upset, [JR: That's for sure!] and he wanted to be there just in case.

JR: But certainly someone who had gone through keeping a step ahead of Hitler all the way was somebody who was very worried about being in trouble somehow with the law. And the kinds of questions that I got asked in the hearing were, You used to

subscribe to Consumer's Union. Why did you stop? I didn't have any more money to spend. When you were at Occidental College, you belonged to such and such a youth group. When you transferred to UCLA you didn't join it. Why not? Were you going underground? No, I didn't find the people at UCLA equally compatible. Et cetera, et cetera.. His father, tell the kind of questions they asked him.

WR: They asked him, You used to do business with the Soviet Union when you were in Berlin. Explain it to us. And he, I mean it was business that he did. And it had nothing to do with politics. As far as I was concerned, and I think with respect to MIT, the thing that impressed me the most is the people from MIT who came to testify on my behalf. (We were really very stupid that we didn't keep the Bill of Particulars.)

JR: Right. Well, we might have it in our files, but I haven't found it.

WR: There was Gordon Brown who came. And he was asked that question, How could you give a man who doesn't have security clearance tenure, and he tried to explain it to them. And I think Al Hill was there too. And was Jerry there? No, Jerry wasn't. Then there was a friend of ours who was a big real estate operator -- Kargman. I mean, the whole thing seemed so unreal to everybody. But it was this fact that MIT stood up -- as long as I was willing to appeal, they were willing to stand behind me.

EM: Now I thought this all started because you received a letter saying that your security clearance was no longer valid...

JR: Not no longer -- denied. He never had it.

EM: But you never had it, so that's -- I don't understand that.

WR: Well, in connection with the project that I did for the Navy about the noise, it turned out that they should have given me security clearance on that, even though on the

carrier, when I was there in the night I couldn't -- I didn't have access to anything. But somehow the bureaucracy of the security clearance got to it only after it was all finished and the report had been written. But as far as this was concerned, my little group suggested the formation of a committee at the National Academy Research Council that should follow what we had found in terms of the effects of noise. And that committee had a board of a few people, but because I didn't have security clearance I wasn't elected to that board. So it had effects in that respect that were continuing.

EM: So you were fighting to be on this board?

WR: No, I wasn't fighting about that. I didn't know that that's why I wasn't on the board. I didn't understand. Here I had done the report and they approved it, and so on, but the Academy at that time was not a very courageous organization either. So I saw the difference between the way in which other universities and even academies -- not the Academy of Arts and Sciences; they were courageous -- but the National Academy wasn't. The National Academy was all on government money at that time and they didn't want to do that.

JR: Later you became a member of that committee.

WR: Yes. Later. Not only the committee but also on the board, I think. But anyway I was chairman of so many committees at the Academy people forgot it. But in that period -- this was a period where it hit us not only psychologically, but it hit us in a way that, You've done the job and nobody talked about security clearance ahead of time -- the Academy -- when they asked me to do it, nobody said anything about security clearance -- but that after it's done, here is what happened. And this was a -- we'll have to find out what was it -- there was a special board that existed not only for my case but for quite a few cases. In terms of my own feelings about MIT I think this sort of imprinted, and that people like Brown and Hill -- Hill was the director of the Lincoln Laboratory at that time -- stood up. And what you read in the newspapers,

this was unheard of. So I think even though Judy wasn't always happy with MIT, but she certainly also felt that there was a great deal of difference that way.

JR: The feelings that Walter is expressing are sufficiently general in our reactions that when he was out of the country, at the dinner for retiring faculty and I went to it, all the retiring faculty members got up and made a speech about this or that, and I got up and made a speech about the fact that on the one hand MIT was the only thing I was ever jealous of because my husband was so committed to MIT, I felt, because of that period, that I could never get him to move any place else that I might want to go.

WR: I hadn't remembered that, darling.

JR: You weren't there. (laughter)

WR: I know. But I don't remember anytime anybody retiring having his wife talk.

JR: No. I don't think that's ever happened. (laughter)

WR: She is something unique. In that period MIT hired people on the basis of their qualifications and did not search for people who were cleared -- even though we had security clearance in several laboratories. Finally, with Jerry and me in office, MIT decided not to have any project that needed security clearance on the campus. If it did, it should be at Lincoln or at Draper Laboratory.

EM: When did they make this decision?

WR: Well, I think after the years of trouble. So MIT, if they hired people that would need security...

TAPE FIVE, SIDE TWO

WR: Even during the period there was still security clearance on the campus, we were very careful to tell people, If you have trouble or if you don't want to get security clearance, this project is not for you. And I think that MIT in that respect was very lucky in the choice of its presidents. Killian, Stratton, Howard Johnson, Jerry, Paul Gray, they all stood up for that. Paul Gray didn't always feel comfortable with the political issues that were raised, and so on, but he was a very straight shooter. And he wouldn't -- I mean in the years of trouble, he was a very firm person with respect to the radicals, and so on. But he always wanted due process. And that's what he has stood for under any circumstances, whatever his political views of that were. I think that Killian was Killian, and nobody could imitate -- or could come up to this kind of a person who comes from where he came from and who doesn't have really an academic status, but of course who also became science advisor to Eisenhower, and who was the one who later on created a lot of what was the famous, not prize -- I'll look that up for the next time -- anyway, who worked with Eisenhower and with the military and so on, and who picked Jerry to work with him on many of the surprise attack things and so on. So he was something that was unique in his way. And Stratton was a person who had a broad education. We once went to his house, and he showed all those books -- when he was young man, he came by boat to MIT from Seattle, and then he was very interested in anthropology. And he and his wife -- and he got married quite late -- went on an expedition...

JR: ...for their honeymoon...

WR: ...for their honeymoon. Yes.

JR: I was very impressed by Stratton at a speech that I attended with Walter when he made a very big point in the speech of defining MIT as a university polarized around science. Both "it's not an engineering college" is in that; it's a university, and yes, it's polarized around science. Yes, it's a great scientific institution. But he, I think -- for me, at least -- formulated the idea of a broadening of MIT, much of which took place after his presidency but which certainly got its groundswell, I think, then.

WR: Well, I think there are some people who would think it was Killian's formulation. I have to look that up.

JR: Stratton used it.

WR: He certainly used it. And he was probably as a speaker more persuasive in presenting it. And the fact, of course, was that Stratton went then to be chairman of the board of the Ford Foundation, and that was the period in which the Ford Foundation was a great agent of change in this country. It was really the first foundation that importantly went in that direction. And then Howard Johnson was a very important person during the years of trouble. He kept that place together in a way that probably all of us, including Jerry, thought that nobody else could quite have done. And of course Jerry is Jerry. So MIT has been lucky in that respect. And I think that if MIT's leadership would have changed very substantially, my commitment to it might not have changed but I wouldn't have given it the same time and effort. And I do feel that during the years of trouble that was part of my commitment to the place, which said that we do not want the police on this campus. We want to try to discuss, and then there was -- we shouldn't just talk about the students. There were quite a few faculty members who also felt that MIT should go out of the business of dealing with the military. And, of course, during the Nixon period, MIT's Wiesner and others appeared on the enemy list. And Nixon gave instructions that MIT's support from the government should be curtailed. So this all fitted together, and we felt that people at MIT, no matter what their feelings were about the war, should not get the impression that we were part of that war machine. And I remember Jerry and me walking in a manifestation of 250,000 people along Mass. Avenue and into town. And that was an incredible feeling. But that was kind of the high point of that. There had been an AWOL soldier who got into the Stratton Center, and Jerry and I used to spend evenings and parts of nights there debating these people. And the leaders of the older graduate students or the young faculty, with whom, like David Baltimore, we always had close relations though they disagreed with us. I mean they wanted to do things fast, and we said, well, we can't quite do it that way. But in that period the

conception of MIT -- these people didn't have quite the experience that we had had. I mean Jerry was not in trouble with the security clearance, but our friend (Louis) Smullin -- the first party we went to was given as a party because Smullin had passed security clearance.

EM: Now when was this?

WR: That was in '51. That's all this period.

EM: I want to go back to when you were talking about your hearing and everyone was testifying on your behalf. What happened after that?

WR: This hearing was in February. The one thing that I remember is that -- I can't remember the year, though we have it somewhere -- the next month, a month to the day, my father died.

JR: When we were at the hearings, the lawyer asked when we could expect a finding. And they said, in a month. And his father died, and the hearing result, positive, came the next day. And clearly, the strain of that hearing and not knowing the result was no help in preventing his father's heart attack.

WR: But I will continue a bit with what happened after that. In the year of the Sputnik there was a big meeting in Wood's Hole that the Academy had organized, which was trying to deal with issues of the Cold War, which had an enormous number of Academy members there. And not only -- I mean when I arrived there, the question arose, did I have security clearance. Not only did I have security clearance, but I had top secret clearance. And not everybody there had that.

EM: As a result of this hearing you were given top secret clearance?

WR: Well, there had undoubtedly been things in between, but I don't know. But I think my father died in March -- but what year? -- we'll have to look it up. But you asked what happened afterwards. The system corrected itself. Of course, by that time, I think, there was no more McCarthy. At least he didn't have much influence anymore. But I got back to what happened in the early part of the McCarthy period -- that was the students and faculty who were against the Vietnam War in the years '68 and '69, and so on, they didn't have the memory of this other period. And the problem that today students who talk to us lately about the memory of Vietnam, the students in general have very little memory of Vietnam. Some of them remember the Kent episode, but memory is...

JR: Well, it isn't memory because they weren't conscious or alive...

WR: But they haven't learned about it. As they don't know about Wiener.

JR: Generations in college students are rather rapid.

WR: When did you -- I'm not interviewing you, you're interviewing me. (laughter) Then after the initial big uprising in terms of manifestations and so on, some of the groups felt that not enough had been done. MIT, for instance, didn't cancel exams but postponed them. And the students wanted to do something that would be a symbolic event against Kent State.

EM: This is in the late 1960's?

WR: Yes.

JR: After Kent State.

WR: Yes. And I think you probably have enough data on that in Howard Johnson's book.<sup>2</sup>

EM: Okay. And we can talk about that a little later. Going back to the McCarthy era, you had mentioned that Lou Smullin had a party when he finally got his clearance. What else was going on? Who else did this affect?

WR: There were quite a few people, I'm sure, at MIT, but I just had come to MIT and we hadn't been to a party. So I think the Wiesners invited us, Come along with us. So we went to Belmont -- up on a hill -- it was Al Hill's house. And I think when you talk to Lou Smullin you can find out others who had problems. But Lou Smullin was a guy who showed that day that a big weight had come off his shoulders. And I think we were a little puzzled by that until we learned -- had to understand ourselves. But there were undoubtedly other people there who had the same problem or comparable problems.

EM: Did you ever learn how these particulars were found out?

WR: No. I don't know. I tell you, in all my optimism about MIT, I wouldn't necessarily have thought that there might not be some people in MIT -- not necessarily faculty -- but some people who sort of were watching out and might report. And I think that was part of the story of the McCarthy period that people who had reasons to be unhappy or who believed that McCarthy was doing all right would report people. And it may well be that we have never gotten the history of that figured out yet. Not just for us, but -- it's amazing how much I have forgotten about that special board -- it had a special name -- that interviewed me.

JR: Like Hearings and Appeals Board or something like that.

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<sup>2</sup> Howard Wesley Johnson, *Holding the Center: Memoir of a Life in Education* (Cambridge, Mass.: MIT Press, 1999).

WR: Yes. But I must say that the experience of that day was quite something, but especially it was something because we learned the absolute absence of any of what we would consider relevant facts. And the impression that it made when my father died.

JR: But also the effect on our kids. Both in terms of lost parental time and otherwise. When our son was in high school, he once came home and announced that he was going to subscribe to a socialist newspaper and our daughter said to him, You can't do that. Don't you remember?

EM: This was way after?

JR: Way after. So there are these kinds of poisonings that take place and don't go away so easily sometimes. The other thing is that our kids were aware, as were we, of the fact that all of our neighbors were being questioned by the FBI. And that's not a comfortable feeling. Even if you don't feel guilty, you don't like having that happen.

WR: But I must say that at MIT nobody -- when I was nominated to be chairman of the faculty I didn't hear of anybody who would raise that as an issue. It's true, of course, that in between I had been at that big -- at various Academy meetings and involved in many things.

JR: What was the Academy meeting you were at? Planning the air defense?

WR: Yes. It was basically air defense. No experts. But I was there with some colleagues...

JR: [?] (laughter)

WR: Yes. Top secret -- Incredible. But to a large extent, I think, it was the support of MIT that made the great impression -- not what we said in response to the particulars.

EM: You had mentioned that one of the particulars was a meeting that you had in your home where you had discussed Struik's case. What was your opinion of that?

WR: I think at that time...

JR: It was one topic at the discussion, not...

WR: Yes. There were other -- at that time people knew that there had been accusations against members of the Cambridge academic community and the academic community, at least those people that we had -- what was the -- Unity of Science -- and people got together to discuss what could one do to stop this. But we were not very effective and we wanted to get -- I especially -- the National Science Foundation into existence in order not to depend on military alone. And in some ways Vannevar Bush was the man who suggested the National Science Foundation, but when he was no longer in the same position it needed support from the grass roots. And this was essentially a grass roots meeting that among other topics probably discussed Struik and Wendall Furry, and who was the psychologist?

JR: I don't remember.

WR: I see Well, I think that's about all I can say about these matters. Judy, maybe you have some more...

JR: I'd just broaden the perspective of MIT postponed exams -- at Wheaton College the president called a faculty meeting to set limits on what students were allowed to do and discovered in the process that he didn't have the faculty behind him. And ultimately, again right after Kent State, in one of the faculty meetings that student representatives were allowed in to, one of them got up and said, Oh, there's no use worrying about what happens at Wheaton College. That's just the training ground for the wives of the future -- not pigs, but something equivalent to that -- and this had

been told her by the head of the STS at MIT. So the more liberal students who thought they might be interested in war protest were not encouraged to be active on the Wheaton Campus. But at the faculty meeting where we set limits, the limits were that you could choose to take your finals or not to take your finals, but if you chose not to take your finals you must go out and do community work in terms of canvassing people and explaining your stand on Vietnam to them.

EM: I have one last question. It is not related to the McCarthy era, but related to one of the last comments that you made. How large a part do you think these grass roots movements made in the formation of the National Science Foundation.

WR: I tell you, I got this book from Larry [Bacow?] just the day before yesterday, or maybe yesterday, and I wouldn't be surprised if I find some information I don't have in there. We have always been involved -- not very active in politics, but we have tried to have some influence by getting peers together to see what you can do. And I think the National Science Foundation was originally founded and given very little money but it grew. I think in some sense you see more influence of grass roots influences when it comes to the Endowment for the Humanities or the Endowment for the Arts, where, if there isn't any grass roots, the people in Congress won't hesitate to essentially abolish them. And I think that's what the grass roots do, they prevent in some ways the worst from occurring and in some ways they enable the good things to happen. But it's all more a question of degree. I think, for instance, if I think of what happened in Seattle, with all the things that were done there, there I think that the so-called grass roots did more harm than good. But that doesn't mean that those issues -- I think both the World Bank and the IMF now are much more serious about some of these issues.

[End of session]