Jagdish Mehra: Ladies and gentlemen, in his book, *Teacher in America*, Jacques Barzun recalls the case of the harried professor, who after a hard day in various committees arrived at the Dean's Office and asked, “Is the Dean busy?” While it is perhaps true that the American campus has increasingly the [inaudible] of busy Deans, we have the great honor tonight of welcoming a speaker in our humanities series whose accomplishment has been dizzying.

He is Dr. Jerome Wiesner, Provost and Dean of Science of the Massachusetts Institute of Technology. Dr. Wiesner is the Senior Academic Officer at MIT and has the responsibility for the interdisciplinary activities of the institute's five schools. Dr. Wiesner returned to MIT as Dean of the School of Science in 1964 after having been special assistant for Science and Technology to the President of the United States for three years. Before his appointment by President Kennedy as Special Advisor in January 1961, Dr. Wiesner was Director of the Research Laboratory for Electronics, one of the largest interdisciplinary laboratories at MIT, in which a broad spectrum of research is conducted by professor and students from various departments.

He had been actively concerned with national problems involving science and engineering and became a member of the President's Science Advisory Committee in 1957. Dr. Wiesner remains a member of this committee. He was made director of The Office of Science and Technology when that agency was established in 1962 and resigned in the beginning of 1964. In 1958 he served as Staff Director of the American Delegation to the Geneva Conference for the prevention of the [?].

Jerome Wiesner was born in Detroit, Michigan and received the Bachelor of Science, Master of Science and Doctor of Philosophy from the University of Michigan. In 1940 Dr. Wiesner was appointed Chief Engineer of the Acoustical and Record Laboratory in the Library of Congress in Washington. There he assisted in developing sound recording facilities and associated equipment.

Shortly after the beginning of World War Second, Dr. Wiesner joined the staff of MIT's radiation laboratory as a leader of the Radio Frequency Development group. In 1945 he went to the Los Alamos laboratory where he served for a year. He returned to MIT as assistant professor of Electrical Engineering and became Director of Research Laboratory of Electronics in 1952. He has held the title of Institute professor since February 1962.

Dr. Wiesner has made notable contributions in the fields of microwave theory and communication sciences. He has participated in several studies of greatest importance to the national defense as well as in a number of international conferences devoted to the subject of disarmament. Dr. Wiesner is a fellow of the American Academy of Arts and Sciences and a member of the National Academy of Sciences.

In his remarkable book "Where Science and Politics Meet" a book that should be required reading for all those who intend to; do indeed manage the affairs of man in a technological society. Dr. Wiesner has discussed the crucial importance of having proper perspective of science in political decision making. In an area of great moment and concern where even eminent scientists and scholars have surrendered judgment to powerful agencies of big governments, Dr. Wiesner brings a sensitive and powerful mind to the consideration of the problems of living with science in an affluent technological society. In this book he also devotes a beautiful chapter to a remembrance of John Kennedy, whom he served as Science Advisor. Dr. Wiesner's concern as a citizen has moved him to participate in the school system of his home city of Watertown. Ladies and gentlemen to speak on using science for public ends, I have the honor to present a man who understands the principles of power, and the power of principles, Jerome Wiesner.

[applause]

Jerome Wiesner: Thank you very much Mr. Mehra. It's going to take me a moment to figure out whether that's good or bad. It's a great pleasure to be here at the other MIT. You have much more interesting student body, if I might say, judging by the audience than we have.

I am very pleased to be here, a little bit perplexed on how to encompass the subject that I've been asked to talk about in a period of an hour because it covers so many ramifications. I actually asked myself what the students here might be interested in, I'll try to shift gears in just a little bit and ask myself what the broad spectrum of your community, who I think seem to be represented here tonight, might be interested in the present discussions of science.

When one talks about science and public affairs or science, I like to talk about science and politics which usually has a bad connotation to people because politics seems to be a bad thing to be engaged in. Only this morning I was engaged in some so I don't think it's very bad. I

kicked the traces as a Democrat and went on the radio to endorse Elliot Richardson. I'll probably be read out of the party but I've almost had that happen to me anyways since Esquire published an article listing Bobby Kennedy's shadow cabinet, included me as a member in it. I didn't think my hair was long enough or I was young enough to qualify, but just being in there made me feel younger so it was a good thing.

But the reason you're here I suppose, the reason I'm here, is that science and technology have become the most potent forces in our society today and probably the least well understood, or at least as poorly understood as many, and forces which totally dominate what we can do, where we're going, the future of the world, in fact whether there is going to be a future. And yet very few people really understand what we mean when we say the problems of science and the problems of technology. And as a matter of fact we mean very many different things.

In the last year or two there have been a whole series of books on this subject, some of them you may have seen. One called “Science: The Glorious Entertainment″ by Jacques Barzun, already referred to, philosopher at Columbia University who doesn't like what science is doing to our world, spends a whole book saying so in many different ways. And then doesn't offer any solutions, but says in the preface of the book that he doesn't know what to do about all this but it isn't the critic's place to offer a solution, just to criticize.

Then there's a book called "In the Name of Science" recently come out that goes back over a whole series of things that have happened in the last 20 years and reads something sort of sinister into almost all aspects of it. In that particular book incidentally I emerge as a hero so I find it hard to criticize, but it isn't a very accurate book in spite of those parts that I like. Because the problem it's not because the man didn't try, as a matter of fact this is the place where I have a certain amount of guilt and I can't complain too loudly because sometime during the Spring I got a letter from somebody I didn’t' know, you might say in explanation I get 40 or 50 of these letters a day of one kind or another asking me to do something and if I didn't have anything else to do I couldn’t do all the things I was being asked to do. This man asked me to review a book that he was writing on science and policy because he thought I might be able to give him some advice. And I wrote back saying I was sorry but I just didn't have the time to do it without even looking at the manuscript he'd sent.

Later when the book came out and I got a copy and glanced at it, I was horrified to think that I'd

allowed that much error to sneak into print without raising my voice. Then LIFE magazine asked me whether I'd read a review of the book. And I thought that would be pretty dirty pool, because the thing that I had the most complaint about was how inaccurate the history was and here I'd been offered a chance to help correct the history and hadn't accepted it. So I finally decided it wasn't really right for me to comment on that book.

Then there is another called "A New Priesthood" which sort of says that those of us who have been involved in science and public policy helping the country decide what to do and how to do it and so on, are a new priesthood with some mystical power that goes beyond the control of President, Congress. I must say when I was working in Washington I never felt that I had very much of that power. I kept trying to do things and being sort of frustrated in the process. I think this is the experience of most people. In any event this man is sitting on the outside wanting to write an exciting book, picked this particular theme and selected kind of information to make it go.

I could list many more. There have been a lot of Congressional investigations; oh I forgot to mention a book called "Where Science and Politics Meet". There is a book called the "Prime Minister and the Professor" [actual book title is “The Professor and the Prime Minister”] goes through the exactly same set of problems in England. And if the Russian leaders, scientific leaders, write a book which I'm sure they wouldn't, not a candid book anyway, they'd probably write one not very different.

Then the Congressional committees of which there have been oh a dozen or so in the last year, one on the conflicts between research programs in nation schools for higher education, another on pollution. You just name it then there has been a Congressional investigation of some aspect of science. And these, each one of these is so different and deals with such a different problem that you feel a little bit like the blind man and the elephant when you say "Can these people all be talking about the same thing, something called science?" They seem to claim they are but yet each one has a completely different set of problems that is worrying me.

I might say that most of these books neither science or scientist come out very well. Maybe we should write more of them I guess. But each person has picked a narrow area of science and public policy and in each case he's critical and each case he's worried about the consequences, and in each case he thinks the scientists' contribution has been less than wholesome. And yet

the country continues to spend more and more money for research and development. This year the nation as a whole is spending 21 billion dollars on research and development. And of that, the Federal government is putting up some 16 billion dollars. Now there must be something very wrong with all of us if a thing which is so undesirable, and so lacking in benefit, and so dangerous, and this is the general theme of all these books, is still costing us, our government elects to spend 16 billion dollars a year on it. And we not only cheer but encourage them to do more.

I think there is a conflict here which can be resolved if you look at the very different things that one supports science for, and also looks at the historical motivations. All of these books that I've talked about have been dealing with a past, and reflect a sort of 20/20 hindsight. That is, the book "In the Name of Science" for example, complains about the things that our military did in the period 1946-1960. Complains about the things that scientists encouraged the military to do. In a sense complains about the arms race. Now as a matter of fact I was deeply involved in that particular period and many of the things that he's discussing, and I often was on the minority side, which as I say he thought was the right side when one interprets his book as I have. And yet I don't feel that the motivations of the people who did things I disagreed with, or the motivations of myself when I was doing things he disagreed with, were wrong. I can look back and I can see an awful lot of mistakes that we made, just as I can in my personal life. And I suspect each of you can find a few if you try. But at the time we were dealing with a problem we couldn't look back at. We didn't have the advantage of 20/20 hindsight to see what was the right thing to do. And in science and technology, the application of science to do useful things, what's obvious afterwards is often not any more obvious than in any other aspect of life.

The thing that's perplexing, there's several things I think that are perplexing to people about all of this, most non-scientists have a completely false notion about the way scientific progress is made or about the, I'm about to tell you some professional secrets I guess, about the intellectual qualities of scientists. They say for example, in one particularly bitter fight in the White House in which we'd argued all day about whether a nuclear test ban was safe or not. With about five of us reigned on one side and five reigned on another, and each of us known to be reputable scientists in our own field. The president said "That I don't understand. How people who can be so right and so logical in their own business can have such violent differences of opinion when you get away from your own field." [inaudible].

Well I could never convince him that they were no better in their own field. But it's true you know. Most people have a false image of the way a scientist works. They think that he is brighter than people first of all, that he is more logical, that he can sit down say "here's a problem and lock myself into a room", it wouldn't hurt if some did as a matter of fact, "and I'm going to think about this until I have an answer." And he locks himself in the room with some peanuts or something and comes out at the end of the day having found a solution.

Well this is just not the way he works at it. He has some half formed ideas of something he's interested in and he starts to work at it. He has an idea and he tries it out, it turns out to be wrong, his experiment doesn't work. So he thinks about it, tries another one and it doesn't work. He goes on doing this. Well sometimes what happens is in the course of doing a wrong experiment he discovers something extremely interesting. In fact most of the very interesting discoveries in science have been made that way, at least in the past very many of them were. And so he forgets about his original question and goes off to deal with the new and exciting thing he's found. If he isn't that lucky to make that kind of an accident he sticks with his original problem and slowly all the wrong things get tried and eventually he finds the right ones.

Now, a bright scientist is one who makes fewer mistakes and who recognizes them quicker. When I was very much younger I worked at the Los Alamos laboratory and I had an office next door to Enrico Fermi. And Enrico Fermi, as I'm sure many of you know, is one of the half dozen of really great scientists of the last half century. He'd rank certainly with Neils Bohr, just under Einstein, but you couldn't think of a half dozen people who have made a greater contribution to science, to physics, in the last half century than he.

He used to come into my office and say, "I want to try an idea on you." And he'd start to talk and put it on the board. Then he'd go away. And I couldn't understand why he was coming to try his ideas on me and I also noticed that he usually saw what was wrong with them before I did. And it was obvious he just needed an audience to talk to. And the first thing that impressed me were the number of silly things he came in and talked about. But then slowly I realized how fast he discarded them too. About 1 in 50 of these would be leading him in the right direction and get him on the thing he's working on.

Now I've tried to caricature this a little bit, but not very much. Now this is the intellectual process, whether you're a scientist or a poet, that's even the way I write. The first ten times I look at a

sentence I don't like it very much and I'm sure many others of you have the same experience. But slowly you work at something and you get to understand it. The more you work at it the better you understand it. You move along, you make some progress. Well people don't understand this very fundamental character of the way scientific progress is made, and the fact that each man has the past to stand on and adds a little bit in his lifetime, and the next man comes along and adds a little more. So that given a new problem which he hasn't seen before, whether it's a scientific problem, or a political problem or an economic problem, scientists are going to guess just as much as non-scientists.

Now if it is a scientific problem the scientist can be intelligent about his guessing because he knows what is possible and what isn't possible, and so his range of guesses are probably smaller and much more productive. But when you're trying to decide, well think of some of the great debates that you've listened to in the last twenty years in the field of military technology. Should we or shouldn't build a nuclear bomb, a thermal fusion bomb. There were bright people on both sides that said it was going to be a waste of money, that it couldn't possibly work. They started out to do it. The things they tried to do wouldn't work, but along the way somebody had a bright idea and something very different did work.

Should you build a nuclear powered aircraft? That one didn't work and after spending a billion dollars we stopped it. Tremendous debates over this for 15 years starting with the same information. Some people would make optimistic guesses about the way unknown things were going to turn out and they would think it possible. Others would make more conservative guesses and they'd say it won't work. And here you are guessing you're making intelligent guesses. This is the first point that I want to make, this is the way we get scientific knowledge. Scientists behave this way whether they're dealing with their own problems or dealing problems of the world. They're no better, they're no worse.

I think scientists have one property which I think is a tremendous advantage which all my colleagues in the government used to resent, and that is they tend to be optimistic. And maybe more optimistic when they're dealing with somebody else's problems than when they're dealing with their own. But nature in a sense has been good to the scientists, working scientists, there has been progress. They're made tremendous strides in understanding the inanimate world, the cosmos and even living systems. And so the view of a good scientist is that there is no question you can't answer if you work at it hard enough.

Now some questions are harder than others. And nobody in his right mind will ask some questions today, scientific questions, because we know that you have to answer a great many others before you move on. And scientists are used to taking a simple, little question trying to make as simple as possible, answering it, regarding it as one brick in the tremendous body of knowledge which ultimately we hope will exist. But when you're confronted with a job that has to be done you don't have the pleasure of saying "let me do a little bit today, and a little bit tomorrow."

And so there is a difference between pure research for the sake of acquiring knowledge and either engineering, that is building something that has to be done, you have to build a bridge whether you know the precise properties materials you're going to use or not. You do the best you can and you can't wait until stronger steels come along so you make the compromises that exist with the steels you have. So scientists tend then to feel "give me time and I can solve any problem."

And this sometimes can prove to be both wrong and a terrible annoyance when you get involved as I did and many other people have, in trying to deal with problems that are a combination of scientific and political or human problems. Because one carries that same optimism over in saying, “Well, there is a solution, let's get on with the business”. And anyone who's been through the political mill a few times is inclined to say it's not that simple. You have to deal with human beings and they may not understand. And they may have motivations which aren't the same as yours. And let's say the scientist whether he's dealing the Congress, I can show you some scars, or with the Russians or with somebody else, tends to say logic will prevail in the end. Whereas a person who's spent his career in the law or in politics may feel very differently.

And so there is this sort of basic conflict too between people involved in science when they become involved in public affairs. I think it's a good thing to have a few optimists around or maybe nothing would get done. President Kennedy used to say I was the only genuine, honest, I don't know quite what he meant by the word, honest, political scientist in Washington. But when I, but the fact of the matter is that most of the things that I had to do had a great deal of technology or science involved. Almost all of the problems I dealt with when I was in the White House were highly technical.

They had terrible political aspects to them like should you put that new laboratory in Houston, or Boston, or somewhere else, Rhode Island. Which were harder to solve than the questions that the laboratory was designed to ask. But when I came back to Boston after three years in Washington, a newspaper reporter asked me whether I could give him a contrast between the two jobs. The one running the School of Science at MIT, the other being the President's Science Advisor, and I realized something then that I hadn't appreciated while I was working on the job and that was the thing I just said. There was rarely a time when I was dealing with a problem in the White House that didn't have at its core some technical or scientific question. It may have had a lot of other questions too, including fiscal. But and there was rarely a time when I was being the Dean of the School of Science that I had anything to do with a technical question. I was mostly being an amateur psychiatrist, or a fiscal officer, or refereeing a space fight between somebody. And people wouldn't, you heard earlier about the faculty's view of a dean, they wouldn't condescend to talk about a scientific problem to a dean.

One other, one other very important, well let me give you two other points which I think you should realize, general points, and then I'll talk about some of the detailed issues. There are two words that are about as badly confused as any two words in American vocabulary, and they are science and technology. I bet there are a couple of you in the audience who can tell me the difference. Most scientists, by now, and technologists, are engineers and use the two words interchangeably. And I think it's really very misleading, because by science we mean the search for new information, new knowledge about some physical phenomena, some living phenomena: how does something work, why does something happen, why does it behave that way. In the field of technology we are interested in using some knowledge to do something useful: make an electric lamp, or a transistor, or an airplane. And we depend on scientific information to give us the tools for doing this.

The fact of the matter is that technology really developed and existed before there was any major amount of science loose in the world. The steam locomotive, the spinning jenny, gasoline engine, these are all inventions which one should call technology. They're made by men who use the knowledge they had of the world around them to harness some physical forces and some material to do something practical that we wanted to do. But they didn't really understand at the beginning what the thermodynamic laws that made a steam engine possible were, or what the efficiency of it would be, or very little about the strength of the materials they were

using.
This is true of the gasoline engine too and many other things and even many of the great medical discoveries were quite pragmatic.

What has happened is we've discovered, these are the people I've described, the old inventors, were creative people who took what existed in the world around them and were sort of gadget minded, could put it to work to do something clever and useful. The technologist of today, more likely than not, is doing the same thing. He's a clever inventor, but he lives in a different world. He doesn't live in the world of levers and wheels and steam that he can see. He lives in the world of some branch of science whether it's chemistry, solid state physics or nuclear physics. And he uses the ideas that are loose in that world to invent useful things. But he's doing exactly the same creative kind of effort, making the same creative effort that the old fashioned inventor did.

But these are two quite different things: the man who is looking for knowledge because he's curious; he wants to know why something goes or he wants to win the Nobel Prize, or wants a promotion to assistant professor, associate professor, has all kinds of complicated motives, among them the fact that he likes doing research.

But the technologist or engineer has a quite different point of view: he is a fellow who is sort of motivated to do something; he wants to build something, he wants to make a better radio, or he wants to make a faster airplane, or he wants to make an artificial heart. He wants to do something and he uses the scientific knowledge he's given in school or that he picks up on his own to put this information to work.

So you have two quite separate things. Science on the one hand and technology on the other. And we boil them all together now and we call them science for the most part. And so we don't know what we're talking about and we're confused. When I was, sometimes you want to talk about both when you're talking about the budget as for example I just did, it's useful. But I got, when I was working in the White House, I got so tired of this word "science and technology" that I offered a three hundred dollar prize to anyone who would come along with a word that would describe the two things taken together in one word that was acceptable to both professions.

The scientists are highly offended if you accuse them of being technologists and most

technologists don’t like to be called scientists. They think what they're doing is useful not frivolous. And so you're in my position; you use both words, and sometimes you say technology and science, and sometimes you say science and technology and that way you don't offend anybody. But no stage did I ever get a taker, anyone come in and offer me any word, say nothing of a word that I thought was a useful word, so we go on saying these two things.

In the budgetary part of this business, science turns out to be a relatively small part of the total cost, and yet everybody's talking about the high cost to science. Of the sixteen billion dollars that the federal government spends on science and technology, about one and a half billion go into research. The rest go into developmental activities: trying to make a better airplane, trying to make a better pump, trying to make a better nuclear reactor and so on. But everybody keeps saying these things are so complicated. How can any ordinary human being understand whether this makes sense or not. How can a Congressman, they keep repeating, know whether this a sensible way to be spending money, and they sort of throw up their hands. And yet the fact of the matter is that 14, 14 and a half billion dollars of this money is spent on applied things, maybe a little less than that, certainly 12.

And one can ask questions about them, do you, maybe we should. Do you think it's a good idea, as citizens for the country to spend 2 billion dollars over the next ten years to develop a supersonic transport airplane? You haven't, it's a pretty straightforward question. The answer may be complicated, but the Congress can understand the questions and the answers as well as the scientists. We can tell you whether or not it can be made, we can have a big argument about how fast it will fly, whether it will be Mach 2.5 or Mach 3 and whether it will weigh 350,000 pounds or 370, and whether it's going to cost one and a half or two and a half billion dollars to develop. But you can get a range on all these properties and then people who represent the money bags ought to be able to make up their mind whether it's worthwhile. And this is true of most technological things: they are quite different, they are relatively easy to understand and in the field of basic research one does have to depend, I think largely, on the scientists to make the decisions.

Now one last generalization, goodness I won't have time to get specific. One last generalization I'd like to make has to do with society rather than technology or science, but has come out of my own interests and research. I've been involved as you heard during the introduction in a field called the Communication Sciences. I was interested in communications in computers and so

called feedback and control processes. And in this work as it evolved some of us became quite interested in the sort of the analogy between sort of thinking and learning as done by living things and some of the artificial things we can do with a computer. And we got quite interested in various properties of learning systems. And people working in this field have discovered a few very general and interesting properties about learning systems which I think are both interesting and important.

First of all I think one can safely say that any learning process is what we highbrows would call an ergodic process, and what is just that random process the scientists employing. Almost anything you want to do new, you make the best possible judgment and guess and then you try it. You usually find that it isn't right; if you've got a lot of information, if the step you're trying to take isn't very far in advance of the last one you took, you're likely to do pretty well and not have to make very major corrections.

If on the other hand, the problem somebody's given you is one that's totally new, outside your range of experience or at least quite different, it's very likely that your first assaults on it are going to be quite far off the mark. And you're going to come back and this is, you know just remember when you were in school. I think there may even be some students in the audience still. You get a problem or you get a quiz and you say how shall I go at this and you start out on the basis of knowledge you have. And after a while you discover you were partially right and partially wrong and you back up a little bit and make a turn, the way I got here tonight for example.

And you keep correcting yourself and this is the nature of any single learning process that anyone has ever observed. They are trial and error and sometimes more error than trial. The more complicated the problem or the more complicated the situation, obviously the more randomness that you're going to have. Well I made the mistake one time, really I think Norbert Wiener was a colleague of mine, got thinking about these ideas as they applied to social systems and made the mistake of remembering this when I was working in Washington. I began to think of everything we were doing in the government in these terms. Namely this government is a great big learning machine and I really, I'm saying this quite seriously. I think any society is a learning machine. It's trying to learn how to better satisfy the wants of all of its people. Sometimes you wouldn't think so but.

And any learning machine has to have, you've heard the word before, feedback. You make a move. If you can't correct the move, if you can't judge it in terms of the target, in other words if you have no measure. Well suppose you're shooting a gun. Just a simple act: you pick up the gun, you line it up and you pull the trigger and it misses a little bit. Then you look and see where you missed and you try to correct and you shoot again having moved the gun around. Well this is called feedback. You get some information about how close to the mark or how far away you were and then you make another try. Well, if you don't get very much information, suppose you're shooting at the target but you can't see the bullets, the target's too far away or the pellets are too small so you can't see what you've done. Well you certainly can't improve your aim as a result of your previous experience and you'll just shoot all over the target.

Now any learning system that has no feedback isn't going to learn. And the better the feedback in the learning system, the better it will work. The first real system of this sort which we tried to make, in fact we did make, were the so called radar gun directors during the war. In which we would have a radar that sent out little pulses of electricity, got back echoes from airplanes so it knew when it was pointing at an airplane. It would hunt around till it found one, then stop and we would put some measuring devices on the radar antenna which would tell us how far up and how far sideways it was, an angle and then measure the distance. And we would use this information to tell a gun where to shoot and if we could see the gun shell we could then get information to correct it. And if the thing wasn't pointing right we could get error signals and redirect it.

Now in this particular machine when we first started we had all kinds of trouble because instead of moving, following the radar smoothly, it would sort of wobble all over the place. And we discovered this was because the feedback information was too slow. The signal saying correct yourself came after the thing had already corrected itself too much anyway. And so it kept on going and so it wobbled around like that. Now we learned after a while how to correct these things, but I think the same phenomena still exists in a lot of systems, particularly in big social systems in which the feedback is so poor and so slow that bad errors have to occur and the thing oscillates all over the place.

This used to be very true in the economic system which is probably the most positive, highly coupled system in the social systems, of the social systems that we have. And we before we learned how to do some tinkering with it, it used to oscillate very violently and we had business

cycles. The economists claim that they know how to control it now and it's not going to do that. And I think that they do know how to keep it from going down too far, but at the present time they seem to be having a little trouble keeping it from going too far in the other direction. And I suspect it's not because the economists don't understand what they ought to do, but they made the same mistake of thinking that politicians can be logical, that I used to make. And politicians don't mind improving the economy, cutting down the unemployment rate and so on; but when the medicine required is doing things which go in the other direction, you'll find a great deal of reluctance and the economists are having problems with that.

In any event my own view is that this is the nature of all the social systems with which we are dealing and that most of them, including our own, the feedback isn't really very good. And the problem is so complicated. There are two problems, one the feedback is poor and takes a long time, secondly the problem is so complicated and usually so strange to most of us, even though we've been working on them for a long time; because of the vast number of interactions, that any given move we make is not very likely to be precisely the right one. And even if it is, it won't be for long because this particular thing that you're dealing with, let us say a military weapons system or a some process for improving water quality or something, will not live in isolation very long. And a whole lot of other things will be going on in the society at the same time. And what might have looked like just the right solution two years ago, namely giving money to universities to improve the quality of education of scientists, or encouraging a given industry in a given location because unemployment was a serious problem and would be taken up by putting those contracts there which sometimes happens. Not often enough in this area I gather from some of the complaints I've heard from your congressmen.

In any event things keep changing and all these interactions take place and they are so complicated that no human being can know what to do. So that, you have, a philosophy I developed, is that you have to expect to be wrong. And the most important thing is to try to be perceptive about what you've done and look very carefully to look for signs, for the first sign of your errors. This is not an easy thing to get other people to accept or believe because the last thing any man in political office wants to do is ever admit he's made an error. If you can change your direction without admitting the last one was wrong, you don't have much trouble persuading people to change. But if you have to start by admitting you made a mistake, it's really something that you can't get people to do.

Now one last criteria of a good learning system, which also relates to some of our technological problems, is that you'd like the mistakes that you make not to be too serious if you can possibly avoid it. And this means several things: you'd like the experiments to be small, you'd like your feedback system to be very sensitive and if you want the experiments to be small, what'd I say? [laughter] [no audio]. Must be a democratic chief of police.

Jagdish Mehra: Sorry for interrupting. Is Dr. Mendez here? Dr. Mendez. This is an emergency call for Dr. Mendez.

Wiesner: That gave me a chance to change my tape. We didn't plan it that way. I have been recording this.

The, I was saying you want a lot of experiments, you want them small so the errors are small. Now just by accident this happens to be the way the American political system seems to be working. When you compare it, I tried to argue this beautiful theory once with a Russian. He didn't accept it. But the, we have a system in which large numbers of experiments are going on simultaneously, at least in the private sector of our society, we have a very sensitive measure of whether things are going right or not and that's the profit motive. If you bet on something that society doesn't think it wants, you don't stay in that business very long.

Now these things don't influence the government and you have a much poorer feedback system in the government, and therefore we do bigger experiments and we make bigger mistakes. But at least in our society, the government is more sensitive to the desires and needs of the people than in many of the others. And somehow this elaborate machine does seem to learn to do things a little better as time goes on, and that is its whole purpose.

In any event I used to remember all of this when I was in Washington watching the President struggle with almost impossible problems. And every once in a while I'd sort of think of this machine hunting out of control, sort of like the dinosaur, and I guess I'd get a smile on my face. And one particularly bad day when it was all over the president said "How the hell can you smile when you're listening to all that?" and I said "I was thinking of something else." And he said, “What?” And I told him this story and every once in a while after that we'd get into one of these things and I'd see him smile and look over at me. So I thought the theory had at least done a little good. It had relieved the terrible burdens of the president and gave him some

understanding that maybe it wasn't all his fault that things weren't going a little better.

Now with all of that as background let me give you a five minute speech about problems of science.

Prior to World War II the government didn't have a great deal of dependence on science and technology. The country did, we were using it in private enterprises. We had the great General Electric laboratories and the Bell Telephone laboratories and the DuPont laboratories and many others. Where science was being exploited to do useful things for the civilian population, for economy and for profit. These laboratories had a tradition of developing their own basic knowledge and putting it to work doing basic research in the fields of their interest, but there was not a great deal of research being supported by the government. There was basic research in the life sciences and the physical sciences in the universities, but there it was really then done sort of for the pure love of it and the poetry. People were as interested in science for science's sake as the poet was interested in Shakespeare for the pure beauty of it.

It wasn't until World War II when the big national laboratories which were created by bringing together the scientists and engineers from our universities into a half dozen very major laboratories, that we discovered the power of large organized research teams. Radar, better airplanes, the nuclear bomb, computers, all emerged from these activities during the war. And after the war the government realized that we were in for a period of protracted tensions and we had better maintain the kind of scientific and technological activities that we had developed during the war; keep us militarily strong so that if an emergency arose we wouldn't have to start from scratch and build up.

We began to spend fairly large sums of money for this purpose. The Defense Department and the Atomic Energy Commission had big laboratories of their own and developed, worked on problems of weapons. But they also recognized that these were not very good places to do the fundamental research, which was necessary if they were going to keep having new ideas on which to base more advanced things that they wanted. And so they went to a half dozen or so of the major American universities, MIT, Harvard, Columbia, Caltech, Berkeley and maybe two or three more, University of Chicago, and asked whether we wouldn't continue more substantial basic research enterprises than we might of had we gone back to a purely peacetime activity.

I remember at MIT when the representatives of the Army, Navy, and Air Force came and asked whether we wouldn't do this. Well we were disinclined to do it because we felt the most important thing for us to do at that stage was to get back to the business of teaching. And we sort of said no, we didn't think we wanted to, and down on bended knee they asked us whether we wouldn't take a little of their money to support our research. And we had some compassion and we finally agreed to take $600,000 a year. [laughter]. You think I'm kidding.

We did it with a very large amount of misgivings and I remember wondering how in the world we were going to spend $600,000 a year. And I wondered about that for about two years, about the third year we had to go back and ask for more money. These laboratories turned out to be god sent for us. See this example, an experiment that we didn't do because we were bright but because somebody talked us into. But these laboratories allowed us to have more equipment and better research for our faculty, and to support graduate students who needed support, do things we never dreamt of doing before the war. And so that not only were they important to national security but they turned out to be extremely important to the schools that had them. And slowly the Defense Department and the Atomic Energy Commission began to support similar laboratories in a number of other schools. These, mind you, were there not to help the schools, but because the basic research they were doing was regarded as vital to the national defense.

But there were many people in the country who were concerned about having all of the basic research in the nation supported by the military. They felt that this would cause distortions in our research programs. Things that didn't appear to be important to the military would not be supported. And this was true. I remember in the Research Laboratory of Electronics which I helped run at that time, I supported quite of bit of medical and biological research. And I used to go around to the Air Force and say "Is it alright for me to do this? Do you regard this as electronic research?" And they would say, "If you say so then it is." And this was the only way that basic research in the life science was supported around the nation for the first several years after the war, because there was no one with a major mission to support it.

Well the first thing that was done to alleviate this was the National Science Foundation was created along about 1950 and given the responsibility for stimulating research and education in the basic sciences in the universities in our country. About the same time the National Institutes

of Health were created to do a similar thing for the health related fields. These two institutions had no applied mission, unlike the Defense Department, their only problems were to see that good research and good education was done in the basic sciences and in medicine and they've played a very important role in the development of the university system. Well slowly they filtered out money to schools that were not involved in Defense supported activities, and began to broaden the base of education and of research in these schools.

But the fact of the matter is we can have all the best intentions in the world and we still can't get much support for an ideal. And even today the amount of basic research supported by the National Science Foundation, for example, amounts to about 150 million dollars, as compared to the billion or so that is supported by the mission directed agencies. So those big institutions that have got their start working for the, on the defense oriented problems, have had much better support than the universities, schools that have worked truly as investigatory institutions. And at the same time of course the big, the schools with the good support have gotten stronger and this has generated some problems.

Now in the past half dozen years Defense interest has sort of tapered off. We've learned that the Russian's military force wasn't as strong as we thought it was. We've learned that our force is probably more protected and safer than we used to think. And we no longer, and the Soviet Union has certainly changed a good deal too, and we no longer feel that its primary motivation, if it ever was, is to get to the position where it can destroy the United States. So that our government doesn't regard national security and national security spending to be quite as important a thing as it did, certainly not the all pervasive and all dominating thing that it was 10 years ago.

So that the motivations for spending money in science are not as much defense as they used to be but people somehow still feel that we need to have research and development for the welfare of the country and we spend it without quite knowing what the motivations are. We know intuitively that is good. And once you get into that situation where you can no longer say this is what we want to do because we understand the motive, but rather we’re supporting research because it’s good, like Wheaties, then it’s a little hard to say you’re going to have this group do it because they’re more able to do that particular kind of research. And you get into a very complicated problem: how you allocate your resources. And we’ve gotten into that kind of a situation today. So when you hear the all the big debates about where, who’s getting the money

and where should we spend it and so on, it’s because our purpose in spending money for research and development has shifted from one of purely military emergency to one of a general recognition; that research and development has made our economy increasingly strong.

We want it to go on growing, therefore we want to continue spending for this purpose without being quite able to understand it. In fact, one of the things that used to worry me when I was working in the White House was how much money was it healthy to spend on research and development. Was a billion and a half for pure research and sixteen billion all together just right or should we be spending half that much, or four times that much? And I used to go around and tease the economists who worked on the White House staff, knowing full well they didn't know the answer.

I'd say, "How much money should I spend? What's the right amount?" and they never, one of them gave me an answer that the professors in the audience I think will appreciate. They said, "You should go on making increases in your research investments until the incremental increases just equal the return on the new increases." That is if you spend money on science you make the country, on technology or on science, you make the country more productive each year. Each year, for example, it's just been announced, that the productivity will end up 3.8% this year. In other words each workman turns out 3.8% more goods than he did last year, and this is a result of better tools and better understanding of computers and so on. And this is worth a lot of money, it is worth about 40 billion dollars. So in principle one should be able to spend about 40 billion dollars on their theory.

The trouble is that there are an awful lot of things that you invest money in that you can't evaluate. Who can tell me about the worth of penicillin, for example? It cost a few million dollars to develop, but what is the return we get each year on it? Or on many of the other drugs, or on the time we save on airplanes and so on? So that one really can't come to grips with what is right and what is wrong when you ask how much should we spend and therefore you're going to hear a continuing debate in my opinion about this.

And then there is one last problem. I understand Archibald MacLeish was here two week ago and he and I have a continuing debate which is really very lopsided and unfair because he because he phrases his half of it in poetry. [audience laughter] Which is much more appealing to people than anything I can write. About what kind of world are we making anyway. And this is

probably the most serious problem that confronts us, that confronts you and me and everybody. Because we seem to have unloosed some kind of a hurricane that we can't direct. We're all glad to have the new drugs and we're all glad, I guess, to have television and transistorized radios and many of the other things.

But we don't like the smogs and we don't like the crowded highways and we don't like the ugliness of the shopping centers, but somehow we have learned how to turn on this peppermill, but we haven't learned how to control it. And this I think is probably the most serious, I criticize, I began by criticizing Jacques Barzun for criticizing science for just this aspect of the problem. And I, I'm not sure that I have a solution, but since I criticized him for not having one I will offer one.

I think first of all we need to recognize that we can't allow just the totally uncontrolled, un-propagated expansion and exploitation of all the new ideas that come along. But have to learn how to try them in moderation first and then see whether their affects are constructive or destructive before we go on to the next step. Now this is an easy thing to say but I don't know how to do it. In fact I remind myself of a time when I was in Moscow at a negotiation. I was there in November of 1960, just before, right after the election and just before the inauguration. But it was already known that I was going to be on the White House staff, even in Moscow. And there was a big banquet for the American delegation and there were a lot of toasts being given.

I had been rather annoyed during most of the conference because the Eastern Block representatives kept wanting to talk in generalities and we could never get them to come to specifics. And when it came my turn to drink a toast I thought I would tease them a little bit. I said I'd rather tell a story than drink a toast and that this conference reminded me of the story of the improvident grasshopper that had spent his time in the summer having a very good time. It came fall he suddenly realized he wasn't going to get through the winter because he hadn't stored up any food. And he went up to the wise old owl and said to him "What do I do?" and the wise old owl said, "It's easy. You turn yourself into a cockroach and go live in that big white house up here." And the grasshopper was happy and he hopped away. But suddenly he ground to a halt and he came back and he said, "But how do I do that?" And the owl said, "I don't know, I just make policy around here." And I said that's what this conference reminds me of. And they roared and I just didn't understand why a Russian would laugh so hard.

Jerome Wiesner: And I said what do you mean and they said "We Knew you were going to work in the White House with Mr. Kennedy." [Clapping]

Jagdish Mehra: Dr. Wiesner is going to field some questions. But before you submit him to your queries, I would like to invite all those who wish to come, everybody is invited to visit at SMTI we are holding a reception in honor of Dr. Wiesner. It is a very beautiful occasion in more ways than one because I think symbolically it's tonight that MIT and SMTI meet officially. Our next event in the Humanities series would be the American Folk Ballet on November the 16th. Our next speaker on December 2nd, a great teacher of Physics, also from the Massachusetts Institute of Technology is Philip Morris. He will speak in this auditorium December 2nd, at 8 pm on inter-planetary travel. Now questions for Dr. Wiesner. Yes please? [Inaudible question]

Jerome Wiesner: The question is that the technologists are creating problems; I'll paraphrase it, faster than the social scientists can solve them. I don't know of any problem that a social scientist has ever solved [laughter]. And I don't think you can expect them to solve them, maybe they'll understand a few. I think we, just as I think it's wrong to say that technologists have created these problems. Technologists invented an automobile, but it took all of you to stink up the atmosphere.

In fact, you know this is really a question of where the, how to come to grips with a problem. To say that technology is generating problems is I think a great oversimplification. I think this society by now has learned to employ technology without learning to do something about its consequences. And the reason is that for the most part, with the exception of a few little problems like the atom bomb which was done by the government, the technological problems at least in this country are generated by sort of, private initiative and free will in a way. The automobile was invented by somebody who thought this was better way to get around and we all voted yes by buying them. But this was never a decision by a bunch of technologists that we're going to peddle a lot of automobiles and cut up the countryside, pollute the atmosphere and congest the cities so thoroughly that you can't move around. This is just what's happened by all of us electing to buy an automobile.

The same, think of the, we wonder what's happening to the younger generation. I've come to that point you see. I don't understand why my fourteen year old doesn't want to cut his hair for example. But we, somebody invented television and then it got taken out of the inventor's hands

by the great commercial exploiters of it. It wasn't the inventor that made bad use of television. If the television had been put to a useful purpose it might have been just as powerful a tool for good as for bad in our society. I think that one of the problems that we face today in our society is that kids are maturing much more rapidly because they're all exposed to much more information on the radio, on television, in newspapers, in magazines they never had before, but we still treat them the same way. But all of this was done without anybody ever voting to do it. Somebody had a great technical idea.

Now the social scientists can understand some of these problems but they don't know how to do anything about it because we haven't made the social invention. Now maybe that's what you mean by the social scientists. But I think it's as much a political problem as it is a social science problem. I don't know how to answer your question and I think this is the great problem of our next twenty years. I think technologically we can do almost anything that anybody can dream of doing, if it makes any sense and a lot of things that don't make any sense. We have the tools to feed the world, to house the world, to cure most of the rampant illnesses. But we aren't very likely to do it because we don't know how to organize ourselves well enough to do it. And even if we did we don't know how to convince people. The politics, as somebody has said, is the art of the possible. And I've known three presidents quite intimately in the last fifteen years as advisors to them. And each of them had a much better understanding of the problems of our country and of the world than they were ever able to apply because of the constraints that all of the political pressures placed upon them.

I remember occasionally being very annoyed with President Kennedy, more annoyed with him than any of the others I worked for because I was closer to him and in a sense, I loved him, and you know and when you're close to a person you can be more annoyed than when you don't think quite as highly of them. Then I'd say well he got elected, I didn't and he knows the boundaries of political reality are today. And I know very well that he often made compromises that he resented having made. And this is, I'm sure that the mayor of this community does that and the governor of this state and this [inaudible] means that somehow we need, all of us, to be better informed and more thoughtful so that the things that we're willing to let our leaders do come closer to being the right things. If we're going to run a democracy. And the only other solution is to put somebody in charge or somebody takes charge and then that turns out in practice to be much worse.

Because democracy is a pretty rough business and it almost works and I described the process of how you learn in a democracy and it's pretty painful. Both for the spectators, inhabitants and the people trying to deal with it. And the temptation to say the hell with this I know the better way, you know, I know what's right and become a dictator is very great. But the fact of the matter is a dictator is just a human being too and he doesn't understand the problems and he isn't prepared to accept this slow learning process. So he goes off and has a new agricultural program that sets the Soviet Union back three years or they have a great leap forward as in China. And they usually go backward because the society is so complicated that simple ideas don't usually last for very long. But it's in your hands really; it's in the hands of the citizens to know more about what it is you want and what you don't want. And to be willing to pay for it. Pay for education, pay for all the other things that we need. Be willing to have more of that and have less of horsepower for example.

Jagdish Mehra: May I perhaps ask a question about the point you raised about all the money coming to the great institutions, to MIT and Berkeley and Caltech. I think that it will be a very great day for SMTI when the legislature comes on its knees to us offering lots of money. But you seem to suggest that all the money coming to the research establishments is perhaps beneficial. That is an unmixed blessing. Is it true? I would have thought that it would create quite a few problems, not the least of them would be increasing bureaucracy and also a pressure to do things which need not be done.

Jerome Wiesner: Well the, let's see if I can do a short answer this time. The, this is a complicated question, one I know a lot about. I think that on the whole this is a very good thing. There has been this great debate in the Congress recently. I read you the title of a report which had just come out on whether or not federally sponsored research has harmed higher education. The committee was chaired by a congressman named [Royce] [There is no record of a Congressman Royce from any state during that time period, but there was a U.S. Representative John Abner Race from Wisconsin who served from 1965-1967] who I think is from Wisconsin who has been a long time enemy of federal support of research for some reason which I don't know. And he invents all kinds of ways of getting at it. His most recent attack was to maintain that too much research has hurt education in the country. And he had a hearing in which he sent out questionnaires to about 500 schools. He got answers back to them, he apparently didn't pay much attention to the answers and wrote a report which said federal support of science is hurting education. And he had several points. One, the professors were

turning from teaching to research. Two, the professors were being weaned away from their academic institutions, not listening to their deans, and their presidents and their provosts, but listening to the government because that's where the money came from. And a whole variety of other charges.

Yet if one takes the trouble to read that report, almost all of the institutions and the students and the faculty, the interesting thing was he sent out questionnaires to a fair number of students in the schools and the faculty members, and you can find people who complained to be sure, but the overwhelming majority of the people think that balance, this has been good. And I think so too. In our own school for example, I don't believe the educational program has ever been as strong as it is now. For the fact is that people who are alive and interested in their research are better teachers and this comes through time and time again from the students who say, "Well as far as we can tell professors who are interested in research are more interesting and we like them better."

And this has certainly been true at MIT. There has been a change but it is a subtle one which [Royce] doesn't seem to be able to get through his head and that is most graduate and most faculty research is teaching. Before the war very few schools that taught science had any graduate educational programs at all. They were mostly graduating B.S. students. A few of the big schools like MIT and Harvard, maybe twenty or so had doctoral programs. But even there, in these schools, the number of doctorates that were granted in any one year was quite small.

Well science has progressed so much in the past twenty or thirty years that no one who has only a B.S. is really adequately educated to do research or engineering. And so graduate education has become as important as undergraduate education. At MIT we have 3750 undergraduates and 3780 graduate students. Well the most important part of graduate training is teaching a man how to do engineering or do research. You know you'll ask any teacher what he's trying to do and he'll say "What I'm trying to do is teach the kids to think." And you say "Well how do you do that?" And you'll get the most blank stare you've ever seen because nobody knows how to teach somebody to think except by letting him think. And you learn it the hard way. Maybe if you are good and peppy yourself, you help him learn. And we teach research by letting students do research under our guidance. And so the research activities in a university are essentially graduate teaching. Now it's possible that there has been a shift, in some places too much of shift, from undergraduate to graduate teaching. But I think that this is

a transient that will take care of itself. I think that MIT and places like Caltech and Berkeley people would say that the undergraduate teaching too is much stronger as a result of strong graduate programs. I certainly believe that to be the case.

Jagdish Mehra: Anybody else please? Questions? Yes? [Inaudible speaking]

Jerome Wiesner: I don't know what is too much and what, the question is, is technology becoming more and more imbalanced internationally? I don't, I can't make a judgment. I can tell you what the situation is and I can tell you that there are countries that think it's becoming more and more imbalanced. Before the war, the strongest technological nations were in Europe. Certainly Germany was the strongest scientific nation and probably had the most advanced technology built on science. Since the war there is no question at all the United States excels in both basic research and in applied research: in engineering, technology.

You have to talk about two groups of countries when you talk about this problem. You have to talk about the underdeveloped, emerging countries where there is essentially no science and so any progress anywhere means that the gap is getting bigger because theirs is not building up very rapidly. Then talk about the developed countries, and you get a quite different thing. In the case of the developing nations there are attempts being made to get some science and some engineering going, but I don't think it's being done with enough energy. And if you measure the result, namely what's happening to the economy certainly the developing countries are slipping back relatively to the developed countries.

Now in the developed countries, Europeans are certainly very worried about the fact that we so completely dominate most fields of technology that they have a hard time competing. And it's partially because our economy is so large and powerful, our country, one big country with an enormous economy so almost anything you can do has a good market. Whereas the Europeans have still not learned to have an integrated market. Even though they keep talking about it. So you find that the biggest computer sales in Europe is IBM as an example, and the best oil refineries turn out to be American oil refineries. And the best airplane designs tend to be American. So there is an increasing sensitivity about this problem in Europe. Whether or not they are any worse off than they were ten years ago I don't know, relatively. There is a growing recognition of the problem. The French are very upset about it, the British are very upset about

it. The Germans are currently not so much. Shall I have another?

Jagdish Mehra: Yes, please. [Inaudible speaking]

Jerome Wiesner: A friend of mine once said I was a scientific vagabond. I don't know how to answer that question, because it, again, has so many dimensions. I think that it's very clear that we're becoming increasingly a country where there isn't much of a role for a person without a good education. Whether you are going into the sciences, or you are going into the law or any other field. So that we want to find a way to provide increasingly adequate education for everyone.

But having said that which is almost a platitude, you ask what should we do? Well I think that we can afford first of all to spend a lot more time and energy trying to learn about how to teach and how children learn and what motivates them and so on, so that many more of them will both enjoy and get something out of the educational process. I think we can spend a lot of time and we have been doing a good deal of this at MIT, trying to develop better and more effective courses. And we've been doing this not only in the sciences, but in the social sciences and other fields.

I think that we need to have more respect for education in this country. I think one of the basic problems in this society is that we give lip service to education, but not support. Teachers are better paid today than they were a decade ago and beer truck drivers, I think, get a better average salary than teachers in the country. So that I think we have to, all of us, get more of a commitment, be prepared to support education more effectively so that more and more of the good young people are challenged to go into education. Now there has been a trend in that direction these past half dozen years. I think the most important thing that has happened in the country has been a sort of invigoration of the field of teaching partially as a result of some of the developmental, experimental things that are going on which made teachers a little more excited about teaching. I think some of the challenges like the poverty program which have made people wonder how you deal with these very complicated problems have made the teaching profession look more interesting. But I think it is just a start and we have a long way to go.

And I think another aspect of this, just go on for a moment, relates to the first question that was

asked. I think we don't understand enough about social problems, individual interactions and social problems, how children develop, how their attitudes develop, so they become part of society. I think we need a lot more understanding in these fields before we can have a vastly improved educational process.

[Inaudible question from audience].

Jerome Wiesner: Well that is a problem but it isn't a question of getting their share. You're voicing the sort of popular image of this problem. I don't think that people normally place themselves on these reviewing panels. They're selected by government people responsible for science. But the mechanism may amount to doing that. If you look around and say, "Where can I find the ten people in the country that know the most about these fields?", I think the way that research activities have developed in universities, I think the ten people are most likely to be in big universities. That's the first thing.

Secondly if your basis for giving a grant is the excellence of the activity or the proposal, a man in one of the top ten institutions is more likely to be a somewhat better and more experience scientist than a man in a smaller institution. If there was a better man in a smaller institution, the big institution would go out and hire him. So there is a selective process of that sort. And finally, a man in a big institution has all the assistance of interaction with a lot colleagues which makes his activities and proposals a little better. So all of these things are at work and one has to try to find ways of conquering it. We, when I was working in the government, developed something we called the Science Development Program, in which we had money, got money from the Congress to provide grant support for institutions that were emerging, but were not quite strong enough to compete, if there was limited amounts of money, with the strongest institutions. And there has been quite a bit of this, if you.

There has been also pressure and I think the president has succumbed to some of it and I think that it's a very dangerous pressure: to say that you ought to just have geographic distribution of the funds for science. If you've got a billion dollars, you spread it out according to population. You pay no attention to the quality of the grants. I think there is no quicker way to push us from first to seventh in science internationally to go all the way in that kind of procedure.

Jagdish Mehra: Can we hear one more question? Yes, please? [Inaudible question from

audience]

Jerome Wiesner: Well I, I promised myself to quit commenting on the space race, so I won't do that. I get into trouble every time I do. And I, but I, I think the American reaction to Sputnik 1 was quite mad, but probably a good thing from the point of view of forcing us to be interested in education for example and a variety of other things. It was a good shock. But we certainly overreacted by a factor of a hundred. We suddenly, in fact when you think about the tremendous political mileage that the Soviet Union got out of Sputnik, I think you have to realize they got it mostly because of our own reaction. We suddenly got scared and we said, "Gee this means the Russians are better than we on everything." While the rest of the world said, "Gosh, if those fellas are so scared it must be pretty serious."

Just why we reacted that violently, I don't know. The shock of, I suppose it has to do with a number of misconceptions. Why do you laugh very hard when somebody tells you a funny joke? Or what makes you react violently? It's when what happens is so different than what you'd expect. Now I don't suppose we laughed hard about Sputnik, but we had a tremendous emotional reaction because all of our conceptions about the Soviet Union were sort of destroyed here. We didn't think, we thought the Russians had five thumbs on each hand and that they couldn't manipulate anything. We knew better. We knew they were very good mathematicians and pretty good physicists, but we somehow thought they couldn't do anything. So when they did something and did it before we did, you know something we thought we were going to be able to do two years after they did, we just had a shock. Went into a state of shock that took us a while to recover from.

Jagdish Mehra: May I thank Jerome Wiesner for visiting us tonight. And I ask you now to go to your school for a reception at SMTI. [Clapping]