A of the work that would have to be done, the approximate costs, let's say in about four months so that the recommendations of the ten or twelve different task forces could then be put together at the next level of integration to see whether things could be put together in an order of priorities that could then be submitted let's say to the cabinet. And the idea also was to have representatives among these task forces from the ministry of commerce and industry, from the various groups of the government dealing with economic development drawing on the information that was already available at the national council for research and development as part of the prime minister's office.

And in preparation for this because I knew that the conference of thirty people doesn't get anywhere, so it was necessary to have something to shoot at. And I worked very closely with the then scientific director, Professor Gerhardt Schmidt, to prepare a preliminary outline of this and we did. We had a meeting and it was a question of getting volunteers for different activities and even before I assumed my presidency on the first of January, 1970, we already had an activity that was primed to explore the possibilities to see the way in which the Weizmann Institute, working together with the other institutions of higher learning in Israel, working together with other groups like, as I said, the council for research and development and various ministries that at least explore the possibilities of harnessing the brain power of Israel to deal with the challenges for accelerated economic development.
To me it was very stimulating because if you gathered from my previous conversations I was always a person who was acutely sensitive to the need for seeing that knowledge that becomes available through research not remain something beautiful on the shelves of libraries or like the works of art hanging in museums, but be used as far as possible to solve basic and important human problems. That is another reason why I chose to go to Israel, as a prototype. It was an activity that interested me very much. And then let me have a little digression: here for a moment.

Because I mentioned Professor Gerhardt Schmidt who was scientific director. He was an organic chemist who was working on very important aspects of various problems in chemistry. And he knew there was problems in Israel very well. He was an unusual man. Obviously Gerhardt Schmidt is not a Hebrew name. Gerhardt Schmidt was the son of a protestant German professor in Munich by the name of Schmidt, but his mother was Jewish. And when the Nazis came the father practically deserted the mother. And Gerhardt Schmidt was still quite a young boy, fifteen or sixteen at the time, and his mother and he escaped to England. And the father remained. Gerhardt Schmidt then was sent off to Australia as an enemy alien, he and his mother. At any rate, the point was that finally he came back and he got his degree at Oxford University, outstanding chemist. And I don't remember exactly what year whether it was in 1948 or whatever, he came to Israel and joined the Weizmann Institute. And he was as dedicated and as brave as you could find. But he didn't give up his father's
name and he continued somehow because I think his father is still living. To me it was also meaningful in a sense because my daughters who also were products of a mixed marriage, they were Jewish, English, Scotch, Irish, German, Americans and it is often said that these mixed marriages lead to disintegration of the Jewish people but it is a two way street. And Gerhardt Schmidt to me was an example of how it worked the other way. And it was another example of the call of heritage. Gerhardt Schmidt could have had a remarkable career in England at Oxford University where he graduated with honors. And also as a specialist in part of x-ray, crystalline. But he chose Israel.

I liked to work with Gerhardt. He has a very strong personality but perhaps a little too strong and autocratic. He made decisions on behalf of everybody based on--without too much consultation. As a matter of fact, I had a very painful experience because I had very high regard for Gerhardt Schmidt, and, in the meeting of the board of governors, November '69, when I was already president elect and I was there. He talked to me about the need for a new chemistry building in which various activities in the broad field of chemistry could be brought together under one roof and thereby utilize equipment and other things for expansion in new directions. And I trusted his judgement. I said whom would you bring in? Well, I would say here I would bring in these people from, who are now in the physics building and these people who are there together and--how much would it cost? He said well it would cost a million dollars. And a man whom I had met in October in Chicago who
said he wanted to help. I was then already president elect. Well I got him, at that point and without going into detail because this is part of my life as president of the Weizmann Institute. I wanted to help raise the money that was needed. I got him committed to a point at that November meeting where he said that I expect to sell a big apartment complex that I have in Chicago. He was a Harvard trained lawyer incidentally. I expect to sell it for about eighteen, nineteen million dollars. And if I do, you've got your million dollars. Well, in December he did and he said you have it. And that was the beginning of the greatest headache because I discovered that Gerhardt Schmidt made a decision without ever asking the people that he intended to bring into that building whether they wanted to go without ever determining how much it would really cost, because that million dollars turned out to be a figure just picked out of the air without any sort of systematic work which I would have expected this man, with the German background who was otherwise so excellently disciplined, but this was the Weizmann Institute was being run. It was figures out of the air without any good preliminary solid planning. Well, it was a source of constant headache. And I decided it was necessary to restructure the academic organization and decision making or the Weizmann Institute. And I would say at this time perhaps out of context that I don't know whether or not I really contributed very much during my three years as president with time out for illness. But I think that there was one thing I really left behind quite aside from a number of new structures for which I was able to get funds and donors. Was a new academic structure in decision
making process which has remained and which has replaced the utter bedlam of the decision making. And that is this. I went around for the first months talking to the different faculty members because the assumption that when you serve as a member of the board of governors and even a scientific member of the board of governors and you know some of the problems, that you really know what is at the root of the many problems in a big institution whether it be university or a research organization, you don't, you really don't. And so, I spent a lot of time talking individually to the heads of departments and so on because there were I think nineteen different departments and the graduate school, each one different. And there was just only one scientific director. Sometimes he talked to people and asked for advice, and sometimes he didn't. And the assumption that I would get an immediate enthusiastic response to a plan or reorganization was certainly erroneous. There was nothing so steady-state as what there is. And any attempt to try to change something with which people have lived always meets opposition.

And I was myself conscious of one thing always when I felt there was a need for change. To be certain that in bringing about the change I wouldn't hurt anything, you see. It is important not to do any damage. First of all. At any rate, what I came up after a great deal of travail and not without opposition although I finally submitted it to a secret poll to the faculty and did it only on the basis of consensus in the faculty because I had to do all that to get ready before the next meeting of the board of governors in April. And I did it with a whole lot of other diversions. I had to work in the institute, and I tell
you I worked long, long hours. I had to work in the institute. I also had to get back to Cincinnati. I came back to Cincinnati and I decided that I couldn't give 10% of my time. I had to close down the laboratory completely in Cincinnati and stop my activities. I also realized then that I had to get a separation from my wife. There was no way in which to patch things up. All this, and then also to go down and meet with the decision makers of the board of governors in the United States to get ready for that next meeting of the board of governors in which I wanted to introduce a plan of reorganization not only of the academic structure of the Weizmann Institute but also in part of the way the board of governors worked to support it. Well I came up with the following.

I decided that it was necessary to subdivide responsibilities into faculties. And so a faculty of mathematics was set up. A faculty of physics, a faculty of chemistry and a faculty actually it was a double faculty in the life sciences. One was more biological and the other more biochemical and biophysical. And the graduate school. And each one would have a dean. And that dean would have the responsibility for policy decisions and priority setting within its structure because what was in mathematics. It wasn't just a department of mathematics, making a faculty out of that. No. In the department of mathematics for example you had one of the most advanced computer science engineering complexes. I have to say a little bit more about that, and you had more of applied mathematics. And you had geophysics and you had so called pure mathematics you see. These were all various mathematics activities and the man who really
was more or less involved in all of them was again a man, an American who was trained in this country, at the Massachusetts Institute of Technology. He worked with the navy in World War II. He was at the institute for advanced studies. He built the first computer in Israel before there were any computers available but he was going on far too long after computer industry had already made long advances spending tremendous amounts of money to develop still new computers at a tremendous cost. You see, but nevertheless, he provided computers for the Weizmann Institute of Science when there weren't any computers, to be bought. He built computers there when Israel had computers when you couldn't buy any computers. And he was an outstanding man also in geophysics and in other places. And the man who subsequently came was another man who was a foremost mathematician in pure mathematics, professor at Stanford. And everybody in the United States wanted him, and again the call of heritage made him come to Israel and he spent part of the time at Stanford and part of the time at the Weizmann Institute and there was an exchange of students. So you see there were these groups and the same way in physics. There was high energy physics and low energy physics and applied physics I mean solid state physics. That was a faculty. Each one with a dean and then there was experimental biology and chemical immunology and genetics and plant genetics and so, I tried to group these into faculties, each with a dean and to have them deal with the basic responsibilities of their group so that you wouldn't have nineteen different departments each one struggling for a piece of the budget and a place in the sun. There had to be some system. And I also decided that it would be absolutely
necessary to have a vice president who would be a scientist who would not continue to give part of his time just a little, but who really he had to almost give the major part of his time and one who knew the place, one who grew up in the place. I knew that I was a johnny-come-lately. And so I decided to have a sort of cabinet, to work that way. That a council of these, each one each dean of each faculty had--and to select the deans also I didn't just appoint them. I had secret ballots, you see. I mean I was working like the devil around the clock really. But finally we came up, it was accepted. The board accepted it, and I had this council of deans made up of the dean of each faculty, the dean of the graduate school and the vice president. And the function of this council of deans was to deal together with all problems of decisions, major decisions that had to be made for the Weizmann Institute. That if, as it came out later, low energy nuclear physics wanted, they thought they needed four, five million dollars for a new accelerator in order to make any progress, and somebody said why should they get four or five million when we can't get 200,000 for something we want. I wanted to have all those things discussed first of all within the council of deans. And for the first time in the history of the Weizmann Institute people were able to see and discuss the problems of the whole institute and not merely discuss their own little problem with whatever person happened to be scientific director at the time. Well this was an institution that remained. But I realized that it wasn't enough. That in a council you always have divergent views. And ultimately somebody, the buck stops somewhere, and somebody has to make a decision. And how was that decision to
be arrived at? Because I was not naive enough to think that
sweet reasonableness would obtain in this council of deans and
this cabinet that they would invariably arrive at a solution
that was the best solution or that I, if I then decided to make
a decision that that would be accepted. So it was necessary to
have still another mechanism. Do we still have more--?

Q Oh, we have plenty of tape.

A Alright. So therefore I suggested that the scientific
members of the board of governors, under some of the outstanding
people in Britain, the United States, some from France, Germany.
It was really the scientific members of the board of governors of
the Weizmann Institute was a who's who of the leading scientists
of the world. And so I said we can't just have them come to an
occasional meeting and expect to use them properly. I said that
we've got to have a scientific advisory council from among the
scientific members of the board of governors, organized in such
a way that problems that would come up for evaluation and presented
by certain faculty could then be presented by the president to the
and to have a chairman to this scientific advisory council to
get an independent evaluation of the matters pro and con, just
on the basis of the proposal itself. Was it too far out. Was
it merely a duplication of effort that was already being done
much better somewhere else. Was this a reasonable proposal
and the point is that with this extreme specialization of
science it wasn't possible for even any one group to make
evaluations. Well, the board of governors accepted that, and
the man who originally approached me and asked would I be
willing to be considered, namely Chris Anthenson was, I recommended
that he be made chairman of the council of scientific advisory board. Our relations also subsequently did not turn out too well despite the best—there were other factors that came in later. The point was that it operated very well because even though with the excellent scientific advisory board that we had you couldn't always have somebody who knew everything but the chairman of the scientific advisory board if it was in physics, we would try to pick physicists we had on the board. If those physicists felt it was a little outside of their field, they would get somebody else. And so I would submit these problems to them for evaluation so that ultimately we didn't have to depend on what you might call the self interest bias of proposal of the dean or the departments in physics or whatever. We got independent judgements and even then, value judgements had to be made. One of the most important ones which has just come to fruition, the decision of a tremendous expenditure on a new accelerator to make nuclear physics studies possible. And you had to decide for example can nuclear physics continue to make progress without such a new accelerator. And, if this is absolutely necessary must it be done in Israel. Must we have a nuclear physics low energy, a low energy nuclear physics activity. And you see there were all sorts of value questions. But they were worked out in this council of deans and ultimately they came down in recommendations to the whole board of governors. And here is another rub on freely associating.

Sure there was a big board of governors, prominent businessmen and rich men in Britain and the United States and so on. But actually there were only two or three people who were running
who were making the decisions. The man who was chairman of the board of governors who was born in New England, in Boston, he was a wonderful man his name was Dewey Smith. Dewey Stone. And, as long as he was chairman of the board he somehow or other kept things together. He had a certain balance. But there were a great many difficulties. Ultimately he developed Parkinsons and he had to go off and there was a new chairman of the board. There were all sorts of problems.

Q We have plenty of time. Go ahead.

A We can't finish this in a half an hour because when I consider all the problems. So let me get back and get some of the--

Q We can go--

A No. Just let me--

Q Go ahead. Keep going.

A I want to just skim off some of the things off the top of my head. In addition to all of these activities we were always running a deficit. There was a question. There was no money. And I had to go to Washington because we--it was an American school abroad and it was entitled by law to certain help. It was like the American University in Behrut. There were a number of very good representatives and I had to develop special relationships through these legal advisors in Washington with people in the Congress and the Senate. There many of them were not new to me. I dealt before when I had to testify in behalf of the National Institutes of Health so there were all sorts of activities that just occupied all of my time. But, there were also certain frustrations, many frustrations. For example,
when I set up with volunteers these twelve task forces. Now I am going to find a little more of what they consisted of. And I said please, I would like to have a report by April. And then I had to forget about it. And leave the people who volunteered to take on this effort that they would do that job because I had to be involved in academic reorganization. I had to be involved with the board of governors. I had to be involved with funds, with problems, with all sorts of problems that a president of such an institution is involved in. And in addition to everything else the Weizmann Institute was more or less a part of the ministry of foreign affairs because every V.I.P. who came to Israel it seemed every V.I.P. was brought to the Weizmann. Because it was really a jewel to show off. And to be quite frank, I spent a good bit of time. I enjoy that part too. I enjoyed speaking to ministers and important people not only from the United States but from many other countries and I tried to convey to them was my enthusiasm of what Israel was all about and what the Weizmann Institute as a part of Israel was all about and that took up a lot of my time also.

Well let me say that when April came around I found that these task forces I set up didn't get to first base. And one of the lessons that was a tremendous frustration. One of the lessons I learned as a result of that administrative year is that if you take a busy scientist and if he volunteers with the best of his motives and he just gives it a part of his time on a very important job that you just won't get anywhere. That it is necessary in my judgement in order to do a thing like that, to get people to take a leave of absence from their work and devote
themselves 100% of their time to a specific task and to come up with a report at a certain time. Quite aside from the fact that they were pursuing their regular work as department heads, and doing their research they encountered a great deal of resistance. This is another part of the academic life. Of getting cooperation when they tried to get people from the other institutions of higher learning, there were the old jealousies and extraordinary competitiveness that occurs in all academic institutions. The job of utilizing the brain power of the world or even of a small segment of the world to deal with human problems is very easy to say and I will mention later another experience I had as president of the Weizmann Institute working with the Nobel Foundation and the Rockefeller Foundation at a special operation on this.

It was difficult. To be quite frank, ultimately we didn't get anywhere. I couldn't give it the personal direction and drive of really sticking to it. Although I met with representatives of the manufacturers associations, with the ministries. I tried to be a catalyst but it wasn't enough. I had too many other things to do. And now, even now years later as I continue to be a member of the board of governors in many more institutions of higher learning, that problem still hasn't been solved. And it hasn't been solved in this country. And it hasn't been solved in the world of how really to organize the brain power, the scientific and technological brain power to deal with outstanding world problems which need new knowledge not merely the application of existing knowledge which is a political process. At any rate this was one of the frustrations. I failed in it terribly. But
perhaps I learned some things that I thought could be done and now I know cannot be done so easily. But nevertheless, it will have to be done somehow.

The other problem was a constant problem with the faculty itself. And that is that I became to realize that while there were a great many top flight people on the faculty of the Weizmann Institute that they were prima donas as are people in universities generally. That the attempt to join together in a collaborative effort, members of different faculties, to plan their work in such a way, still as individuals, but plan it in such a way that the sum total of their effort would make a much larger impact on a specific segment of a problem that their own individual efforts would--that turned out to be a difficult problem. Let me give you an example.

At that time I was very much involved in the planning for a special cancer research effort that was going on in the United States. As a matter of fact, in 1971 I was among a small group altogether when this was being considered before the Congress and the president to consider ways of which such an effort when if it became implemented could best be carried out in the United States. I was also one of the people who was being considered to head up this cancer research effort. It was in the newspapers, the gossip columns that I was among the top people being considered for the job. That what I tried to do was this. There were a number of different departments in the life sciences at the Weizmann Institute who each in one way or another were doing research on aspects of cancer, which had a
bearing on fundamental problems with cancer. And what I tried to develop is a program in which a consortium of people in which they would at least plan their work together and identify certain specific problems and see what is holding it up. See what kind of research would be done. See how they could do it together and sort of make a very special impact. So you wouldn't have ten different little pebbles being formed by different groups at the Weizmann Institute, but you could say here is a consortium of cancer research scientists working together at the Weizmann Institute and this is the important problem that they have chosen to address themselves to and this is the information that they have come up with. I found the biggest resistance. Nevertheless I spent a lot of time to get up an application to have the Weizmann Institute as a cancer research center that would work with the National Cancer Institute. And be supported. And we went as a group I mean, up to a point, you see, I was pushing this hard. And we went to the N.I.H. and people from the N.I.H. came out and some things actually got started but it was full of frustrations. And the frustrations came not from problems dealing with the N.I.H. The frustrations came with the scientists at the Weizmann Institute. It was a kind of prima donna attitude of not taking the trouble of defining the most urgent part of the problem but skirting the periphery, skirting that which was most difficult, skirting that which required an organization for a bigger effort in order to reach an end point of decision, and there too I failed. I failed because some of the things that they even promised that they would do when they got support for this work from N.I.H., they didn't do. And this
is one of the great problems in science planning and the pursuit of the scientific effort that somehow or other scientists instead of deciding, even in their own field, in their own department, out of ten spheres of interest that I have which is the most important and which should I concentrate the most. That is not being done. One of the great problems at the Weizmann Institute and in other places is that each one is working, trying to work on ten different problems at the same time. And as I looked at the annual scientific reports, they were just sort of picking away at it, picking away at it instead of having a concentration of effort. I also realized that there was going to be more of a sort of problem-oriented effort joined onto the complete exploratory type of research which should go on, that you would probably need different people and you would need a different kind of organization.

I also was finding in the midst of this work there were gratifications. There were also many gratifications that I shouldn't overlook. But I am thinking now of the fact that with all of this effort probably subconsciously I had my first attack of coronary schemia in 1971. And it came during the course of a meeting of an executive council in which I really felt very proud of myself because I thought I really had the collaboration of the group and it came with--I had a letter from the director of the then national cancer institute because the bill hadn't yet been--I mean the reorganization of the separate cancer effort had not yet been made. And that the National, N.I.H. was going to support a specific cancer program which we put together
in a very nice, systematic way of projection and I was sitting quite happy when suddenly I had a pain over my chest, a little different from any that I had had before. It was not getting better and I went to my house on the campus and well I was taken into the hospital that night and I was told that well I had obviously an anginal attack, a coronary schema. I would have to be observed and I was in the hospital and I would have to go on a diet. I was weighing too much because there were many banquets and luncheons and all sorts of things and I gained a good bit of weight. And it did not develop into an infarction or a thrombosis but I was warned that I was having problems with the schema, coronary schema. I would have to reduce. I would have to reduce my weight and perhaps also my activity some. Well within about four, five weeks after this episode which took me out of activity because I had to be in the hospital and then I had to rest at home, and I began a reducing diet. Nevertheless I had to go off to Washington to testify before Congress and many other meetings. And I had many other engagements. I won't go into now because there were all sorts of international activities that were associated with this.

Q: Well we can always come back.

A: No, there is much too much. We just don't have the time to go into it in detail. But what happened was that I went back and all over Europe, the United States and Brazil and actually it was in 1971 in October that I met very briefly the lady who is now my wife. I met Eloisa during a reception actually that was held as part of the Weizmann Institute activity in Brazil
and from there I went to the University of California at San Francisco to participate in a special symposium which was in honor of my old friend Stanley, Wendell Stanley who had died. And then I came back in time for the board of governors to Israel and after that in November again, I was sitting quietly working in my office. Everybody was gone. I was alone. There were no particular pressure. There was no crisis. And when I had crisis there was no pain. I had no problem. The two basic attacks that hospitalized me for coronary scemia came during periods when I was not under stress because I was rather--I had periods of elation and quiet, and I was sitting in the office alone and again I had another pain in my heart. It was a really bad one. I didn't think I could drive. I had my little car because the distance, the walking distance between my house or between my office and the house on the campus was more than I felt I could undertake. And again I was hospitalized. And this time I was again out of orticum bas till January. Fortunately by that time I had a very good vice president, a man whom I admired tremendously. He was the best organized scientist in Israel. He was chairman for many years over the Israel atomic commission. I had a hard time getting him because Golda Meier and Dian wouldn't let him go. His name was Israel Petrowski and he carried on very well. But, I was advised after the second attack, coronary schema that I had better have my coronary blood vessels examined to see what was there and so early in 1972 I went to Cleveland and they discovered that I needed to have coronary bypass surgery. The main artery and the left ventricle was almost 80%, about 80% obstructed, that if I
didn't have the operation which had been developed by then for it
the probability of my dying was at least 50-50 the next time
I got another such attack. So I had open heart surgery which
again took me out of commission for quite some time. And it was
then after that that I realized that my commitment, my compulsion
to do things that I believed had to be done was so great, and that
the changes that I believed that had to be made in the Weizmann
Institute to have it not only a small Institute of very good
people. And they were not all very good. There were a lot of
people who were not. Because as in any other institution perhaps
eight mediocres were riding on the coat tails of two top flight
people. And, Israel couldn't, and the Weizmann Institute couldn't
afford it. And the changes that would have to be brought about
in order to have the Weizmann Institute more directly involved
in the development of Israel than it was would require certain
important basic changes in the structure of the institute that
if I tried, to put them into operation as an American Jew as a
Johnny-come-lately, the sacrifices that would be called for
I thought I would not be able to achieve it. And also I thought
it would be much better if it were done through someone who really
grew up in Israel and who was a part, more a part of Israel than
I was and I believed that the man who was acting as vice president
then, Professor Catrovskei was the best man really to do that.
And that is why I decided and I am really jumping over a great,
great many--

Q Go ahead I'll come back to it but--.

A I decided in 1972 not to continue as president of the
Weizmann Institute although I had a contract. By that time I
had been a president for about three years. I had a contract for a longer period but that I would resign as president, hoping very much that Professor Petrovski would be made president and would somehow or other I would continue by working in the United States to help with the many ways in which the Weizmann Institute needed help, and especially from the United States. And in the middle of that year I was offered a Fogarty's, to be a Fogarty scholar at the Fogarty Institute, at the National Institutes of Health which I accepted. And so at the end of 1972 I resigned as president and returned to the United States. But there were many more problems, serious problems with difficulties with the board of governors, difficulties with operation, difficulties in fund raising, difficulties in finance and the institute was growing too rapidly and out of bounds it increased by about 500 people during the three years I was there. The budget was getting unmanageable, the sources of funds were becoming more and more difficult to come by and it became very much more difficult to work with a chairman of the board of governors who was not a person whom it was easy to work with. Nevertheless, we had an agreement by the time I left that I would continue to work together with the American group, the American friends of the Weizmann Institute but later it turned out to be impossible. So that it was a very exciting period. Of course it was also during this period that I continued to make trips back and forth to Naples to work with my scientific son who didn't turn out so well later. But that wasn't evident until 1972. As a matter of fact, the development of the work appeared to me, it appeared
that I could do a great deal in that I fell really to move it to an end point of decision so that as a Fogarty Scholar I wouldn't merely sit around and meditate as I originally intended on certain philosophical problems. But I really organized an activity to go to work and really work on those aspects of cancer research which I believed had to be done on which I described before at Fort Dietrich. Now I find that it is eight minutes to eleven. We have to stop at eleven. It has been a motor log in which I was just picking things off the top of my head.

Q Let me--

A eight minutes.

Q I won't attempt to do anything in ten minutes and I am going to shut the machine now.

END OF TAPE