Q  Okay, Dr. Sabin, we are on again.

A  Let me for a moment say something about how I tried to fulfill this mandate. For hepatitis and then I will discuss later how I wanted to see this same kind of mandate applied to expert consultants for the cancer program. This is the expert consultant to the U.S. Army medical research and development command. So let me say just a few things about the hepatitis activity while I was still in Washington at N.I.H.

I had of course known about developments in hepatitis. I was myself interested in hepatitis at one time but the tremendous advances that had been made in the field of hepatitis studies just in the preceding four, five years were such that I had to educate myself. So first of all I began to read voraciously hundreds of reports so I could bring myself right up to the horizon of where the new knowledge had brought us in the understanding of hepatitis. And because one of the places where some of the best work on hepatitis was going on was in the laboratory of infectious diseases in the National Institute for allergy and infectious diseases under the direction of my old, beloved Bob Channick. But it was done by what I would regard a scientific grandson. Someone whom Bob Channick had trained and then his name was Robert Purcell. Who then became a very good independent investigator in charge of this special field of hepatitis. And I regarded him as my scientific grandson and I had the greatest admiration for the beautiful organization of the work they had done on hepatitis so I had an entree, you see immediately right there on the ground of where
--of what was going on that wasn't published, wasn't anywhere. And I utilized this to the maximum. Furthermore, my first cousin, Dr. Saul Krugman had been one of the pioneers who continued on working on hepatitis despite the many difficulties when most people dropped it and he had a remarkable bank of material that everybody was using, with the new tools that came into being. And he was another source of information so that pretty soon I myself was in a position to begin to put together a summary of the situation, where it stood now and where it was going next. And one of the problems that really were learning problems at that particular point, 1974 was about the development of a vaccine from a subunit of the hepatitis "B" virus particle. I won't go into a discussion of the whole hepatitis problem in which I became very much involved. And I thought that I would bring together again as part of the mechanism of collaborative work that I had already visualized, the people who were most actively involved. And some of them really not talking to each other. Because one of the people who was already very actively involved in the field of possibly producing some sort of vaccine from such a subunit. It was called the hepatitis "B" virus surface antigen, was Dr. Morris Hilleman who was in charge of virus research for Merck, Sharpe and Dome Company. Certainly an excellent investigator, reliable investigator. A person with whom I had ties for many years and mutual respect and admiration who however was not in very good relation. He wasn't giving the time of day to the N.I.H. group who was studying hepatitis. And they weren't giving him the time of day. Well at any rate I remember bringing together a group, bringing together the N.I.H.
bringing together Morris Hilleman, bringing together the director of the bureau of biologics and his deputy who was also another scientific son, Parkman who had worked with Busher. You see this is much in the family sort of thing on a personal basis and Saul Krugman and I had, because at that time you see I forgot to mention that after I became a special consultant, full-time expert consultant at the National Cancer Institute, I was given a very excellent office in the administration building of the National Institutes of Health, with a full time secretary and this sort of conference room, very nicely set up. So that I had this conference where do we go next. What do we have to do in order to achieve an answer to the question whether or not a practically, useful safe hepatitis vaccine at least one of the proponents of hepatitis, viral hepatitis, that caused by hepatitis "B" could be developed. And also in that group I forgot to mention the other place for excellent studies on viral hepatitis were being carried on was at the Walter Reed Army Institute of Research of which my scientific son Ed Busher was commandant and who had grand sons so to speak. I am using this, this way--who were doing really top flight excellent, sophisticated work on viral hepatitis. I brought them in you see because this was the army and the representative from the U.S. Army medical research and development command, the deputy commander to General Dirks who was the commandant of this. And this was Colonel Barkquest. And the deputy director of the science--. I mean this was the group of decision makers and I had them all in my office around a conference table. It just held enough for a significant--not a workshop, not anything like
this. But a group to see where do we stand and where do we go from here. It was very useful I think in a sense. So let me leave this for a moment because it shows how I was using this wonderfully expressed mandate for the U.S. Army medical research and development panel to deal with a problem of importance to the army and of course my thinking was after all I am an expert consultant for the National Cancer Institute, I am particularly knowledgeable in the field of, the problem of, the ongoing problem of viruses in cancer in which hundreds of millions of dollars have already been spent over the years and which, incidentally, was under attack by the scientific community. I was also knowledgeable in other fields of cancer research, partly because I was trying to develop proper programs at the Weizmann Institute with people who were working in other fields, in immunology and cancer and differentiation inducing factors in cancer. And certain hormones in cancer. So I asked myself how could this concept of an expert advisory panel that the army was using, how could that be used, this consequently, to implement the National Cancer program through the use of expert full time, expert advisory consultants, which the act by that time there was a provision in the act for 100 such expert consultants. In other words, the thinking that went in was that this should be used some way but it wasn't being used. And by full time didn't necessarily mean that a person who still had a full time let's say university job would have to give it up, would have to resign. But I was already under the influence of my experiences in administrative, at the Weizmann Institute of Science, realizing that a man if I wanted to get something out of a man who was on the faculty of the Institute, you would
have to take a leave of absence from what he was doing. It would require one month, two months, three months, whatever, he was to have the, had to have the opportunity to give 100% of his time and thinking to the particular project. Then he could go back. This was what I meant full time. I didn't necessarily mean quitting one's job in a university and becoming another administrator in the National Cancer Institute. So, in preliminary discussions with Dr. Rauscher especially, it was decided that I put down my recommendations for such a program and I have a copy of this memorandum here which I am going to give you incidentally. In which basically it was a proposal of functions. You have it there.

Q Yes I have it already.

A Alright. A proposal of functions for a panel of N.C.I. expert consultants for accelerating progress in selective fields of cancer research. The emphasis was selective fields which means that first of all they would have to be identified and the emphasis was for accelerating progress. Now I am going to read this because I may want to make some remarks and that is alright?

Q That's fine. That's fine.

A And you will have this in the record.

Q Yes.

A So I start off with the reason for the proposal. Why have this. And I said that there are many fields of cancer research of great potential significance for the understanding and ultimate control of certain types of human cancer. Incidentally I regard this as fundamental basic research as fundamental and basic as anything else. I will discuss that aspect of the problem a
little later. I said certain types of human cancer again the understanding and ultimate control in which varying number of investigators have worked independently for many years and I am emphasizing the fact that they worked independent of one another for many years. And have come up with intriguing but not definitive results. And I underlined, I emphasized intriguing but not definitive. The literature in almost all fields of scientific research is full now that so many more thousands of people are working, of intriguing results. Results that fall into my category of how wonderful it would be if this were true and if this could accelerate the progress of what we could do with this or the other human cancer. You see this is what I had in mind.

And then I continue from here now. There is a need I said for developing a mechanism or mechanisms by which it may be possible to accelerate progress and translating intriguing observations into definitive conclusions. And reliable stepping stones for further research. I saw that as a very— I am speaking aside now, not from the manuscript. I saw that as a great barrier to progress because I knew I could list offhand any number of fields in which people have been working for ten years with intriguing but contradictory, not definitive results simply because certain things have not been done and which should be done and which no amount of publications, no amount of international meetings, no amount of so called workshops in which nobody takes on the responsibility to do anything, just listens, and you hope that somehow or other it will have a cross-fertilization effect and maybe it has but it just goes on anyway for years and years. So that this was in the back of my mind why the proposal I was making was needed, the
reason for it. So now I go on to describe the conscience of the proposed panel. One, the first job, select fields of cancer research which fit the above category. That is, years of work independent investigators of no definitive conclusions. Select the fields which fit the above category, placing special emphasis on those areas in which definitive reproducible results and the emphasis is on reproducible and definitive, would provide reliable methods for studies on the epidemiology, pathogenesis therapy or prevention of human cancer. And I listed a whole number of examples of independents and I underlined human cancer. I need to explain the background of my thinking for this.

The field of cancer research is full of studies in model systems. The studies of model systems are absolutely essential as a preliminary orientation into the methodology, and the what would have to be done to get a greater understanding of the human cancer problem. To me, as a human oriented person, model systems had a role only to the extent that they led us to the human problem. And not just to the phenomenon that you might work out for a mouse or a cat or whatever. The study on the mouse or the cat to me was important only insofar as it would give you a tool for approaching the human problem. That also if you have this in mind influences how you construct your model system. It means that you have to have some sort of concepts of the human cancer problem so that when you construct a model system, the model system would bear some relationship to the human problems so that if you found certain things in a model system you could then carry them over to the human problem. This is what I emphasized, the human cancer. I think even now model systems
are being explored ad nauseum in a way in which I can see no possible relationship ultimately to elucidating the human cancer problem. I cannot give you a figure of how much of the hundreds of millions of dollars that are being expended over the years in cancer research are being spent on model systems which I think are sterile a priori. I think. I said I think. I think other people also think. But there must be some mechanism for making a decision ultimately how long do you continue before you try to get a definitive answer. Yes, this methodology, this approach is valid. Let's study it in human cancer and let's see what the results are. I will not go into the appendix now.

But let me take the next point. Having identified the selected fields, and this is problem number 1. How do you do that. How do I individualize (?) You select a number of full time expert consultants with expertise in the different fields that are related to cancer research. And you don't have one of those workshops in which papers are presented and no conclusion ever reached. Too many people. You have them on the basis of their knowledge, try to identify which of the many fields in which there is a great deal of activity going on now, are there things that remain unresolved. Because of uncoordinated, independent, prima dona efforts if one doesn't know which is which. This was obviously the first step, to select a number of fields and in the appendix I think I listed twenty or something like that myself. Or, no. I listed ten off the top of my head. As examples.

Q Right.

A Let's assume that that has been done. I listed ten. There are probably twenty. You get a certain group together.
If you get a group of ten expert consultants together representing different fields of activity, if I could list ten, each of them could probably list ten and you would have a hundred. Now there is a job, again, you have to--. Somebody has to make a choice. You see. In addition to a provision for certain group that will just follow and do good, innovative research, not gilding the lily. There is a need to make some decision. What are the intriguing findings that we must find out. Are they or are they not true. Are or are they not relevant to the next step. So there would be a job of perhaps developing priorities, making a decision however let's say faulty it might prove to be, that out of these hundred fields, let's say these ten or twenty deserve our special attention or our initial attention and then when these are eliminated, one could perhaps go to the others.

You see there is a need for planning. There is a need for dealing with current, highly complex research and highly complex fields that goes beyond the individual investigator. Not eliminating the individual investigator but supplementing the activities. There must be a general staff in a sense, a general staff in the sense that the Rockefeller Foundation used a general staff fifty years before in making decisions on where is the thrust. What are the challenges. Alright, this, this is in the background before I read here the next point.

Having done this, evaluate the present status of the data in each area and determine whether or not, whether or not you see, coordination of the efforts of individual investigators or development of "investigator initiated and investigator monitored". I underlined that. Collaborative efforts can be
brought into being by means of catalytic activities of the expert consultant who is given the responsibility for a given area. Now I must explain that, again. Although it will become clearer now in the light of the mandate of the organization of the army panel, advisory panel. In the first place, that if ten fields are selected out of a hundred, then it wouldn't be turned over then to a professional staff of the national cancer program or the national cancer institute. But the expert consultant who is responsible for bringing together, analyzing the study, summarizing it, bringing the needs into focus, that he is not then discharged from his duties and have his major interest and expertise that he has brought to that sort of grafted onto some third, fourth, or fifth grade hack who is then employed as a full time employee of the national cancer institute. But he carries on the fulfillment of the objective that is decided on by the group. And why did I emphasize investigator initiated and investigator monitored. Because one of the great problems is that monitoring of organized collaborative research by somebody at the National Cancer Institute is not the way to do it. And it must, you must retain. I tried to emphasize despite the fact that you are trying to bring about coordination, that it is investigator initiated, you see, and I am going to describe how I proposed that that be done. That there not be a feeling that somebody in the administration building in the National Cancer Institute in Bethesda worked out a plan and then gives it out by advertising and says alright you come up with proposals as how you are going to handle this. It is no. My idea is that the plan should develop from among
the investigators who are going to do the work. That is what I mean by investigator initiated. And that the monitoring of the outcome of the work should continue to be monitored by the group of investigators who would be involved in that work. In other words, to put the emphasis on the investigators, that the initiative is not taken away from them. But that their initiative is made into a stronger effort by working together and monitoring each other instead of continuing at odds by a variety of different methodology and so on so that years go by and we have no endpoint of decision.

Now this is the reason for the things that I emphasize in point number 2. And that leads to point number 3. I said that at the request then of the director of the national cancer institute, evaluate in depth on-going extramural or intramural or both programs and make recommendations that might be the basis either for phasing them out or for modifying or expanding them. You see, I am following the mandate of General Tygert wrote down there which appealed to me very much because it is reasonable. This hit the nerve. This is the very disturbing thing to the equilibrium of the present system. Can you imagine, having a separate board of outsiders, competent people perhaps without or with the minimum possible amount of bias who would do—and I am repeating, they would evaluate in depth the on-going extramural, intramural research program. Now that is supposed to be the function of the advisory board to the national cancer program. But those people are all working all the time. They come in for a few days. This, in my judgement was to be a full time job of somebody appointed, one or two persons per field, you see who would give it their entire time. And not merely get
twenty pounds of papers before a meeting and come and listen and then go home. You see, this is, these are some of the thoughts behind these statements here. But, this is of course very touchy. And then, what is the next touchy thing. Not only to make an evaluation of these programs but to make recommendations that might be the basis. I didn't say to say well this has got to be cut out. I didn't foresee any such dictatorial basis. That is why I chose my language very carefully. Make recommendations that might become the basis for a broader decision by responsible groups, either for phasing them out and some things had to be phased out because you can't continue adding to the periphery. There was a built in momentum that once an investigator starts on a program he doesn't let go. Unless somebody takes the money away from him. This sounds very, very rough, perhaps inhumane to the investigator. But you cannot run a system, a biological system or any other kind of organization with limited, finite resources unless somebody does it. It is impossible. I like to think of the human body. Take away the freedom from the cells that keep producing either red blood cells or white blood cells in the body, say no they must have the freedom to do, and they would kill the whole organism that way. Either with polysithemia or with leukemia, with undifferentiated cells, the body in its wisdom has developed a way of a feedback that when enough has been done, stop. We need the same sort of thing in the big scientific enterprise which cannot be run on the basis of 1000 or a hundred thousand individual prima donas each one doing their thing. It is an absolute impossibility and if anybody says that using the power of the purse is something inhuman, and contrary to the best
ethics of the scientific procedure, to phase out, to stop a certain scientific activity, he is totally unrealistic and if that is not done, it will destroy the whole effort ultimately. And it is more danger than the fact that an individual investigator who has his funds cut off and will have to begin to think anew instead of just going the easy way in a groove, just digging, digging, digging when it is obvious there is no gold there. You see these were the thoughts behind these very carefully chosen phrases. Evaluate in depth the ongoing extramural or intramural research programs, make recommendations that might be the basis either for phasing them out or the reverse, in modifying them, or expanding them. Because there are certain activities that have much more promise than others and somebody, there has to be a mechanism for making a decision that this should be expanded and this should be phased out. The assumption that there is no such basis for making such decisions is self defeating because there is no alternative. If you consider the alternative of doing it at all, it is worse than anything. You may make mistakes in deciding to phase out something. I said phase out, not cut off abruptly. Phase out. You may make a mistake in phasing out one thing and expanding another. But when you consider the alternative. What is the alternative. The alternative is anarchy.

Q In effect, aren't you saying to the high command of N.C.I., look you are not doing your job.

A I was saying that precisely. And I would say not only to the high command of N.C.I. of the national cancer program. I would say that as I will come later to the whole national institutes of health. And the whole biomedical research activity.
I will stick to biomedical research because it applies to other research activity as well. Because this has a much broader bearing. I has a bearing on the modus operandi of the highly complex activity that scientific research has become. It is no longer in an era in which scientific research was a privilege, or the field of activity for a few explorers where beyond that stage. There is still a need for exploration. There is still a need for explorers. But my God it has become a tremendously complex activity, expanding in an extraordinarily rate for some sort of built in feedback, some control mechanisms for decision have to be utilized. There is no. The alternative is, as I said, anarchy. You will make mistakes with what I have just suggested. But the alternative is anarchy. And the present method for making decisions on what has to be phased out, modified or expanded in my judgement are totally inadequate because it becomes either an activity of very low echelon people because the high echelon people are only giving a little bit of their time to this and they cannot possibly do it. And my experience of going in and making a study of the hepatitis problem about which I was totally ignorant. But in which I gained insights that I didn't have when I went into it in very great detail, particularly in the field of ongoing activities that won't be published for two years and getting an idea of where the controversies are, the spoken and unspoken controversies. That when I did that, I gained even a greater insight than all my previous decades of experience had given me. And that is why I felt then, or believed rather not only felt but believed that called together the system of advisory boards being altogether for a few days
during the year does not achieve the purpose. So now I, having said that and I know that it is stirred up and caused considerable consternation when Roger took it to discuss with the division chiefs. I proposed a composition and modus operandi of the panel. See I have always felt that when I worked as a consultant that I should not only criticize the defects of the present system, that if I proposed something else I should go into detail. How I visualize it to be constituted, operate, at least to start out with you modify as you go along. You adapt. But you've got to have a plan of operation.

Let me digress here again. I sometimes wonder why I work this way in the scientific field aside from tackling an individual small piece of the action. I don't know whether it is correct, whether I am attributing it to an experience in my life that I had which may not be relevant. But I feel that I can trace it back to my very limited period of training as an army medical officer when the basic principles of military science were included in my training at Carlisle barracks. Now I have been associated with army and military people long enough to know that many of those in uniform do not apply those basic principles and some of the many failures are due to the very fact that those basic principles are not applied. But nevertheless there is something very important in those basic principles and I have the feeling that my way of trying to set things out in a planned, organized fashion of almost step by step this I do now. This I do next, in order to reach the final objective, that somehow or other I was influenced by those basic military principles that I learned and I think that are applicable to all sorts of other activities
of planning. Not only for an army campaign, not only for a scientific program but also for the business and other things. Now let me go on to the next part and see how much digression I will make in reading about the recommendations for the composition and modus operandi of the panel. We are now on page 2 of that.

I say it is proposed that the director of the national cancer institute recruit an ad hoc and I emphasized ad hoc which means only for a specific purpose and they can be recruited for a month, two months, a year or whatever it is and then they go back. Not to add on to the administrative structure forever. You see not to have a civil service appointment but under the act, under the National Cancer Program, in which he is allowed 100 expert consultants, recruit ad hoc, panel, of critical investigators of established scientific stature who have a broad knowledge of one or more of the many different fields of cancer research in order properly to perform the envisioned catalytic functions. Now I want to stress the word catalytic here. That they merely bring the people together and don't do anything themselves, you see. They don't participate in the reaction. They help to have the participants in the reaction, the components of the reaction, bring them together but not themselves be a part of the end product. So let me continue now.

In order properly to perform the envisioned catalytic functions as I just defined them, it is highly desirable that the expert consultants have no current, personal involvement. I emphasized no current personal involvement in the field of
cancer research that he is competent to evaluate. And I want to
digress. What did I have in mind when I said that. I had in
mind my experience of the inevitable competitiveness that exists
as a fact of life among scientific investigators working in the
same field. They are competitors and it is good. And therefore
there is a certain distrust and for this reason I was suggesting
that as far as possible people who may now be in administrative
positions in universities or people like myself, in 1974, I was
already 68 years old at the time. I was almost 68 at the time I
was writing this.

Q A strip link (?)

A I was already but I was no longer involved in laboratory
work and there are other people also in that stage of emeritus,
early emeritus or post emeritus years who while they may not be
very good in a laboratory any more have years of experience of
critical evaluation and synthesis of data and who could be
utilized for that purpose and would not represent a person with
self interests to the group of investigators he might bring
together and would not himself be doing work so that he couldn't
be accused of going off to his own lab and organizing a group
quickly to get ahead of somebody else. In other words, picking
everybody's brains in order to do things for himself. Can you
imagine somebody who is still engaged, actively engaged in a
specific field going out and trying to pick everybody else's
brains, how, the kind of reaction he would get. I know the
kind of reaction. It is negative. Even the other isn't
absolutely positive. But it is an important consideration and
I think there is an important need in the scientific enterprise
to institutionalize the utilization of people who are no longer themselves engaged in laboratory work, but who have a record of achievement in the past and a record of being critical investigators who have exhibited a good capability of putting things together. This was the basis for my statement and emphasis that as far as possible, I said highly desirable, not necessarily exclusive. It may not always be possible, but highly desirable that the expert consultant have no current personal involvement. He may have been involved years before and stopped. This is very important. I say then that it is envisioned that this panel of expert consultants would work as a group only for the sake of general perspective and consideration of priorities. I already mentioned that before. First of all as a group to select the priorities of the selected fields of importance. Otherwise they would work mainly as individuals with responsibility for specific areas of research, those specific areas would be chosen by the group. Now the authority and logistic support for the work of this panel would come from the director of the National Cancer Institute. It is not an attempt to make an end run around the national cancer institute to say that he is not doing his job. This is like a cabinet. This is like a group to support the extensive responsibility of the director of the national cancer institute which he was not now using. He was using buddies, in the system, in the cancer institute. And the sometime advise put in from this or that member of the advisory board based only on casual exposure or self interest. Because some of the people on the advisory board are currently working in the field and do have the self interest so actually this again,
when I say that it is envisioned that this panel would work, that the authority and logistics for the work of this panel would come from the director of N.C.I. He remains the boss. He appoints them. He gets their advice. He then must make the decision of what to do with it. They are responsible to him. So it is not an end run around the director. It is only making the work of the director more fulfilling, the way it was originally envisioned. The recommendations of the panel would also be made to the director. In other words, he sets them up, he provides them with logistic support and then they make the recommendations to him. Whether directly by individual members or only after consideration and decision by the whole panel can be a matter of discussion. But it would be made to the director rather than to the division chief.

The division chiefs in the national cancer institute, some of whom have 50 million dollars for their program, some have a hundred million dollars per annum. They have built in self interests. I've been there; lived with them long enough to know that they, that the business of empire building is an inescapable part. That no division chief is out to cut his own activity. That each division chief wants more autonomy, more money out of the whole cancer research budget so the recommendations cannot and must not be made to the division chiefs. They must be made to the director. And I left open the question about whether or not the recommendations shouldn't be made to the total panel of expert consultants who then even though they may not be expert in that particular field, would have the opportunity to ask questions. Because ultimately, the
decision would have to be made which of the many recommendations there is enough money to support. You see because this is decision making at multiple levels, you see, which it must be. And actually if I had, if this thing had gone through, I would have seen that any one individual expert who is given a special field to develop and investigate and make recommendations, he reports it back to the whole panel so they would all know it directly before he even makes his final recommendations, and not get it down by a piece of paper that is read to be submitted to the director. He would submit it to the director and to then simultaneously and then he would have to come before a meeting of the director and this advisory cabinet which it basically is, or general staff if you like, and defend the recommendations. And to the extent that it may be desirable to modify those recommendations on the basis of an input of the critical expertise of the others, he would do so. But the important point here is is that it must be to the director and to the general staff. Call them a cabinet, call them general staff. Some people are very opposed to the use of military terms in this era in American life. So I don't want to necessarily insist on words that immediately call up attitudes which are opposed. But, a cabinet it is a cabinet see. Now I said here rather than to division chiefs and this was very important. Because what Rauscher did with this and I am doing it a little prematurely before I read the whole thing. Was to discuss this with his division chiefs and they were constantly threatened by this. And they were dead set against it. In my judgement he should have taken it to the advisory board, not to the division chiefs. The domain
of the division chiefs must be determined by a higher level of decision because they will not give up one bit of their domain. They will not allow it to be phased out. If you are going to decide to expand it they will be very happy. But you see the problem there.

Q Yes.
A That one faces and one faces that not only here but at every level of policy decisions whether it be biomedical research or in other kinds of research. Because here are the obstacles to necessary reorientation, necessary restructuring of the decision making apparatus. I remember here again is a digression, of many private discussions that I had with the then secretary of health education and welfare, Casper Weinberger, who I think came in to this cabinet position, was appointed by Nixon, because he previously served as director of the officers management and budget, had a long experience in accounting, in business and so on. And he--

END OF TAPE