Dr. Carl Baker, former Director of the National Cancer Institute (NCI)

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Interviewer: Dr. Robert Stevenson, formerly of the National Cancer Institute

Baker: Well, Bob, shall I give you, just for the record, a brief background of my experience?

Stevenson: Please do.

Baker: I graduated from the University of Louisville, first with a Bachelor's degree in zoology in 1942, an M.D. degree in 1944, interned in Milwaukee County Hospital, went back on active duty in the Navy as a physician in the Navy, including going through Combat School at Oceanside in the Marine Corps, and decided when I was an intern that I really wanted to go into research, particularly in cancer, since we needed improvements in cancer therapy and prevention. I decided, when I was stationed at San Francisco, that I would seek entry at Berkeley in biochemistry and the Head of Biochemistry, Dr. David Greenberg, helped me get a Jane Coffin Childs Fellowship, which supported me for a couple of years at Berkeley.

Jesse Greenstein, Head of Biochemistry at NCI, gave a series of lectures one summer in Berkeley and invited me to come to NCI, which I did on January 1, 1949. I was in the Lab of Biochemistry working on mostly amino acid biochemistry and peptides. I got asthma from lab animals, which moved me into the administrative side quicker than I would have otherwise. So I joined Ralph Meader's staff in Grants. I went back to the lab when the Clinical Center opened, but the asthma still bothered me, and so I left the lab for good.

I was assistant to Joe Smadel in Building 1 for a year and nine months, and went back to NCI as Assistant Director.

Stevenson: Now, what were the dates of those?

Baker: I was in Grants from 1952 to 1953, back to the Lab of Biochemistry for a couple of years, 1953 to1955. I was in Building 1 with Dr. Smadel 1956-58, and back to NCI as Assistant Director in 1958, and I was also Acting Scientific Director of the NCI for part of that time.

Then, when Endicott came on board in 1960, after Rod Heller left, we had a re-organization shortly thereafter and I was appointed Associate Director for Programs, which was a confusing label, but it basically meant anything Dr. Endicott wanted it to mean. But I did have main responsibilities for program analysis and planning.

And when we were unable to replace Paul Kotin as Scientific Director for Etiology, I went and headed that area for a couple of years. Those years were, as Scientific Director for Etiology, 1967 to 1969. And then I became Director of NCI after that, until 1972, when the new Cancer Act made the heads of NCI and the NIH Presidential appointments. I was not appointed by Mr. Nixon.

So, I briefly headed Hazelton Labs, but again got problems with asthma from lab animals and became a private consultant for a year and a half. Then I went back with Endicott in the Health Resources Administration and was glad to get an offer to be Medical Director for the Ludwig Institute for Cancer Research, which--

Stevenson: What year was that?

Baker: 1976. I was on the Scientific Advisory Committee from its beginning in December, 1971. I became the Institute's first Medical Director in 1977 and stayed as Medical Director for the Institute until 1982, when I became Emeritus Medical Director. From 1977 to 1982 I was responsible for the day-to-day science and medical management of the Institute's activities.

Then I've been teaching some at the University of Maryland, University College, first in General Science and later Science and Society Courses. I also taught Group Dynamics and Organizational Behavior at Columbia Union College prior to teaching at Maryland.

Stevenson: Okay. But you neglected one thing. Your mother didn't find you under a toadstool. What year were you born, and where?

Baker: Oh, 1920, in Louisville, Kentucky.

Stevenson: All right.

Baker: Do you want to turn to the questions that we've been asking other people that we've interviewed?

Stevenson: Yes. At least get those out of the way.

Baker: Well, the first one deals with the most significant scientific findings, and who made them. I think it's valid to divide the area of cancer viruses into three periods.

Before 1953, most people thought viruses had nothing to do with cancer. Peyton Rous had developed, from 1910, his sarcomas in chickens and he did a fair amount of work in that area, particularly with Joe Beard later, and people said it couldn't be cancer because it was viral-induced. That was the kind of thinking that was common in those days. Beard, Bryan, and Burmester kept the flame alive during this period.

In addition to certain methodology advances, I think the change in outlook to where most of the scientific community accepted the idea that viruses were involved in cancer induction came with the work of Ludwig Gross, where he showed that cell-free extracts would produce leukemia. But this work from 1951 was not confirmed until about 1953, so I think it was at that point that we had a real change in outlook, where interest in viral induction of cancers became fairly well accepted, and we had a blossoming of work.The *Polyoma* work with Stewart and Eddy also created a great deal of interest, and then we had a whole spate of various tumors being produced by viruses, many of which bear the names of those who first found them, such as Moloney leukemia and sarcoma viruses; Rauscher virus; Friend virus; Abelson; Lieberman and Kaplan; Kirsten; and so on.

This era continued, I think, on up until the oncogenes were demonstrated. And this led to a shift from the idea that viruses, per se, induced cancer, to the concept that the genetic information in our own chromosomes have similar sequences to the sequences in the tumor-causing viruses. So this led to a shift of viewpoint from viral causation of cancer to causation on the basis of genetic information present in chromosomes of higher organisms.

Varmus and Bishop, of course, made the most significant findings in the oncogene area. And prior to that Huebner and Todaro wrote an interesting paper postulating oncogenes, although they thought it was related to production of C type antigens, so they didn't have it quite right, but I think it influenced the thinking of a number of people.

The reverse transcriptase findings of Baltimore and Temin and the provirus theory of Temin were very significant steps along the way, with reverse transcriptase providing, first, an answer of how RNA viruses could induce tumors by way of DNA in the cells and, perhaps more significantly, provided tools that--along with plaque assays of Dulbecco, Huebner, Wally Rowe, and others, allowed us to have the tools that led to the oncogene developments.

More recently, the findings that some genes can repress other genes, or de-repress certain other genes, has added to the complexity of understanding cancer causation; and have stimulated great interest in the rapid pace of developments in this field.

So, those are some of the highlights. Of course, there are subsidiary things, like the developments in tissue culture with Wilton Earle and George Guy being pioneers in these developments. The ability to freeze and thaw cell lines effectively, the ability to understand the quality control aspects in tissue culture; animals, primates included; other reagents; virus preparations; and so on, I think contributed very significantly to these developments during this period.

So, that would be my short answer to the first question. Do you have any other questions along this line?

Stevenson: No.

Baker: Well, the second question deals with what key administrative or management decisions were made that affected this field, and who made them, and I think Ken Endicott certainly is one who should be remembered for his willingness to seek a special appropriation from the Congress for the Viruses Cancer effort. That development was based on briefing of Endicott primarily by information put together by Bryan, Rauscher and me, with Zubrod reviewing some of the documents, which provided the justification for asking for a special appropriation of $10 million dollars.

Needless to say, Jim Shannon wanted a great deal of justification provided before he agreed that Endicott should go and ask for the funds. So a fairly long document was prepared for Shannon and, as was customary, or at least on occasions, Shannon wanted additional information after he had read that. We then prepared some more information for Shannon. So this was one critical point.

Now, prior to that, there were important decisions made, and I think we should go back to 1960 or so when, in the Grants Area there was a Viruses Cancer Program under Harvey Scudder's direction in Ralph Meader's shop. The Microbiology Study Section, later the Virology and Rickettsiology Study Section, provided a good deal of input to Dr. Scudder on kinds of resources needed. So, Dr. Scudder initiated a grants funded Viruses Cancer Program to provide special resources. Some of the motivation for establishing that program came from Wendell Stanley's testimony before the Congress arguing that the time was ripe for expansion of the Cancer Viruses area.

Stevenson: Who stage-managed his appearance before Congress?

Baker: Well, he was on the Cancer Council, and we had a number of informal discussions with him--"we" being Endicott and I and Ralph Meader and others, some of the other Council members--and I think he already had this concept well before that. I don't have the information on how he got selected as a witness.

Stevenson: Did Dr. Shannon have anything to do with that appearance?

Baker: Not that I know of. But there was a meeting in 1957 at M.D. Anderson, as one of their annual meetings, where leading proponents of the idea that viruses caused cancer, included Stanley. This meeting included the outstanding virologists of the day who were proponents of expanding cancer virology work and Stanley was a key member of that group. So, there was a group, that included Syverton, that included Ludwig Gross, included Goodpasture, included Sabin and people like that. Syverton was a key person who was on the Council also. So, these people already, I think, had the idea that we ought to be having more funding in this direction.

Stevenson: I have a feeling that Syverton was the propelling force behind a lot of this, and his untimely death in 1961 removed him from the stage. I had very little opportunity to get much history out of him but I'm sure, you know, I could have learned a lot of interesting background material for this from him.

Baker: We had been helped from people like that, particularly on the Council. Chuck Evans, from the Microbiology Department at the University of Washington, I remember as being very helpful in initiating and funding programs at that early period. So, I think Bryan, Rauscher and I and Endicott were key individuals in pulling the managerial aspects of this together. Earlier Scudder, and you, yourself, Bob, played a key role in implementing it and actually managing these resources programs.

So, that's part of the third question. Thus, my main participation, early, was helping pull together the information that formed the justification for the special request. After Congress voted the special appropriation of $10 million, Endicott came in to see Carrese and me and said, "Okay, you guys have been talking about planning, plan me a $10 million dollar program in cancer virology." So this led to a more formal pulling together of the managerial guidelines, so to speak, in which Carrese and I utilized the technique we developed and called the Convergence Technique, which is a systems planning method, modified considerably from things like PERT because in *research* programming the degrees of uncertainty are so high you can't do it the same way you would do it in the usual systems planning, so we made modifications of that.

I must say, most of the virologists didn't pay much attention to these Convergence Technique systems plans unlike the chemotherapy people who came in with a whole process of drug development according to the systems plan and used it increasingly. Later on, in chemical carcinogenesis, those people also used the plans, especially to help get funds, but, by and large, the virology scientists didn't pay much attention to the plans. However, it was very useful from our managerial end to integrate various discipline-oriented components into one integrated whole program which has certain strengths that you can't have if each bit and piece is separately run.

So, this led to a difference in philosophy of how one goes about programming. Most people still believe that the individual investigator initiating what he wants to do and having peer review in his scientific discipline is the way to go and many people say the "only" way to go.

I originally got on the idea of using systems planning for budget development. How do you decide what you go to the Congress to ask for in terms of funding, and how much, and what are the relative priorities? The systems networking provides a way of looking at the various components *in toto*, seeing relationships among them, and looking at that kind of networking to reiterate various possibilities on budget development.

And then, once that's done, it provides considerable help for answering the question, "Why did you ask for this particular amount of money in this particular part of the budget?" This networking technique then is a very useful technique for systems planning.

So, this led to our development of the Convergence Technique and that and my subsequent support of the viruses cancer area as Scientific Director for Etiology, after being Associate Director for Program, and also later as Institute Director, demonstrated my participation in the strong support the whole area.

The next question is, "Who were the main leaders who influenced the direction and course of events?" Well, this is slightly different from the question of who made the main scientific findings, because some of the leaders in developing programmatic aspects weren't necessarily the same scientists who made the outstanding research developments.

For example, Joe Melnick was one who certainly influenced the course of things by his activities on the Council and various committees, even though he didn't have the kind of research findings that matched the findings of Bishop and Varmus. But yet I would call Joe Melnick one of the leaders in helping this program go along.

Sabin was another example who, in as far as the cancer field goes, didn't make necessarily any outstanding findings, but he was helpful in his advice and reviewing things and bringing us new information, and so I would include him in the group.

Werner and Gertrude Henle were another helpful pair who might be included in this category. Hillary Koprowsky was another one.

On the Council, Syverton and Evans were certainly two that helped move things along.

Do you have any additions to this?

Stevenson: No.

Baker: I've already mentioned the internal ones, like Bryant and Rauscher and Moloney and people like that.

Stevenson: Some of the people, for example, such as Werner and Gertrude Henle, who were working on DNA viruses, like the Epstein-Barr/Burkitt lymphoma complex and nasopharyngeal carcinoma, *et cetera*, were sort of like voices in the wilderness calling out for approaches to non-RNA viruses. The steadfast refusal of the scientific community to call or designate Burkitt's lymphoma as a true cancer, to me, has been one of the enigmas of all time. The E-B virus fulfills all of Koch's postulates, it does all of the things which one needs to do to prove etiology and so forth, and yet, for some reason, it was always relegated to the back burner in terms of attention and in terms of importance. It's a whale of a disease in Africa and Asia, not particularly a big one in the United States where the Epstein-Barr virus causes mononucleosis more than nasopharyngeal carcinoma, but still the recalcitrance of members of the scientific community sometimes to embrace or deal with phenomena, concepts, and so forth, that are staring them in the face, to me is a remarkable phenomenon.

Baker: Well, at one period perhaps part of the answer is that the RNA viruses were generally smaller in molecular size and the number of genes present were of sufficiently small number that it was easier to identify which genes did what, such as either produce cancer transformations or were necessary for replication, so a virologist is often much more interested in that sort of thing than he is in the fact that some complicated virus causes a disease in Africa. I might say this is part of the answer to why.

Stevenson: It's like the drunk who lost the parakeet and looks for it under the light post because that's where the light is.

Baker: Yes.

Stevenson: Okay.

Baker: So I think we've mentioned, certainly, some of the people who were outside the Cancer Institute who were very helpful in both advice and in making sure that programs moved along and that funds were provided.

A lot of people who were not familiar with the details have been, and are still, under the impression that the reviews given for the individual contract projects were somehow not as well done and didn't have as outstanding people reviewing them as grant proposals had. I think, if you look at who sat on the review groups in the areas supported with contracts, it was the same people generally that were on the study sections at one time or another so, for one, I don't think the review groups were of less scientific capability.

Secondly, the question of whether the projects that were funded with contracts were of lower caliber than the grants is related to what criteria one wants to apply. Most of the contracts had a different basis entirely than the basis for grants, so the criteria that would be useful for reviewing basic research grants were very different from those used to see whether a project would provide quality control of viruses, or tissue cultures and other resources. I think there is some confusion here on the part of those who have not worked in both areas. They're different worlds with different criteria and different purposes, and often different kinds of expertise are required even for review of proposals.

And so, from my point of view, it didn't matter to me whether it was grant supported or contract supported; it didn't matter to me whether it was in-house or out-house. Here were certain areas that needed work done. Let's get on with it.

Stevenson: I think the final evaluation of whether something produced what it was supposed to produce of good quality and of sufficient quantity and on time and within cost were criteria that were seldom, if ever, applied to the results of grant activities. There, the end result is a scientific publication, peer reviewed, that eventually gets into a journal and somebody reads it and is supposed to add to their accumulated store of knowledge. But, in many of the areas that we were talking about, resource development was the end desirable object, and the only way you could do that would be to quantitate and put a time line on when you expected the material and whether it was of sufficient quality to do what was expected.

Baker: Well, I was always sorry we seemed to have this schism between the grants area and the other areas. I think with respect to the people on the staff who ran these contract funded programs, we expected a lot from them, because it required, not only knowledge of science, but managerial ability, and not everybody can do that. You've heard me say I thought you combined those two skills very well, and I don't just tell you casually that--I've told other people the same thing--but it is because I believe it.

So, for example, Moloney certainly carried a very heavy managerial load after Rauscher left, or even while Rauscher was Director.

One of the questions deals with whether there were lay individuals who played a role here, and certainly Mary Lasker comes to mind, of course, as such a person. She wasn't specifically supporting the Viruses Cancer area; she was supporting the cancer field broadly. I think Mary always wanted more applied work than was being done. That's partly why we got Cancer Control given back to us with the Cancer Act of 1971.

A question was raised as to whether there were scientific politicians involved and, well, Sidney Farber, I think, could be described as a scientific politician, a politician who was also involved in science and medicine, and a very good one. He was wonderful in his testifying before Congress. He combined the kindly doctor, treating children with a terrible disease, with an outstanding pathologist on the Harvard University faculty, and lucky enough to have discovered one of the first useful chemotherapeutic agents in human cancers. He did a great job in testifying each year.

I was asked every year just before he testified to go up and brief Dr. Farber on the latest findings, which I could do something about because where I sat at NCI I had good inputs, and it was interesting that always lunch was the same, a lamb chop and peas in his office; but they were good.

Well, let's see what's next here. I've already touched on how significant the availability of quality controlled resources were and are. I think one of the great contributions of the Special Virus Leukemia Program and the Special Virus Cancer Program and earlier the Virus Cancer Program was producing quality controlled resources which, at that time, were not available. When we talked to the outstanding virologists who had been in the polio area and were ready, perhaps, for change because the polio problem had pretty well been solved, I got to know several of those virologists. I admired the fact that they were very good about exchanging samples of resource materials that they had produced to make sure that they were of sufficient quality, but I said, "By the time you guys send your samples around to each other, you don't have any left to work with because you're not making enough quantity, and the way to go is to have industry produce these."

"Oh, well, industry can't make the materials good enough."

I said, "Well, we can require your same criteria for quality and you guys can test them, and obviously we're not going to try to make you use them if the quality is no good."

"Well, they can't make them good enough."

Well, I knew I was over that hump when Moloney came in one day and said, "Guess what? The Pfizer people have sent us Moloney virus and it's as good as anything we ever produced, and we've got buckets of it."

And so they were finally convinced that it could be done. But again, it was the attention from the program side of proper quality control and making sure that the contracts that were awarded included these requirements.

Stevenson: Harvey Scudder as head of the Virus Cancer Program bought all of the viral reagents that had been produced by the March of Dimes. They had required large amounts of various enteric virus-related antisera and antigens, and had gone out to people like Herb Wenner at the University of Kansas and produced a large amount of these materials in large animals--cows, sheep, goats, and so forth--and Harvey was able to buy up a lot of their residual inventory since they had gotten out of the business of isolating and identifying new viruses and stuff, and he added that to the V&C Program early on, and that was considered quite a leg up in terms of not having to go out and produce this material, and it was not commercially available.

That developed into a Viral Reagents Program which eventually, in a year or so, went to the Institute of Allergy and Infectious Diseases thereby precipitating a crisis in the Virus and Cancer Panel, which caused Bayne-Jones to resign. But, nonetheless, that was a major resource which then the Allergy Institute carried on for a number of years and added to those reagents and made them generally available to investigators throughout the United States and abroad.

Baker: I think it's hard for people--scientists--working today to realize how sparse quantities of these resources were when today you can go buy most of these things commercially. And I'm pretty sure that this program laid the groundwork for much of the biotechnology advances. We now purchase things from catalogs which, in those days, were almost impossible to come by.

And also, in the case of primates, at that time it appeared that we were going to need a lot more primates to assay samples that came from humans with the idea that the higher primates were more closely resembling man and therefore you had a better chance, if we found a virus in man, we could perhaps evaluate it in primates. But the primates in those days were full of disease--I suppose they still are, the ones that are imported--and it turned out that we had very little information on what I would call animal husbandry data. We didn't even know what the blood counts and differential counts looked like for normal, and so it took some doing to develop that area. But, if we need to produce primates in captivity now that are clean, we know how to do it because of what we learned from the Program.

And likewise, I think that in the tissue culture area, many of the tissue culture cell lines were contaminated and mislabeled, and so some of that got straightened out in the Program. I think we did not know that some of them had multiple viruses present in some of the preparations because we didn't have the tools to learn of that.

So, with hindsight, it's very easy to say, "Yes, but some of those virus preparations had mixtures of viruses," and of course the straightening out of the helper virus with Wally Rowe in Huebner's lab, and others, certainly led to the kind of quality control that this program was insisting on.

Well, I'm sort of getting ahead of some of the questions, but I suppose that's fine. So I would say that one of the real contributions of this program was the development of resources that were not available beforehand and in sufficient quantities that they could be supplied broadly to the scientific community and people were utilizing the same *standardized* materials. These often were not as easy to come by as some people might imagine, or don't even think about.

Stevenson: I think one of the criticisms that I've heard from time to time about the program was that a lot of the resources had been generated and were utilized mainly by the in-house NCI research staff, and yet, to the best of my knowledge at least, in the entire time I was there, the resources that were created were advertised and made available on a quite reasonable basis to any investigator who could demonstrate a need for having them. And it was a collegial experience with a great deal of sharing, not only of the physical reagents themselves, but also in people very generously teaching other people how to use them effectively.

Baker: Well, I think another great contribution was this collegiality which resulted from a series of meetings which were organized by the Program, first at Airlie House, and later at Hershey, Pennsylvania. Many people I have talked to have said that this exchange of information before publication and on an informal basis, and everybody in the spirit of sharing their information early, was one of the great contributions of this program. I think even we, at the time, probably under-estimated how valuable that was. As people talk about the program they frequently point out that that was one of the great contributions. And I think Bob Gallo learned of the value of this kind of meeting and has continued that sort of thing annually in the area of AIDS and the areas he's working on. So, that was another related element.

So, when one plans out programs where one is attempting to integrate the various disciplines and resource activities with the science, it becomes clear what decisions need to be made, such as how often should one establish such a meeting and who should be the persons who organize it and who should be the main speakers, and it takes some kind of organized activity to do this. One of the main purposes, then, of the planned program was to integrate all these various components into one overall program that had ultimately certain goals that were spelled out. I think we all got sort of fooled a bit in the sense that we had a lot of animal viruses which induced tumors, and yet, in the human we were having such negative results.

When I left NCI, in 1972, there were over 200 viruses that had been shown to cause cancer in some animals, yet, at that time, we still hadn't--except for perhaps the Burkitt's lymphoma--established any human cancer virus. And I always wondered why that was so. Basically, I think it's because of the inbreeding of strains of animals and setting up experiments in such a way--manipulation, if you will--that led to the isolation of these viruses in animals, and you didn't have that inbreeding and manipulation in the human species so you got entirely different results. This, of course, gives added credence to Huebner's work on trapping the wild animals and looking at what viruses did in the natural habitat outside the laboratory.

Now, you mentioned that some people were concerned that too many resources went behind the individual scientists in-house, and certainly Huebner was accused of that. It was interesting, in Janet Hartley's interview, she felt that Huebner was not at all selfish; he was doing these things and talking a lot of people into working together and sharing information and that it was not for his own personal aggrandizement and he didn't even think that way. So I think Huebner has been tarred with a brush unfairly on this sort of thing.

Stevenson: You know, people look upon him as a 300 pound gorilla in terms of his scientific output and stature in the field, but I had dealings with him long before

I came to NCI and I remember he was working on the *Adenoviruses*, and I was in Cincinnati and we needed *Adenovirus* Type 3, to investigate chlorination in swimming pools, and he sent that virus--you know, he was right in the midst of publishing on it and had far from exhausted the possibilities of research in it--but there was no question and we got a large quantity of it immediately, which we used in water disinfection studies.

And later, when I dealt with him on a programmatic basis at NCI, I never found him anything but helpful and more than willing to help people out, give them advice, or actually do tests for them and quality control of materials.

He was also, I think, one of the smartest scientists that I ever met. He had the ability to take raw data that would be described at a meeting and think on his feet and integrate, synthesize, and come up with meaningful relationships. Just awesome, his ability to cerebrate.

Baker: He was accused, a couple times, of stealing peoples' ideas, and I had occasion to look into those charges and I found no basis for that. He usually had ideas well ahead of most other people and he was not stealing ideas; he was usually way ahead.

Stevenson: Of course, one of the problems was that he had such resources at his command, with a large laboratory of colleagues, very intelligent and able people, that he could take an idea and investigate it very quickly.

Baker: Well, the seventh question deals with the relative funding of grants and contracts and, while I'll need to look up exact figures, my impression was that during the period, say through 1972, from '64, the amount of funding in contracts was greater than the amount of funding for corresponding Viruses Cancer grants but, after that time, the funding for the contract area leveled off and there was a steady increase in grants. So, after I left in '72, I'm not sure how big the grants monies got, but I think they were increasing and I know the contract funds were leveled off and then subsequently reduced. So, we will look at the exact figures eventually here.

The question of, "If there is anything you could have changed as you look back, what would it be?" Norman Anderson indicated that perhaps there were some people we should have--as he put it--"stroked" more carefully. Maybe our public relations, in a general sense, could have been improved upon.

I think we could have solved some of that problem about grants versus contracts--I don't know why it's always "versus," but it seems to be the way it's stated--if we'd have had a little more time to hold more meetings where the scientists could provide more input. It's hard to make results of these meetings evident to people who were not there. The meetings we had in late 1971 and early 1972, were related to developing plans for the new Cancer Program that we were sure was going to pass and was indeed signed into law December, 1971. One of the elements of that activity was an overall plan, and so we started down that road in late 1971, and we had meetings at Airlie House where about 200 scientists were invited to participate and provide inputs of high priority projects that they thought were important in their own fields. These planning sessions were organized around major goals and then, under each goal were several approaches of how one might generally approach the goals, and under each approach were a series of projects that would be done to carry out the approaches. I had trouble communicating this concept to many of the scientists there. They had no trouble understanding the overall broad goals, and they had no trouble understanding projects, but approaches was a level in the hierarchy where I had great difficulty in getting across what I was after. And it was mainly a problem that scientists, who were used to thinking in terms of research projects, had trouble looking at this as a hierarchy in which something between the overall, broad, almost "motherhood" type goals and the project--that you ought to organize your concepts and your priorities and your funding somehow between those two levels in the hierarchy--and that's what I was calling approaches. It was difficult, even when I met with just the chairmen of the various sessions, getting across what "approaches" meant.

Stevenson: Did you get the impression that people felt that if they revealed sort of like project-oriented approaches, or whatever, that these ideas which they came up with would be out there for everyone to see and steal and run off to their own lab and work on and that they would be denied the concept of authorship?

Baker: No. I didn't run into much of that. I thought almost all of these scientists were very cooperative, very helpful, very open. It was a tough assignment to just sit there for five days and think about their field and what was needed and what could be possible and where there were gaps. No. I felt that they did that very well.

Now, my impression when I proposed this kind of meeting was that about a third of the scientists were opposed to this whole idea of planning and they didn't want to have anything to do with it. They thought it was for the birds. About a third of them said, "Well, we'll go along with this. Let's see where it goes. So, we'll participate and see how it comes out." And another third said, "Well, it's about time you did this sort of thing. Why haven't you been doing this all along?" And so that was an interesting experience.

But I think, if we could have continued meetings of that kind, we would have continued to build an *esprit de corps* in this whole area and continued the collegiality of the sharing of information which, I think, was founded in the Special Viruses Cancer Program. But I was no longer there to continue this course, and it just didn't go that way because after a bit of time, most academic scientists who would rather see the money that was being spent for contracts spent in the grants area put pressure on the President's Panel and on the Board to reduce funding of contracts on the assumption that if you cut funding in contracts the money would go into grants. History has shown this not to be the case. When something is cut out of Federal programs they don't necessarily find that money someplace else.

Stevenson: One thing that's always amazed me about the biological science community is that they will fight for a piece of the pie tooth and fang and toenail and everything else, but they don't put their shoulders together to try to make the pie bigger.

Baker: Somebody said that the physicists, when they are attacked, draw their wagons around and fire out at the opponents, but that the biomedical scientists and biologists put them in a circle and fire at each other.

Stevenson: There is a lot of truth to that.

Baker: Well, I've already touched on the idea that these developments have helped in laying foundations for biotechnology. I think it's also true that they helped further molecular biology *per se*. Many of the techniques that were developed out of the Programs certainly led to molecular biology developments. And that period where the oncogenes became worked out, certainly that is a transition from the philosophy of infectious disease virology being transformed into a molecular biology concept of genetic information being the key to cell functions in disease as well as normal. So, I believe that the Programs made great contributions in that direction.

The tenth question is a broader one that is not just related to Viruses Cancer, but relates to science in general, and that is a question of whether the public is more knowledgeable, or less knowledgeable, today than they were in 1960, and whether the public is more sympathetic, less sympathetic, or the same, compared to 1960 and today.

I believe there is a very disturbing illiteracy and lack of knowledge of science in the general population and I think proportion-wise this knowledge is probably less than it was in 1960, although the availability of scientific information for lay consumption is greatly improved. For example, *The Washington Post*, on Mondays, has a Science Section that's really quite good. *The New York Times* has an excellent program. So, if a layman wishes to obtain information, I think more is available now, even written for laymen, than was true in 1960.

On the other hand, I don't think as many people, proportion-wise, are availing themselves of that. So I'm very disturbed about that ignorance and I think, in the long run, that may have its effect on the support for science because, without understanding, it's not easy to expect people to provide funds if they don't understand what it's about. I believe that the Science Departments in the universities and colleges have to bear some share of the blame here because it seems to me most of these departments are very good at providing education and training for those who are going into science, but have pretty much ignored how you teach science to those who are not interested in going into science, and then they wonder why the public is not more sympathetic to them.

That's one reason I was teaching at Maryland. I taught General Science to those who were not going into science. I think we're deficient on that and I hope we'll see some improvements in that.

So that covers the questions. I think Dr. Moloney has discussed what he called the "demise" of the program following the Zinder Committee. I think it's more complicated than the Zinder Committee because I think a change of management at NCI led to less--shall we say--imaginative programming.

Stevenson: Yes. There was a period of time in there which I have no personal knowledge about, and neither do you, where things obviously got into a bunch of difficulties and where the perceptions were that the program had run amuck and that there was no internal control sufficient to keep the output for the dollar in balance. I'm not, you know, in a position to speak to any of that.

The thing that, I think is perhaps arcane for both of us is some of the politics that went on at different levels. I read Strickland's book on *Science, Politics and Dread Disease*, and a lot of the maneuvering and manipulation that went on at the Mary Lasker-Congressional level and the players involved in some of that certainly had an influence, not only on the amount of funding that the various Cancer programs received, but also the perceptions at the Congressional level of how well that money was being spent. One of the things that came about when Nixon declared the "War on Cancer," was that I think the expectations of a "cure for cancer," a vaccine for leukemia, or something like that, were overblown. I think they expected, perhaps, too much, too soon. I think it was partially the scientists that were involved, in terms of over-interpreting how fast some of these scientific results could be applied to practical problems and so forth, and I think it's again a case of misunderstandings between a group of people talking one type of language and another group talking a parallel, but arcane, language to each other.

Baker: I don't think very many scientists did that. I felt that was more what I call "science politicians." I was very careful in all of my testimony before Congress and my advocacy for the strong Viruses Cancer Program. I never slipped into making a prediction on what the results would be. I think DeVita made a mistake by saying that by the year 2000 we're going to have the mortality cut by such-and-such amount. All I did was try to get across that here are real opportunities for further work. Work is going well in this area. Here are some additional things that can be done. But I never promised results, because I don't know how to tell that.

But some people did. In the Mary Lasker effort to develop the new National Cancer Act, I think they left impressions that a layman would reasonably conclude that we're going to have something really good in terms of a cure and prevention much sooner than most scientists thought.

Stevenson: The paradigm up to that point had been the Polio Program and the development of a successful vaccine after a lot of money had been spent on research and development, and I think people were expecting some syllogism, like polio is caused by a virus and we have a vaccine that's effective for that, both killed and live vaccine, and, okay, now the scientists tell us that cancer is caused by a virus, it should be only a matter of time and enough money spent that we'll have a comparable vaccine for cancer.

Baker: Certainly, if we can put a man on the Moon, we ought to be able to--

Stevenson: That's right.

Baker: But why isn't that so?

Stevenson: Well, because when you get into the biology of the cancer viruses and so forth you find that this is not an infectious disease type model. And Huebner found it early on with the *Polyomaviruses* and so forth. These mice were not, in fact, infecting each other and so forth; it was coming down vertically transmitted. So, you know, in the early days of the program there was enough knowledge from the epidemiology that should have alerted people to the fact that hey, you've got a different kind of critter here.

Baker: Yes. Bob Miller wrote a paper on that as early as 1961 and showed that there was no evidence from epidemiology that this was horizontally transmitted.

Stevenson: Right.

Baker: We should have paid more attention to that paper, I think, than we did. I remember it bothered me. I said, "How do I relate that to all this stuff we're doing in mice?" It didn't fit.

Stevenson: Hindsight is great.

Baker: But I didn't give that enough weight. I should have given that more weight. Well, I think the basic answer to the question of why aren't we further along is the complexity--to use one word--of living organisms.

Stevenson: Well, the one thing, I think, that has come out, if nothing else, is the fact that even though the story of HIV is a discouraging one from the standpoint of practical efforts to control it, the complexity of the cancer viruses, the retroviruses, did set the stage for a very rapid understanding of the complexities of HIV and the fact that easy answers were not obtainable. Still I see a tendency though on the part of the public in general to feel that if you throw enough money at something you'll solve it. And I think the current AIDS research is being supported to the hilt in terms of useful ideas that can be explored. I don't see that doubling the expenditure of money is going to shorten the time that we have answers to AIDS by half. It just doesn't work that way and that calculus doesn't attain. But certainly, I think, without the basis of the Virus Cancer Program, I think we would have been wandering around the wilderness for several more years before we finally figured out what was causing AIDS and doing anything in terms of studying it. So I think that was a useful investment of time and money.

Baker: Well, with respect to the question of whether something like the NASA Program of putting a man on the Moon could be done in the cancer field, I was trying to derive certain managerial techniques and certain abilities of the organizational arrangements from NASA, and information flows, and I liked the *esprit de corps* I saw among those working on the Apollo Program. And so, even though I don't think we're in a position to say just because we got a man on the Moon that if we followed that same kind of program we would have the cancer problem solved, I do think there were managerial techniques that could be learned from the NASA Program which still ought to be applied in biology and medicine which have not really been tested yet, because we never really implemented the planning that was done under the beginning of the National Cancer Act of 1971, and so I don't think we've tested yet whether, in biology and medicine, an integrated planned programming beyond what we'd done in the Viruses Cancer area has been demonstrated. It never had a chance to be implemented, so we don't really know whether it's better or not.

Stevenson: Well, I don't know. I call this program a "Camelot" type of situation. You know? Thinking back to that time with Kennedy, it was a whole bunch of things. The whole environment and the whole mindset at that time, I think, was different than it is today.

Baker: Oh, I don't expect this to be implemented any time soon.

Stevenson: I must say that looking back as my own life has spanned a large period during which, when I went to college embryology and things like that were in the dark ages and people were talking about organizers on the dorsal lip of the blastopore, and a whole bunch of things, and there were phenomena that people could induce but not explain, et cetera. You know, in the past 50 years we have learned more about the basic parts of how things work in biology, we've learned more in 50 years, than were learned in the previous 5,000.

Baker: Actually, the last 30 years.

Stevenson: Yes. And it's been accumulating momentum as more and more tools, techniques and insights have been found. So, it's hard to predict what will happen in the next 30 years, but certainly I think this was one of the things that laid one hell of a strong foundation for moving ahead, just in terms of the resources that were created.

Baker: It's amazing the pace at which information is being collected now. In fact, we may be in a problem in biology and medicine like some of the space people are in now. The amount of data flow is so voluminous you have trouble keeping up with analyzing it. We're not quite there yet, but with the genetic code, you get a tremendous amount of information flowing down. I used to count radioactive samples one at a time when I took Biochemistry at Berkeley. Now, you know, you put samples in a machine and you not only get hundreds of them counted overnight, but the data are printed out and the graphs are plotted for you automatically.

In embryology, in particular now, the flow of information there now in getting at how this wonderful sequence of events takes place in development of the embryo, we're really getting insight into that. And now with the various growth factors and differentiation factors and all this signaling across the cell membranes and into the nucleus and the cytoplasm, repressor genes, etc., it's amazing the rate at which we're gaining understanding.

Wouldn't it be fun to be young enough again to be starting out in this?

Stevenson: Yes. But, on the other hand, I'm grateful for having had the opportunity to see so much in the course of my lifetime, from crystal sets to supercomputers, and the whole variety of things. I don't think I'd want to do it over again.

Baker: Well, we've been very lucky. We had a great time. We've met some wonderful people in our careers.

Stevenson: I remember things that people put on blackboards, and Lou Carrese had up, "Where is the knowledge lost in information?"

Baker: Do you know where that came from?

Stevenson: No. I don't.

Baker: I told Lou about it. It's from T.S. Eliot.

Stevenson: Is that from T.S. Eliot?

Baker: Yes.

Stevenson: I didn't know that. No, that has always stuck with me. And when I took freshman Chemistry in college there was a thing above the podium. The periodic table was on one side and this was on the right, and it said, "The English language is the most important scientific tool you have at your disposal. Learn to use it with precision."

Baker: Ah, yes. Well, I guess we're getting into philosophy.

(Whereupon, the interview concludes).