Baker, Carl G. 1996

Dr. Carl G. Baker Oral History 1996 B

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National Cancer Institute Oral History Project
Interview with Carl G. Baker, M.D. conducted on November 20, 1996, by Gretchen A. Case at the offices of History Associates Incorporated, Rockville, Maryland

[Editor's Note: *This interview with Dr. Baker focused on discussion of a brief biography written by Ms. Case, rather than a structured format of questions regarding his involvement with the NCI, as in other interviews. It has been transcribed and included because in the course of discussing this biography, he more generally discussed his involvement in the NCI.*]

CB: First, the draft that you had, I went over quickly. The first point you were raising in your questions—I was a Jane Coffin Childs Fellow at that time—

GC: Okay. All I had was Childs Fellow, and I didn't know what that was.

CB: Well, this is of course a private foundation. It's administered by Yale faculty. It goes back many years. The Childs family set up the funds to provide fellowships for young people in the biomedical field. So that supported me while I was at Berkeley.

GC: Okay. And that's not a Public Health Service fellowship?

CB: No. I was at Berkeley on that fellowship. I had a Special Fellowship at NCI the first year I was at NCI (1949).

GC: Oh, you did? Did it have a name or it just was—

CB: It's a National Cancer Institute Special Fellowship. That was in 1949. Actually, the reason for that is I had a delay in joining the Public Health Service.

GC: Why was that?

CB: I had an unusual looking white blood cell count and I'd been working with radioactive materials at Berkeley, and in those days everybody was worried about what radiation from isotopes might do, and so I had a delay in getting into the Public Health Service—

GC: Until they were sure—

CB: We got it all straightened out, yes. So I had seven years in the Navy, and twenty-three at NIH. Actually I came to NIH expecting to stay a couple of years and go to a university somewhere, and stayed twenty-three and had a great time.

GC: What is an Associate Director for Program?

CB: Associate Director for Program is an interesting title. It's hard to tell what that means. What does an Associate Director for Program do?

GC: Right.

CB: Well, I even had trouble at first, but what it basically meant was whatever Dr. Endicott wanted it to mean. But it did entail analysis and planning of the programs of the whole NCI. So that got me into every area of the NCI, and I spent a lot of time editing documents going to the Congress, and special reports and comments on other proposal and program-planning-budgeting-system (PPBS)—do you know about that one?

GC: No.

CB: Well, I don't know if you want to go into that. But McNamara who was Secretary of Defense liked this program-planning-budgeting-system, which is a systems kind of analysis, and convinced President Johnson that all the agencies should follow what Defense had done, utilizing this system. Cost benefits analysis is a subcategory of that in which you try to lay out plans and you try to indicate how much they'll cost, and then you try to calculate the benefits. And obviously if the cost/benefits ratio is favorable, it looks good. The trouble with that is there are so many important things you can't quantify like that. So this implementation didn't get very far because I think everybody felt that so many important things couldn't be quantified quite that way.

For example, I represented NIH in the Department of Health, Education, and Welfare effort on program-planning-budgeting, and I gave breast cancer as an example of why you couldn't depend too heavily on this approach. I said, "I know a way to guarantee we have a good cost/benefits ratio. You take off the breasts of all the women before they get to the age of 21—that would certainly give you a good cost/benefits ratio, but I don't think you want to do that."

GC: No.

CB: And the guy who's head of the HEW program says, "You're not very helpful!"But even though I'm very much in favor of systems analysis and planning, you have to be careful trying to put numbers on things when there's really no way to tell what they are. So in research in particular then, for NIH, we didn't see that we could put in the benefits numbers—we could figure costs fairly well, but how do you figure results as benefits in our basic research programs? The timeframe is too long. So, anyway, that was another interesting sidelight. I don't know that we need to mention it.

GC: Well, it's good to know about.

CB: Your second paragraph—there's confusion, not only here but many places, on the rank titles in the Public Health Service versus the position titles.

GC: Okay.

CB: So Senior Assistant Surgeon is a Public Health Service, uniformed services rank. It's not related to the job position title. I came in as a PHS Senior Assistant Surgeon, and I went through the ranks and retired as an Assistant Surgeon General, which is the equivalent of a Rear Admiral rank in the Navy.

GC: Very good.

CB: But the positions I held then had entirely different titles.

GC: Okay. So I've probably confused them a bit in here then.

CB: I didn't serve as Senior Assistant Surgeon to the Medical Director—I don't know which medical director you had in mind there. I was actually in the Laboratory of Biochemistry or the Grants area of NCI during this period.

GC: From '49 to '55?

CB: Well, it's not that simple, but in that period. For example, I started out in '49 in the Lab of Biochemistry as an investigator, and I got asthma from laboratory animals after a while—

GC: Oh, did you?

CB: I couldn't breathe; so I left the lab and went to help administer the grants program at NCI with Dr. Ralph Meader. His title was Chief of the Grants and Fellowships Branch. I may be getting too detailed for you. Let me know anytime, and we'll cut this out.

GC: Well, it's nice to have more than I need.

CB: When the Clinical Center opened in '53, I went back to the lab expecting not to be bothered by the animals in the Clinical Center, but the wings of the building were full of animals, and I had almost as much trouble breathing there as I did in Building 6; so I gave up on staying in the lab even though that is what I would have preferred to have done. I was doing reasonably good basic research in the lab, but I only did that for a short time. I was all set to go back to the grants area at NCI when I was offered the position of Assistant to the Deputy Director for Intramural Research, NIH level.

GC: Do you remember what year that was?

CB: From '56 to '57—probably lapped over into '58. In '58, I came back to NCI then as Assistant Director. I didn't like that wording in your document [here he refers to the biography, which describes his lectureship at Georgetown University Medical School] because in those days you couldn't really have outside work; now you can. So this was all *gratis*, of course, and I only lectured a little bit, so I don't want it to sound like this took away my primary responsibilities at NIH.

GC: Okay. So it wasn't like a second job at all. It was just you gave lectures on occasion at Georgetown.

CB: Yes. So maybe just serving as special lecturer would be good enough.

GC: Okay.

CB: I think you ought to mention that the Cancer Control program was brought back into NCI with the signing of the New Cancer Act in '71.

GC: Someone at NCI mentioned to me that that was a very important topic to cover.

CB: It was a big change.

GC: Can you tell me a little bit about what the change was?

CB: Cancer control, which is not easily defined, basically is a bridge between the results coming out of the research on their way to application. So it's really a different world than the basic research area because you're trying to apply stuff in populations. For example, mammography was one of the early successes of cancer control. Mammography, picking up breast cancer earlier in women, has led to some improvement in the therapeutic results. So the control programs then are really trying to apply what we've learned on the research side. There are still arguments on whether the NIH is primarily a research organization or a research and application organization, and there's really been quite a shift. Early on, when Dr. Shannon was Director of NIH, he moved those control programs that earlier were at NIH, such as those of the Mental Health and Cancer Institute's to another part of the Public Health Service. So that was probably back in 1967, somewhere in there. Dr. Shannon felt that these were not good research programs and that NIH was a research organization, and other parts of the Public Health Service should have those responsibilities for implementing the Control programs. And there's still a lot of argument on whether this is the best role of NIH or not. I happen to think it would be preferable if we were only to do research and did not have the Cancer Control. So I did not favor Cancer Control bring brought back in the NCI. But nobody asked my view on that.

CB: On the other hand, Mary Lasker, who I suppose you've heard of—

GC: Oh, yes.

CB: And some of her colleagues were very strongly in favor of having the Cancer Institute and also the NIH as not only a research organization, but responsible for implementing the best in terms of application. And also on the congressional committee in the House, there was an M.D. who I think was instrumental in putting the control programs back into NCI, although I don't know that for a fact.

So this changed the whole pattern of activities from essentially a research organization to research and application. And this changes the way you think about the problems, it changes especially the clientele that you deal with from the NCI, because the application people often are not research-oriented, and so you get a whole different philosophy and you have a whole different set of clientele when the Control program was brought back to NCI. About two years before that, the Control program in another part of PHS was essentially defunct anyway. So these people who were interested in cancer control obviously lobbied for getting this in the new Cancer Act to bring it back to NCI, reinvigorate it, and believing that NIH, rightly, was a strong organization that once they had responsibility, would carry it out.

Another change was that the former National Cancer Advisory Council—I get the word order wrong of that, I think—

GC: I think it's National Advisory Cancer Council, NACC.

CB: Yes. There were twelve members on that council, and the new Act made a twenty-one-member board. Mary Lasker intended that to be a Board of Directors that were in charge while the old Council was advisory. The Chairman of the Council meetings was the Director of the NCI, acting for the Surgeon General, while the Chairman of the Board is not a member of the staff. That to me made a significant change.

GC: Yes, that's very significant.

CB: Not only have you got twenty-one members, you've got some more *ex-officio* members and many more observers, and the meetings now remind me of a circus. I went to a meeting this past year. There were over 200 people in the room. I happen to think that's not the way to run an organization, but that seems to be the way it's done these days.

GC: It's still done that way then?

CB: With all these people, oh yes. We used to have a few . . .

GC: When it was NACC it was just the members of the Council?

CB: Well, no, we had a few, but nothing like the—see, you've added all these special interest groups as clientele now, the women pushing for women's programs on breast cancer, and all kinds of groups like that are now attending the meetings in the periphery; they're not sitting on the Board, but there's a lot of pressure for having presentations that relate to these various groups. So you've got pressure groups galore here now, which we didn't have very many of; we had some.

GC: Right.

CB: So this I think is a significant change; whether it's good or bad depends on your point of view. It's not necessarily good or bad, it's just different. I think it's gotten overelaborate, and I don't really favor running things with huge committees. The American Cancer Society—I was on their Board of Directors for a while. They were interesting because they must have had about 200 people or so on their Board of Directors, and their Saturday morning meetings were fascinating to watch.

GC: I'll bet.

CB: Well, the only way you can run something like that, of course, is to have a small Executive Committee. Then the Executive Committee kept bringing things up for discussion and voting on them. The game was: don't discuss it, vote! And so there was some fellow from Brooklyn who got up and said, "No, I want to discuss that!" and everybody looked at him like he was out of his mind!

CB: Because apparently it was not to be done that way! So that was an interesting experience, too.

GC: I'll bet.

CB: The other changes that happened after the National Cancer Act was passed: at first there was great interest in planning and in fact the new Cancer Act called for more planning and analysis, planning in particular. But we started that planning before the Act was signed; this goes back well before that. The planning sort of died a quiet death, so that after about two years, there was really hardly any interest in this kind of planning. I think what happened is that a lot of good scientists, who as I say are not in favor of planning, talked Benno Schmidt, who was head of the President's panel, into stopping putting money into contracts but into putting more in grants. That of course led to lack of planning, because in grants you don't approach it that way. Each individual investigator submits his ideas of what he wants to do, and a group in his specialty will review that for quality, which is good. For exploratory research, I don't know a better way to do it than the grant system. But I didn't think it was appropriate to do everything that way. And that leads to what I mentioned before, establishing the Organ/Site programs.

When I became Director, we had only nine grants totaling $212,000 on large bowel cancer. Well, that seemed rather strange because if you add male/female statistics together, that's the leading category of cancer incidence. And yet we only had this sort of piddly amount of research on it. Well, what was happening? The way the study sections were set up, there were people who were not knowledgeable about large bowel cancer and weren't interested in it, so what few applications came in were usually turned down or given very poor scores.

So I asked, "Why such a small amount of work on this important area?" "Well, nobody has any ideas on what to do." I went in the library about three nights. I found we had a good animal model system. You give me a model system, I can build a whole program around it.

So I established then three groups consisting of people who were interested in and knowledgeable of cancers of large bowel, prostate, and urinary bladder (and we were starting on pancreas), and let the word out that NCI was interested in more work in these areas. So we began to get applications coming in, and they were approved by these groups. And so we, I think, had some good programs.

I expected the Division of Research Grants to object to our setting up our own review groups, but they didn't. I think they didn't want to fool with it because it was multi-discipline, and most of their study sections are discipline-oriented, i.e., scientific disciplines.

Both approaches are good. I don't think we ought to be either/or. In fact, one of the distressing things still is that people keep discussing this as either/or. Grants are the more popular.

So this is very related to the issue of whether you fund with contracts or grants. And that's another ongoing battle, with debating all the time, and controversial, so that after this planning effort dropped, the amount of money and contracts dropped, and a lot of that money ended up in grants. Some of it didn't end up anywhere.

I was in favor of more planned programming, which generally was funded more in contracts because if you have a planned program, the participants can't just go their own merry way if you're going to follow the plan. That's why a lot of them don't like it of course. But if you're looking at problems in cancer that involve various disciplines and, what we also showed in the Viruses Cancer area, the need for resources, then some integrating controls with contracts are essential. These resources included special animals, tissue cultures, cell lines, virus preparations, antibody preparations, things that you can now buy with kits. But in those days they were not there. So we spent a lot of money in the areas with contracts developing resources. And there again, the grants system could provide resources, but generally didn't.

I think this whole argument about contracts and grants is unfortunate. And as the program got bigger—politics entered more heavily the bigger the budget. So my impression is that we have more political interference now on scientific issues than before. My favorite pet peeve is the establishment of the Office of Alternative Medicine, which I think has got a lot of crackpot stuff in it. But that was after I left. That was set up because one congressman wanted it set up. His wife had been treated with one of these methods not receiving NIH funds. Well, that's not part of this.

GC: Can we talk about this program you set up? You called it the Organ/Site Program? Is that what you were saying?

CB: Yes.

GC: Was that during your term as Director or was that before—

CB: I started it before I left, yes.

GC: While you were Director.

CB: Even though people seem to think it was part of the National Advisory Cancer Act, the National Cancer Program.

GC: But you had started it before.

CB: Yes. That's one of the things that I think it was a good thing I did. [Laughter] So I don't mind having it mentioned.

GC: No, I'll be sure to mention it.

CB: [Dr. Vincent T.] DeVita cut those programs out, but then the next Director put them back together. I don't know quite why DeVita closed them out. I never talked to him about that. He may have had a good reason. Sometimes if things aren't running very well, you cut them out sometimes. So I don't know why he did, but anyway, he did. But they've been reestablished. In fact, the planning and that, I think, are the two best things I did while I was Director. Now, let me go back on the planning a little more because not only was I pushing planning, there was a lot of attention paid in the development of the Cancer Act that we should have more planning. And the Congress rightly expects it. And it's amazing how much NIH has gotten away without planning.

GC: Oh, really?

CB: And this development was our first examples of this kind of planning. And if you're interested in the philosophy of planning, there's a lot of that in here, too.

GC: This is your paper with Louis Carrese?

CB: Yes. When I was Associate Director for Program, I had these responsibilities for analyzing and planning new ventures; so how do you do that? I've always been interested in reading broadly to find out anything that would help us with cancer. And it seemed to me that this kind of planning which NASA [National Aviation and Space Administration] uses very well—maybe we can draw on some of the experiences of those people who planned very sizable, multi-disciplined programs.

I read a lot on systems planning, including having an opportunity to review the original NASA plan and then the other books on systems planning. A remarkable book is *Space Age Management* by James Webb, former head of NASA. So it seemed to me that we could benefit from what was learned. On the other hand, most of these systems planning efforts were done for production of missiles or the Manhattan Project and that kind of thing, the space program. And you knew pretty much that the task was accomplishable. All you had to do was put resources behind it and run it and move toward the objectives. Well, in research it's not that simple because you don't know even if the thing is going to succeed, much less when. So we had to modify the systems planning techniques to fit the research program. Well, that's what this paper is about.

The first area we tried this on was the Viruses-Cancer area. Well, this was presented before the Cancer Council, and everything was blessed, and they thought it was a good idea.

And then chemotherapy was the next one we did, and it lends itself to this a little easier because the program proceeds in a nice, linear flow, one step to the next. But what we did was specify goals and objectives, and then key steps in here on the key decisions that have to be made. We had decisio n points where we've defined what the decision is, and just as important, what kinds of data must you collect to be able to make the decision? We spelled out what kinds of data you have to have to make the decision. And this hadn't been done before. So committees would argue for hours whether a drug should be passed from this stage to this stage, without any data.

Four of us spent three weeks away from telephones to lay all this out. And we added the monitoring points, too, which periodically you consider where you need to change what you laid out or not. So a monitoring point is making decisions on whether you need to change as a whole program or not, while decision points are important for moving drugs from one step to the next.

This works very well. NCI staff still use this program plan as the basis of their program efforts. They've added decision trees to it, which is another refinement. I think this has been quite successful in the chemotherapy area. But in the Viruses-Cancer program, they didn't use it much.

GC: It just didn't lend itself to it or they just didn't follow it?

CB: Well, as I say, there was a lot of opposition to the whole idea of planning, so one easy way to get around that is to just ignore it. However, that was only about a third of the research investigators. Another third said, well, let's try it and see. And another third said, it is about time you all did this planning.

CB: So I would say they were split about three ways here on whether this planning effort was the way to go or not. But, anyway, this earlier effort put us in a good position to talk about planning for the total program of the NCI. We ended up with over 200 scientists that we got together at Airlie House down in Warrenton, Virginia, to take a look at what we were proposing in the way of planned programs, to try to develop an *esprit de corps*, and to obtain their inputs. And this round chart that encapsulated the goals, objectives, and major program thrusts—if you cut this and lay it out, it's really a hierarchical set with this overall goal at the top of the hierarchy [Here Dr. Baker begins to refer to copies of two versions of the planning chart, one laid out in a circle, the "dart board," another laid out in a linear fashion.].

GC: Okay, so it's like—

CB: So Carrese said, "Why don't we curve this hierarchy around, and it makes a very nice target."

CB: Also we got some cracks about using the circular chart as a dart board or even in the mens room.

GC: What kind of target?

CB: Wet it down!

CB: By those who were opposed to this. But right before I left, my staff sneaked in a change of wording here, which wouldn't have happened if I'd stayed there, I think. I had the goals to actually cure and prevent cancer, but they changed it to "develop the means for" curing or preventing cancer, which is a very much weaker goal and objective. And one of the points discussed in the paper is that these objectives and goals should be in terms of ends /results, not some process. That one hanging on the wall in Building 31 has "to develop the means for" curing and preventing cancer. That's why I say that's not the original edition. So this was done in 1971 before the Act was signed, but a lot of people think that all this came out of the Cancer Act.

GC: But this was actually planning you had started before.

CB: It was well along before that. And we had already gotten together with the 30 Panel Chairman of these 200 scientists. We had divvied up the program plan, and I picked the Panel Chairman for each of the areas. If you look at the circular chart layer-wise, this is a broad general goal at the center, then more specific goals and objectives at the next ring, then approaches, which I had a hard time explaining—very good scientists had trouble in here, they had no trouble down here at the project level at the outside ring. They had no trouble up here at the goals and objectives or at the project level. But in between, they had trouble getting hold of that.

As I say, this is a hierarchical thing, it's hierarchical in terms of concepts, so that if you have all these projects, they are related under this next layer, and these in turn relate to here.

GC: Oh, I see.

CB: So you're divvying up the efforts all aimed at the goals at the center. These approaches are groupings of individual projects. And the reason I think they had such trouble is they never had the experience thinking about it at this level. They were all at home with the individual projects and most of the grant applications were written around projects, and this depiction is the lowest level in the hierarchy. And by "low" I don't mean poor, I mean location in the hierarchy.

GC: Right.

CB: The bottom of the hierarchy.

GC: Right.

CB: Just that—that's where the work gets done, really. So that's very important. But how do you group all of this conceptually (and also for budgeting)? This is a hierarchical thing which is all aiming at the center goals and objectives, and this is why this is called convergence technique; you're converging on goals and objectives.

GC: Oh, okay.

CB: I put together the conceptual content of the hierarchy. The curving the hierarchy around to a circle was Carrese's idea. This round thing I thought was a great way—it's a good way to present what you're trying to do. So we had panels then on each of these thirty approaches areas. We got quite a bit of input.

Let me say also that the people who were picked to attend this meeting, i.e., were invited to this meeting at Airlie House, were based on names submitted by several organizations like the American Cancer Society and some of the other scientific societies, clinical investigators and whatnot, and then actually picked in the final names from the lists submitted. And in those days I knew most of the people whose names were submitted.

One of the great things about being Director of the NCI is the information input is tremendous. You can learn a lot just because people want to tell you about it, often because they want money.

GC: Well, that's true.

CB: I didn't feel that I had any trouble keeping up with the main thrusts. And I've been a generalist for a long time, and so I didn't have any one favorite area. That was true conceptually as well as how I dealt with the staff. I didn't favor one area over any area. We need all these areas, and you have to worry about how you balance them out because you never have enough money to do everything you could do. You <u>should</u> have your plans well beyond your funding; so if Congress asks you what you would do with more, you're ready. We usually did get that question when we testified before Congress.

With respect to this idea of going to a billion dollars per year NCI budget, which was believed to be new, NCI had testified what we would do with a billion dollars two years before that idea got into the planning of the National Cancer Act. My argument was that the public is interested enough in cancer and concerned enough about it, fearful of it in fact and rightly so, that they would be willing to support a program at a billion-dollar level, and of course now it's $1,200,000,000.

I testified for $430 million budget for NCI right before I left. So one of the good things of the Cancer Act certainly was expanded funding. But that's leveled off more or less the last few years.

GC: The funding?

CB: But that's worked. As Dr. Shannon used to say, "We ought to be able to do something with $1,200,000,000."

GC: I would think so.

CB: In those days it was, "We ought to be able to do something with $1 million," which looked big in those days.

This planning experience I think is a very interesting one. The history of it is not too well told. That's one thing in my memoirs I picked to relate how I saw it. As you know, historians don't always see things the same way.

GC: That's true.

CB: What's that other joke about how history gets written by those who live longest?

GC: I've heard that one!

CB: I'm well aware that what I put down is how I see it. It may not be the way it really is, because the way it really is nobody knows, all these different perspectives. You do the best you can. So I'm not trying to sell anything. I'm interested though in trying to get the record out as close to reality as I can.

GC: Right, that you had started all this before the Act was in place.

CB: As a matter of fact, the journal *Cancer* is coming out in the December 1996 issue commemorating the 25th anniversary of the signing of the Act, and I was invited to a meeting to discuss what this should contain. We were invited to present papers before the meeting, so I figured my job was to point out that many of these things were started before the Cancer Act was signed, because I keep hearing as though history started with the signing of the Cancer Act, it's not so.

GC: That's right. That's the way it.

CB: It just continues.

GC: That's the way it's often portrayed in a lot of these, is that the—

CB: But Cancer Control, though, is a very dramatic shift.

GC: And that was directly from the Act, right?

CB: Oh, yes. As I say, I really didn't think it was a good idea to have it. But I could understand why people would want it included. And if I'd stayed, I would have certainly tried to support it because you're ordered by Congress, really.

GC: Right.

CB: And they had a right to do that. But I don't think an individual Congressman does, but it happens.

GC: Right.

CB: The funding for Cancer Control, when the Act was passed, we of course had not had time for a bigger budget to be developed under the new Cancer Act because of the budget cycle we were in, and so I started funding Cancer Control even though I had nothing in the budget for that. So I moved $3 million to a pocket to initiate the Cancer Control program. And then I got bawled out because I didn't put enough money into Cancer Control. I had to take it out of my hide to get it started. So I thought I was doing a pretty good job on something I didn't particularly want anyway.

GC: Right.

CB: To foot $3 million, and take it out of the other programs. It got them started, yes, so I still think that was about the right level to get them started.

Well, where are we? I'm trying to give you things that seem to be important to me, but I don't know whether that—all that doesn't need to get in here [the biography], of course.

GC: No, but I think—

CB: This will give you background.

GC: Right, and what I think I'm going to put in there is the major change during your administration.

CB: I should have brought a copy of that article I wrote for the journal *Cancer* listing the program accomplishments that were there before the Act started. It's a whole page of them.

GC: I would love to see that sometime. I think I have an idea now of the major changes and the planning and the Organ/Site program, where the things—

CB: Well, let me make one more area that I think was very important for laying groundwork, and this is the Viruses-Cancer area.

GC: Is this Virus as Cancer?

CB: No, we've hyphenated it. It's really the cancers initiated by viruses.

GC: So it's Viruses-hyphen-Cancer.

CB: Yes, which later got shifted to the genetic information rather than viruses, which is a fascinating story, but I'll touch on it. So of the stuff that came out of the previous programs before the Act, one of the most important areas was the Viruses-Cancer area. And NCI requested special appropriations of $10 million to expand the programs on viruses and cancer because we had a lot of evidence that viruses could cause cancers. In fact, when I left NCI, NCI had found and grantees and contractors had identified over 200 viruses that could cause cancers in certain animals. We still don't have more than two or three viruses that have been established to cause cancers in human beings. But in those days, we were finding that we could begin to understand how the viruses produced cancers in all these animals.

And, as I already mentioned, resources are very important there for moving ahead. So this program—a major effort was to produce the resources needed to move the field along. And this request for $10 million for that program allowed us to expand the program, and we had to put together a lot of information justifying why we wanted to go to Congress to ask for special appropriations. That isn't done very often.

So three or four of us put together a memorandum justifying this request. Dr. Shannon, with his usual emphasis on quality, wanted more information, and so we had to pull together some more information. And he said, "Okay, we'll go request this." And we requested it and then got the money, and that led to the planning of the viruses area. In fact, after the $10 million was appropriated, Ken Endicott came to Carrese and me, and said, "Okay, you guys have been talking about planning, give me a plan for a $10 million program in cancer virology." So with Rauscher, who became Director after I was Director, we took three weeks off and laid out the Viruses-Cancer planned program.

GC: So this was all under Dr. Endicott when he was Director.

CB: Yes, and so certainly he ought to get a lot of credit for pushing this idea, asking for special efforts and budget, pushing the idea of laying out plans, and so on. He was a wonderful person to work for, was fun, and had similes about people, names of people—his similes were wonderfully descriptive and some of the labels for people were delightful.

GC: Anything you can repeat?

CB: They didn't always know what label he had on them!

CB: So one day I went and asked him, "Ken, you've got all these labels for people, what's my label?" He says, "Oh, you're Uncle Carl."

CB: So I got off pretty easy. But he was a great fellow, a marvelous administrator, a sharp guy, but fun to work for, too—and tough when he had to be.

GC: Did you work for him under his full term, the whole time he was Director?

CB: Yes, when I was the Acting Scientific Director, and Associate Director for Program—in fact, he cooked that title up and that position. And then I was Scientific Director of Etiology, which was under him. And then he left to go to Building 1.

CB: On the Viruses-Cancer history project, we've interviewed over thirty people now on tape.

GC: Oh, really?

CB: People who were directly or indirectly involved in the Viruses-Cancer activities. So I've been spending a bit of time getting the transcripts edited and this sort of thing.

GC: That's very interesting because I'm trying to find out who—it seems like a lot of people are doing little history projects or have done history projects at the NCI, but I don't know what they all are, and I'm not sure any one person knows.

CB: Well, John Moloney who headed the Virus-Cancer program when Rauscher moved to Director, is writing up some stuff. So he wouldn't give me a signed clearance on the interview with him.

GC: Oh, really?

CB: We're trying to get all of this into the public domain, so I've been getting signatures for release. I just sent them out though on November 8th, and I've gotten five okays so far. So Bob Stevenson, who headed the Viruses Resources Development Program, and I are doing this together, so we need to get our text written now. We've got all the interviews down. Well, I've got a little labor revising—you know, I sent them out saying it didn't look like we needed to edit them anymore, but you know how people are, especially some people nitpicking the heck out of it.

I tried to maintain the conversational tone of the interview on the transcript. So, you know, there are incomplete sentences and so on. So some purists are really bothered by that, so they'll fill in.

GC: They'll fill that in, I know. I've seen that happen before. But what can you do?

CB: No surprise. So that's what I've been doing lately. Well, where are we?

GC: Well, how about—let's go back to this for a minute [the biography].

CB: See what questions you still have here.

GC: Consultant in private industry. I found that in one source, but there were no details. I didn't know if that was the correct way to term it.

CB: Well, when I left NIH I went to head Hazelton Labs as President and Scientific Director. But again I got asthma from lab animals, so I didn't stay very long—less than a year; well, about a year. And then I consulted, just private consulting for about another year. I had consulted for about seven or eight organizations, including TRW, JRB Associates, Hazelton Labs, Mitre Corporation, University of Kentucky, and the Roswell Park Memorial Institute for a while. I didn't see where that employment was headed, so I went back to work for Ken Endicott again in the Health Resources Administration (HRA).

GC: Oh! So you were working for Dr. Endicott again.

CB: Yes. He was such a good boss. But this area wasn't very gratifying because you could work very hard and you didn't feel like you had much effect—the politics just swept right over everything. This was in the application of health care. The medical field had a real shift. Earlier, doctors were curing people, treating people, treating injuries, and all of a sudden it was enlarged, and now we're supposed to have you have good health. And that changes the whole scope and outlook of the effort because you're switching from treating disease and injuries to, we're going to make sure the health of the nation is improved.

GC: Right.

CB: And so I kept asking when I was in Health Resources Administration, "How do you define whether you know whether you've improved the health or not?"

GC: How do you quantify it?

CB: It was all talked in these vague, general terms, "We're going to improve the health." How do you tell whether you've done it or not? And we didn't have any staff meetings, so I volunteered to develop the agendas and write the minutes. I had six years of writing minutes for the Executive Committee of the NCI. Unfortunately, those minutes are no longer in existence, and I'm sorry to say, because with my minutes, I write not only the decision, but what the issues were that led to that decision. That would have been a good history document.

GC: Oh, yes. And they're gone?

CB: When the Assistant Director, who also wrote the minutes after I did, was pushed into a small room and relieved of his job, he didn't have any room; so when he got moved, they got thrown away.

GC: Oh, no.

CB: I really think that's unfortunate.

GC: Yes, that's heartbreaking.

CB: Not because I wrote them, but because it was a good account of how the NCI made the key decisions over those years. For example, the mammography study, in a big population with controlled trials, we had to make a decision on whether to fund that, and there was a $3 million price tag on it in those days. You know, do you really want to make a decision for $3 million, when, as I say, in those days the budget was only about $300 million. So this got all presented and discussed at the Executive Committee of the NCI, and we decided to go ahead with it, which I think was a good decision.

But this kind of decision-making, as well as the day-to-day housekeeping kind of stuff, it was all laid out there for you.

GC: And that is often what is not recorded, what's behind the decision.

CB: Yes. I regret that. But you can't win them all, as they say.

GC: Right. That's too bad.

CB: Well, anyway, when I got an offer to be full-time Medical Director for the Ludwig Institute for Cancer Research, I quickly accepted because I wasn't too happy in this HRA area.

GC: And that's when you went over to Switzerland, right?

CB: Well, I was in this country for about a year, and they kept urging me to move to Switzerland where their home office was. I don't know whether you know anything about the Ludwig Institute or not.

GC: Not really.

CB: Well, before we get to that, is there anything else you want here on the—you know what etiology is.

GC: Yes, and I have a question for you. The title is always given as Director for Etiology.

CB: This was Scientific Director for Etiology.

GC: So it's not Etiology Division or anything like that. It's just "for Etiology," when you talk about that title?

CB: Yes. Whether it's a division or laboratory or branch—it kept shifting, especially as it got bigger and bigger, and so the titles are a little confusing I think on that score. Originally the lowest level was Section. For example, I was in a section in the Laboratory of Biochemistry when I came.

GC: Okay.

CB: And then Laboratory was also equivalent to Branch in those days. For example, Grants and Fellowships Branch was what Dr. Meader headed as Chief. So that a Branch organization was equivalent to the Laboratories of Biochemistry or Pathology or so on. But as things got bigger, you shifted most of the definitions and the size and the organizational position of these terms, and so it's really quite confusing.

GC: I just wasn't sure if I was leaving out a word.

CB: So actually, Etiology was a Division. Earlier, when things were smaller, the whole Cancer Institute was a Division.

GC: Right, okay.

CB: But then it got bigger, so the Division moved down one echelon step with a new title—I've forgotten what the organizational term for the NCI was at that point. Just National Cancer Institute. So that's a very tricky subject. Actually, Etiology was a Division when I headed it.

GC: Okay. All right.

CB: Now it's something different.

GC: You don't know what it is now.

CB: Incidentally, if you want an interesting experience, take a phone book if you can find one of NIH, say in 1970, and take one today and look at the titles and the number of people sitting on committees. The proliferation of titles and a number of internal (and external) committees have increased greater than the budget. Imagination and creativity have been shown in the wording of titles.

GC: I'll bet that is interesting.

CB: Tremendous growth.

GC: Yes. Oh, that place is huge now.

CB: Well, there are 14,000 employees, I think. And the budget for NIH now is $7- or $6.8-billion, or something like that and that is a lot of money.

GC: That's a lot of money.

CB: It doesn't go as far as it used to, though.

GC: It's still a lot of money.I just had a couple of questions in here [the biography].

GC: Okay. And, yes, you did serve on committees at the American Cancer Society.

CB: Oh, yes, lots of them. Also with the American Chemical Society. I think this wording would be better turned around because it sounds like I was Secretary of the American Chemical Society, but I wasn't.

GC: You were Secretary of the Division of Biological Chemistry?

CB: Yes, which was a long time ago. I had that job for three years, which meant I had to organize six national meetings for American Chemical Society, meetings which met in the spring and the fall. And nowadays they've got all kinds of committees to do all this. I did essentially all of it myself, but it was, of course, easier back then. But I would receive all the abstracts and arrange them into a program, and then I had to line up who was chair at each session. And we didn't have any money to pay for transportation, so you had to pick somebody you were pretty sure was going to go anyway.

CB: So that was interesting. Now, Dr. Greenstein, who was the Chief of the Lab of Biochemistry, got me into that position because he was elected Chairman of this Division, and a Secretary before had said he'd had enough. So Greenstein talked me into doing that, which was a very interesting experience. I probably shouldn't have been that young to have that experience. Usually that comes much later. But it didn't interfere with anything in particular. I still turned out stuff in the lab because most of that time I was still in the lab.

So that was, as I said, an interesting experience. I got to meet some of the outstanding biochemists, of course, this way.

GC: Right. I'll bet that was interesting. I guess the last little thing was, I didn't fill in what you're doing now. I don't know how you like to say what you're doing. Do you just say you're retired?

CB: Well, I am retired, but I'm writing. In fact, I have retired three times, so far.

GC: Right. So retired and working on your memoirs, or working on several histories?

CB: I don't want to call them that specifically. I'm still struggling with what the title ought to be.

GC: How about retired and working on several history projects?

CB: Yes, well that's—several?—two.

GC: Okay, working on history projects. I'll work it out.

CB: I was teaching before that.

GC: That's right. You taught at University of Maryland?

CB: Well, that and Columbia Union College, both. Now my wife, while she was still working at NIH, NCI, in the Contracts Review area—

GC: Oh, so she worked there, too.

CB: She was in the business end of things—had not finished her degree. So while she was working she went to school at night and got her degree at Columbia Union College. And the way I got into teaching at Columbia Union College was when I took her over to look into going to school, I got to talking to the dean, and he said, "Would you be interested in teaching?"

GC: Oh, really?

CB: And they needed somebody to teach Dynamics of Group and Organizational Behavior. And I said, "Well, I've sat on so many committees, I ought to know something about that."

CB: So I said, "Yes, I might be interested." So this was in the Sociology Department, which I'll come back to that in a moment.

GC: Okay.

CB: And the textbook was already picked, and so I had to read the textbook just to keep ahead of the class. But it worked out all right. It was interesting. These people came to school at night, after they'd worked all day. You had to keep them awake for three hours (seven to ten p.m.). But they were well motivated. They were really interested in learning things that would help them in their jobs, and the thing that they seemed to find most helpful was my outlining how you conduct staff meetings with the agendas out beforehand. With any accompanying information that was pertinent, and writing good minutes, but the agenda should be spelled out with time limits on each discussion, and the minutes should reflect your discussion, and the follow-up procedures had to be attended to, and this kind of thing. They thought that was good, but the rest of it they felt was a bunch of hokey, I think.

GC: Oh, really?

CB: And there's a trend in education now: group activities where the students are going to teach each other. Uh-huh.

GC: You're not big on that?

CB: Well, I think that's overdone, because when I got to teaching general science, I ran into that problem again. They said, "Well, you teach them how to think," in these group activities. In my view, particularly at the freshman level, you've got to have something to think with. So I taught my general science course in the old-fashioned way of giving them a lot of information to absorb. There was enough in there to teach them how to think if they really did the job, because I actually started with basic physics and took them on through to behavior, with oceanography, DNA, genetics, evolution and all in between.

GC: That's a lot of information.

CB: I may have covered too much, but they got through all right. Most of them did all right. I was pretty gentle on the tests. So that was at Maryland. Then I taught one session on science in society.

GC: That's interesting.

CB: This was in the Sociology Department there, and again, they had six books they had to read that were already picked before I took over the course. And I have trouble with some of these sociologists on their version of the way science works. I don't think science works the way they often describe it, "they" being several sociologists who have focused on writing the sociology of science. To me they were setting up straw men and knocking them down. It didn't seem to me that they had a very good hold on how science really works. This is another example of conflicting concepts from different areas.

The sociologists, of course, are looking at the interpersonal relationships and how people relate to each other, always looking for controversy I guess to liven things up or something. But fraud in science, you know, has gotten greatly overblown, I think. The sociologists are partly responsible for that.

GC: Oh, really?

CB: I think so. This is an opinion, of course. But we're nothing without opinions.

GC: That's true.

CB: I don't apologize for opinions.

CB: Anyway, that was interesting, too. I, of course, learned something from teaching. I knew that the students would have trouble with formulas.

GC: In the sociology class?

CB: No, in the science.

GC: In the science.

CB: Really all these formulas do, say in physics, like force equals mass times acceleration, F=MA, show relationships among (in this case) these three things. In my first lecture, I would try to give them some general idea of what science was about and important things like measurement. A lot of people never thought about measurement, scales, standards, much less classification and definitions.

And the thing that I think that science has that the other areas don't have (which relates to this fraud issue), is you can't for long fool people in science because the system will find you out. So with respect to these fraud cases, which are fairly rare still, it's stupid to try fraudulent activities in science because you're going to be found out because the whole system works for repeating the work—everything depends on confirmation of research finding or lack of confirmation.

GC: Right, because someone else has to independently confirm your results—right?—for it to be accepted.

CB: Yes. Scientists are no more or less honest than anybody else, but the system catches fraud and therefore less dishonesty is practiced in science than in other areas.

GC: That's true. I hadn't thought about it that way.

CB: So, anyway, I tried to get across some of these more general things, and I started out by saying that science is just one way of looking at the world, and then I listed the other areas, like religion, as another way to look at it, and the sociology area is another way, and the humanities is another way, and science. And one is not right or wrong, necessarily; they're just different ways of looking at the world.

GC: Yes, different ways of looking at relationships.

CB: Of course, I happen to think some of them are wrong.

CB: But I didn't even say that when I taught the course. Well, I'm rambling all over the place, but if you think it's helpful . . . you can see I enjoyed myself.

GC: Good. I also wanted to ask you, did you know Dr. Rauscher at all?

CB: Oh, yes, sure.

GC: Can you tell me a little bit about him? I'm writing his biography, too, and I just don't have as much information about him as some other people.

CB: Well, you know he was trained at Rutgers for his Ph.D. He was not an M.D.

GC: Right.

CB: Ray Bryan, who was head of the viruses laboratory—actually it was a virology section in the Laboratory of Biology, was for many years about the only person in NCI doing anything on viruses. Everybody believed in those days that viruses had nothing to do with cancer. One of the pioneers in cancer virology was Peyton Rous of the Rockefeller Institute, who did his work in 1915, and did not receive the Nobel Prize for his early work until about 1966, I think.

GC: That's the way it usually works, isn't it?

CB: So Rous and Ray Bryan and Joe Beard at Duke were about the only ones really doing much in viruses-cancer, and Bittner from Minnesota. I like to say that they kept the flame alive. In about 1963, evidence became abundant that viruses could produce cancers in animals. Ludwik Gross was the first one to show that you could get leukemia transmitted by cell-free preparations. Nobody believed him, and it took two years to get that confirmed. But once that was confirmed and the work of Sarah Stewart and Bernice Eddy with polyoma virus, which led to all kinds of cancers, got confirmed, then the field blossomed. So you had viruses after viruses being isolated that would induce cancers, and then began the look at the structure of these viruses.

So I maintain there are three different periods here: before '63 hardly anybody was interested in working on viruses in cancer; then from this point until 1970, '71, viruses causing cancer was a major thrust, but then it became clear that it was the genetic information in the viruses that led to the cancers; and then the great finding of Varmus and Bishop that this information matched very closely segments of the genetic code in our own chromosomes. And so what's happening, it looks like, is that certain things will switch on the activity of a certain genomic stretches of DNA that leads to cancer. And so these stretches were called proto-oncogenes, converted when they're turned on to oncogenes. So the discovery of oncogenes is a major shift, paradigm we say these days. Have you read Thomas Kuhn's book?

GC: No.

CB: All this about how periodically the evidence gets built up that doesn't match the going concepts, and finally you have a shift in the overall concepts, called shifted paradigm.

GC: I've heard of that, the shifted paradigm. I haven't read the book.

CB: You ought to read that book. It is philosophically important.

GC: Okay.

CB: Thomas Kuhn, K-u-h-n. I'm sorry I don't remember the name of the book.

GC: I'm sure I could find out.

CB: He's a historian.

GC: Dr. Rauscher isolated a virus. Is that right?

CB: Yes, he did. I went into this background because the third period then is the oncogene period, which we're in, and some genes are switched on or off, and then we have some genes which repress other genes. And some of those you want repressed, and when you de-repress, then you get proteins that are formed that affect the function of the cell and you can get cancers. So that, as I say, was from about 1970, '71.

So the third period is the oncogene period. Now people aren't looking at viruses except as tools that could get at the genetic coding. Well, this is partly to relate to how Rauscher got here.

GC: Okay.

CB: Ray Bryan recruited Rauscher. He had been friendly with people at Rutgers who were virologists, especially Vince Groupé. Rauscher was Groupé's student. When he got his degree, Ray Bryan hired him, and he worked in Bryan's lab. And he was lucky enough to discover one of these viruses early on that caused leukemia. Thus the Rauscher Virus—it's nice to have things named for you like that. Moloney has two of them named after him. And Charlotte Friend who was up at Sloan-Kettering Memorial who had another one named for her and so on. But we stopped doing that after a while.

CB: Rauscher was good in the laboratory, did good sound work. He was also a very pleasant person, had a pleasant personality; everybody got along with Rauscher; everybody liked Rauscher. He was a good leader. Ray Bryan didn't take to the administration too well, and Rauscher did, so Rauscher replaced Ray Bryan as the key manager for the Special Virus-Cancer Program, which was first the Special Virus-Leukemia Program, starting in '64. We made Rauscher the administrative head of that Program, and he did a good job. So when I became Director, I appointed him head of Etiology.

He, unfortunately, smoked like a furnace. And he didn't quit. And Endicott was the same; they both smoked heavily. At least I quit (mostly cigars) when I became Director**.** I never liked cigarettes particularly, but I smoked a lot of cigars and some pipe. So I thought it was appropriate to stop smoking.

I'll tell you a little story about Ken Endicott. Endicott, two days before he was to testify before Congress, stopped smoking cigarettes because he knew he'd get asked, and sure enough, "Dr. Endicott, do you smoke cigarettes?" "Oh, no, sir. I've given them up." He could hardly wait to get out of the hearings so he could light up again.

CB: Well, I didn't have to lie. I just decided I'd better stop. Also, my wife thinks cigars stink, which they do, but these new cigarettes stink, too.

GC: That's true.

CB: Anyway, that was a funny little story. So Rauscher was both a good scientist, a good manager, and a personable fellow who—as far as I know, everybody liked Rauscher.

GC: When he isolated his virus, was that before he came to NCI or was that while he was working at NCI.

CB: That was when he was working in Bryan's lab.

GC: He was in Bryan's lab.

CB: And there's no T on that, it's Bryan.

GC: But it's B-r-y-a-n?

CB: Yes.

GC: Okay. And Ray Bryan, right?

CB: Like it's Silver Spring [Maryland], not Silver Springs.

GC: Right. A lot of people call it Silver Springs, I know.

CB: There's Silver Springs, Florida.

GC: Is there? Different place.

CB: What else do you want to know about Rauscher? He was overweight and he smoked a lot, and so he had a massive cardiac failure on the Tappan Zee Bridge.

GC: Oh, is that where it happened? I didn't know that. I just knew he died of a coronary.

CB: He had a nice family with three or four children. Frank Rauscher, Jr. is now an outstanding scientist in this same field.

GC: Oh, really? I had no idea.

CB: Everybody called Rauscher "Dick" Rauscher, even though his name's Frank. I'm not sure how that originated.

GC: I saw that somewhere. I wasn't sure if that was correct or not. But he did go by Dick?

CB: Yes, everybody called him Dick.

GC: One other thing on Rauscher. I've seen things that said he had a Ph.D. in virology and some that say that he has a Ph.D. in microbiology. Would you know which one of those is correct? I wasn't sure if virology was a subset.

CB: Well, virology is part of microbiology.

GC: Right.

CB: So I don't know which way Rutgers designated it. What you really need is to find out what his diploma says, I guess.

GC: Okay.

CB: I'm sorry. I don't know the answer to that.

GC: I just didn't know if you knew. But that's what I figured, that virology is a part of microbiology.

CB: I guess I just assumed it was virology. But it may formally be microbiology—since it probably was in the Department of Microbiology, I suspect the diploma said that. Well, you could hedge that by saying his degree was in Virology-Microbiology.

GC: I might have to get a little creative there.

CB: But the project dealt with the viral causation of cancers.

GC: His Ph.D. project?

CB: What else would you like to know about Dr. Rauscher?

GC: I think that's okay.

CB: There was an obituary on him written by an NCI staff guy in the NCI Information office. Do you have that?

GC: Yes, I do. I have a couple of obituaries on him that are pretty detailed.

CB: Well, that covers it pretty well. There is one thing I got kind of misquoted on in the NCI Rauscher obituary.

GC: What's that?

CB: They had me saying that Dr. Rauscher was broadly knowledgeable of cancer. And what I said was he was broadly knowledgeable in cancer virology.

GC: Oh, okay.

CB: Because he wasn't very knowledgeable of chemotherapy, for example, or epidemiology. He became that way when he became director because, as I say, you get such an influx of information there. But no point in making a point of that.

GC: No, I won't even probably mention that. Now, did you suggest him for Director when you left? Or how did that—

CB: They didn't ask me.

GC: They didn't ask you. I wasn't sure how that came about.

CB: Well, I can speculate on some of it. One, as you know, under the new Cancer Act of 1971, the appointment of Director became a Presidential appointment for both NCI and NIH, which I think is wrong. In the old days appointments overlapped whatever party was in and whatever President was in, and I think it's preferable for it to be that way. But it's a Presidential appointment now.

So it was pretty clear that I wasn't going to be appointed because as Jesse Steinfeld, Surgeon General at the time, said, "This is a whole new ball game. They've got to have not only new program elements, but they've got to have a new team." So I would have been interested in staying longer, but I could understand why I wasn't.

When Benno Schmidt told me that I wasn't going to be Director—I guess he was telling me that Rauscher was going to be appointed, which was all right—I think that was a good choice. I probably would have suggested that if they had asked me. But with Benno Schmidt, I asked him, "Well, why was I not appointed?" He said, "Well, number one, your relationship with the staff appears to be excellent. Number two, you know cancer, broadly. Number three, you haven't got too good of relationships with Congress." Well, of course I'd only been in the job a very short time.

GC: You weren't in there long, right.

CB: "Fourth, you barely tolerate committees." I just smiled and said, "Well, it's not quite that bad!" But I was disappointed, particularly at the Council level, because I thought this represented the nation's focal point for cancer. Too many of them were still too concerned with their own institutions and their own fields. Yet from time to time they would rise to the occasion, but I still got disappointed too much, I guess and I let it show, too much of an idealist probably.

In '71 or earlier, President Nixon went up to Fort Detrick, at Frederick, Maryland, and hammered swords into plowshares by converting the germ warfare facility there into a cancer facility. So we briefed the President right before he made his speech, and I took with me Rauscher and Zubrod because they present well. Nat Berlin is a wonderful fellow who's an excellent scientist and good leader, but he doesn't come through on presentations very well, so I didn't take him. And I didn't take Palmer Saunders, the head of grants, because we were trying to focus on the content, not the mechanisms. I covered everything in cancer and the NCI programs except therapy and viruses. Gordon Zubrod covered therapy and Rauscher talked about viruses.

Rumor has it that the President remembered Rauscher when they were talking about who they were going to appoint—"Well, how about that young fellow we saw at Frederick?"

GC: Oh, really?

CB: I don't know if that's so or not. It's an interesting tidbit.

GC: Almost like a little bit of folklore.

CB: Yes. It would be reasonable. As I say, Rauscher comes through very well, and he and the President talked about baseball before the briefing started.

GC: Oh.

CB: I was, of course, strictly business. [Laughter]

GC: Of course. Well, how were you appointed? How did that come about?

CB: Well, when Endicott shifted jobs and moved to Building 1, I was immediately appointed Acting to keep things running, which was no problem for me because Endicott and I worked very closely together and I was in on practically everything he was, and often feeding a lot of information into him, so it was no big task. I mean, I didn't have any problems assuming that role.

As usual, a committee was appointed, a Search Committee. I don't know who was on it, except I know Bob Gallo was on it, but I don't know who else was on it. My suspicion is that they couldn't get anyone else to agree to take the job.

GC: Oh, really?

CB: Well, I remember one guy from the University of Wisconsin came down to consider it, and I showed him all around, and he decided he wasn't interested.

GC: Oh.

CB: I think he ended up in industry. As a matter of fact, that's the only one, as far as I know, that was interested enough to come have a look. So I don't know how many people they considered or how many they talked to or what. But anyway, that committee apparently ended up recommending me, as I understand.

GC: So then you were appointed officially by the Surgeon General?

CB: The Surgeon General in those days, which, as I say, I think is a better way to do it. Even the term of office of the Surgeon General should overlap the administrations of Presidents, in my view. As a matter of fact, I wrote a letter to the *Washington Post* on that subject.

GC: Did you? Did they print it?

CB: No.

CB: There was an article in the *Post* about the authorities of the Assistant Secretary for Health in HEW, or now "H_2S" (like the malodorous hydrogen sulfide) [HHS] as we call it, that his authorities were to be moved up to the Secretary's level, [Secretary of HHS] to Secretary Shalala. I think this was exactly the wrong way to go.

GC: Really.

CB: I pointed out that the nation still needs the nation's doctor. The PHS Surgeon General had served in that role for a long time, quite effectively in the past. The Surgeon General's authorities had already been moved up to the Assistant Secretary, now you're proposing moving them up to the Secretary's, which means that your health matters aren't being discussed from the health point as much as from the political perspective.

GC: You're right.

CB: And the authority should be restored to the Surgeon General to represent the nation's doctor and should return to the tradition of his not getting fired every time he says something that the administration doesn't like.

GC: Which, as we see, can happen easily.

CB: I had served under six Surgeons General, and we've had some distinguished people in the job. Of course, the most distinguished one was before I joined the Public Health Service, which was Parran.

GC: Parran. He was very instrumental.

CB: But of course part of the key to that was that he and Roosevelt were close—because he had been Roosevelt's head of the Health Department when Roosevelt was Governor of New York. Parran could get things done. He was a very impressive guy on his abilities. It wasn't just all due to the fact that he was buddy-buddy with Roosevelt. He had plans and a vision and saw where to go, such as establishing NIH in a bigger sense than it was before. The Clinical Center—he even had that in mind.

So, anyway, I sent copies [of Dr. Baker's letter to the *Post*] to the President, Vice President, and [Senator Paul] Sarbanes [D-Maryland]; and [Senator Barbara] Mikulski [D-Maryland]. I got what we call a "bedbug" letter from Sarbanes and Mikulski.

GC: What's that?

CB: And [Representative Constance] Morella [R-Maryland]. Well, in the old days of the Pullman sleeping cars on trains, the Pullman Company actually owned the cars that the railroads pulled, and they had berths. And the vice president in charge of operations of the Pullman Company got this letter complaining that a person who had a berth had bedbugs that bit him, and they ought to really do something about the bedbugs. The vice president said, oh, well, send him a letter, tell him we're taking all immediate steps to do what's necessary, we're very shocked and sorry this happened, and we will make every effort to take care of it. They sent that letter, and so that became known as the bedbug letter and this is the kind of answer that is often sent.

GC: I like that.

CB: And so there are a lot of times when you get bedbug letters. You may even have to write a few.

GC: Oh, I do have one other question. I've seen in some of my sources, instead of saying Director of NCI, they said Chief of NCI. Was that interchangeable?

CB: The correct title is Director.

GC: But it was Director always?

CB: In the beginning the first Director was Voegtlin. I think he was spoken of as the Chief. Because the whole NCI then was a division, so he was a Division Chief. Chief was a term used for divisions. So in those early days, I think that probably was the correct title.

GC: But you were definitely referred to as Director.

CB: Yes. We still count Voegtlin as first Director.

GC: Right, right. Absolutely.

CB: So I was number six. And we're on number eleven already.

GC: Yes, that's right.

CB: NCI is the only institute other than NIH itself who has hung portraits of their Directors. (We laugh and say that NCI hangs its Directors.)

GC: The oil portraits?

CB: As far as I know there is no Institute that does that except NCI. In fact, we used to—it was very interesting—you'd go to scientific meetings, and when people would ask questions and were asked to identify where they were from, the NIH people outside of NCI would usually say, "NIH." The Cancer Institute people would always say, "NCI."

GC: Oh, really?

CB: We had good esprit de corps. But we started out with the Cancer Institute in '39 and went into Building 6, and that whole building was the Cancer Institute. And we had the National Institute, singular, of Health, and the NCI. It was only later that it merged and NIH became the National Institutes of Health, and the Cancer Institute was then moved into it, which I think's fine. Because part of the hassle on this National Cancer Act was whether NCI should be pulled out of NIH. I guess I got on Mary Lasker's bad list when I told her I was opposed to that.

GC: You wanted to keep it?

CB: That took a little guts to tell her to her face that I disagreed with one of these things she really wanted to do. But I thought it was wrong. A lot of my friends, I think, didn't realize that at first. They thought I was going right along with everything that Mary wanted. Maybe I should have, but I think that would have been a mistake.

GC: She was pretty determined and influential, wasn't she?

CB: Yes, and on balance was very helpful, very important. So she could be irritating, but on balance I think she did a good thing. Have you got enough on Rauscher?

GC: Yes, I think so.

CB: Well, with that biography, that ought to be enough.

GC: The obituary.

CB: You didn't know about the portraits?

GC: I do know about the portraits.

CB: Oh. I thought maybe this was in line with this because there's another history going on, NIH, which I'm involved in through the Alumni Association. It's to identify artifacts at NIH.

GC: Oh, yes. Somebody had mentioned that to me.

CB: So we have a bronze bust of Voegtlin, the first Director.

GC: I've seen it in the NCI Information Office.

CB: And then there are some lovely portraits at the NIH level of the NIH Directors. With respect to the NCI portraits, Endicott and I are photographs; the rest of them are paintings, I think. Well, DeVita, I guess, is also a photograph. Leonard Scheele, who was the third Director, became Surgeon General.

GC: He only served about a year.

CB: I don't like his painted portrait, particularly.

GC: I can't think of that one in my head.

CB: It's not really abstract, but for a portrait it's bordering on that, I think.

GC : Isn't he standing back by a chair or something like that?

CB: He's got a uniform on, with his hand on his side showing his stripes.

GC: Yes, yes, I do remember that one. Some of them are better than others, I think.

CB: A couple of the other things we might go into: one, when I was consulting, one of the firms I consulted for was the TRW. TRW had a contract with NCI to develop a management information system for NCI as part of this expanded program. Part of the plans called for an information system. I was consulting with TRW and my idea was, on the basis of the goals and objectives and the planned programs, that part of the planning should include definition of the information flows that should result from the different program areas. This should be based on the scientific elements, not as usual information systems. They are mostly administrative and managerial and have very little scientific content in them. This is a big chore because it requires a lot of difficult definition of what kind of information you want, and it involves having some assessment across the board on what are the important scientific issues. But these are related very much to the goals and objectives and the program thrusts that were set out in the circular chart. So I had hopes of developing scientific information flows so you could see where you were in the research side of things. This came to naught, too.

I might say also that the planning effort for the Airlie House meetings, we had a fellow named McShuiskis who was an excellent worker, a real workaholic. If he hadn't been so efficient, we might not have had the overdose of administrative matters that were developed as part of the planning. So the planning books on the administrative side got to be bigger than the ones on the scientific side. If I'd stayed there, I would have, I think, avoided that, but it became almost monstrous on the managerial requirements that got spelled out. So you almost lost sight of the scientific program. That probably contributed to the diminishing of planning efforts, too. Now, whether we could ever get the scientific information flows as I was trying to develop, I don't know whether that could be done or not, really.

But I was very impressed on a course I had, a one-week course on systems planning where they illustrated information flows in a $2 billion project, and every Monday morning the head of that program had a summary of the status of the program on one or two sheets of paper. I thought that was a great way to manage something. What I had in mind was to see if we could develop something like that for the NCI program, but I never got a chance to do that. I must say, I was a little surprised that the project officer from NCI on this didn't really know what it was all about. So anyway, I stopped consulting and joined the Ludwig Institute for Cancer Research. Now, I think you ought to have a little background of the Ludwig Institute because this part of my career is about as important, I think, as the NCI part. We created from scratch thirteen research branches around the world, each one of which did both clinical and laboratory research. Each branch had a different area of cancer for focus. Most of these are still operating, turning out excellent research. It was gratifying to start from scratch and develop these good laboratories.

Now, it is interesting how this got underway. The funding for the Ludwig Institute resulted from Mr. Daniel K. Ludwig giving irrevocably to the Institute all of his assets outside the United States. There were about sixty corporations, so that the Institute really in effect became a holding corporation, but since Mr. Ludwig was the only stockholder on all these things, what would normally be profit was plowed back into the Institute's operations, which allowed us to fund new branches. It was sort of a unique system of funding.

The amounts of money—we got up to about $20 million a year for the expenditures on these branches—when I left. The way this happened is I got a call one day when I was, I guess, still Acting Director of NCI, from Senator Scoop Jackson of Washington who said he had a wealthy friend who was worried about stomach cancer, could we check him over? I said, well, we'll be glad to see him, but we really had no special program on stomach cancer, and my best advice was for him to go through a Mayo Clinic workup—I knew their head of gastroenterology, Dr. Hugh Butt, who had been on a number of our advisory committees. So my suggestion was that he should go see him; I would be glad to call Dr. Butt. Fine. I called Dr. Butt and told him to expect this call from Mr. Ludwig—I didn't know who he was, and neither did Hugh Butt. Incidentally, since he had red hair, his nickname was "Red," which was interesting.

GC: Yes.

CB: So Mr. Ludwig called Hugh and said he wanted to come in Friday morning and be checked over and get back out Friday night. And Dr. Butt said the right thing: "Well, Mr. Ludwig, I'm not a horse doctor. We have to keep you here as long as necessary to do a proper job." "Well, I've got to get back to my business." Dr. Butt said, "Well, Mr. Ludwig, you have to decide whether your health or your business is more important to you." "Well, I'll talk to my wife and call you back in fifteen minutes." And sure enough, he called back in fifteen minutes, "Okay, Dr. Butt, we'll do it your way." Well, he didn't have stomach cancer. I think he probably had a diaphragmatic hernia.

Senator Jackson had mentioned that Mr. Ludwig wanted to put some money into cancer research, but I didn't pay much attention to that. When Mr. Ludwig was still at Mayo Clinic, he told Dr. Butt he wanted to do something about cancer; he thought this was very important and he wanted to put some money into this area, and would Dr. Butt get a small group together to advise Mr. Ludwig on what should be done. So Hugh Butt called me back to ask if I would join in on this, after he had checked to make sure this was all honest and legitimate business. I said, "Yes, I'd be interested in that." Our first site visit in London was December of '71. I was involved in the Ludwig Institute starting in late '71. Hugh and I quickly asked Lloyd Old from Sloan-Kettering Memorial and Henry Isliker from the Swiss Experimental Cancer Research Institute to join us to form the Scientific Advisory Committee of the Ludwig Institute.

This was interesting, because here you were with a sizable amount of money, and you can start setting this up any way you wanted to. We made a decision I think was important and probably right: instead of building one big institute in one place, we decided to create branches in different locations where our staff could collaborate with other people, other good scientists. Legally we had to associate with a not-for-profit hospital. But we also were trying to set up and usually succeeded where Ludwig Institute staff could collaborate with not only hospital staff but nearby universities or institutes. We proceeded that way and we ended up creating fourteen Branches in different parts of the world.

Now, Mr. Ludwig had land holdings in the Amazon areas, Brazil, so he asked if we could do something for the people in Brazil. We would have never thought of creating a Branch in Brazil until we were asked to. We looked into it, and so we have a very respectable program in Sao Paulo. Other Branches—two in Sweden, four in London, one in Cambridge, one in Brussels, two in Australia, two in Canada, one in Lausanne, one in Bern. When Mr. Ludwig was alive, we were not able to go into the United States, but now there are two in the United States (New York and San Diego).

As I said, each Branch had a different focus on cancer; some were on virology, some were on immunology, some were on therapy, some were on genetics and heredity. This was a very gratifying experience, as well as being interesting. As you can see, I travelled a lot.

GC: Yes.

CB: So I got to see a lot of the world. I made eight trips to Australia.

GC: Oh, my gosh.

CB: It was a 17-hour ride, whether you go from Switzerland or from here, the other way. My title was Medical Director. At first I was just on the Scientific Advisory Committee, but after about a year, they asked if I would be interested in being Medical Director. As I said, I was in the Health and Human Services Administration—it's now called something else; it was HEW then—so I quickly said, "Yes, I would be interested to get back into the cancer field." For the first year I stayed in the States, but they kept asking me would I come to the home office. I moved to Switzerland in '78. I, as Medical Director, and Donald Park, the business administrator, were responsible for day-to-day operations. I was the Institute's scientific medical manager, and along with the business-type administrator, and one accountant and two secretaries for a while—that was the whole home office. As usual, it's bigger than that now.

GC: Right.

CB: Our philosophy was to give a great deal of managerial responsibility to the Directors of each Branch. We spent a lot of time talking to outstanding scientists in various locations to see if we could identify bright young men or women who could run the program. It was interesting how the same names often came up, when you talked to different senior scientists, of the young people that were coming along. We got good help from some really outstanding people around the world, in different countries, on who their bright young people were. And by and large, this has been pretty successful.

But our operating philosophy then was to pick good men and give them a lot of leeway on how they ran their own Branches. They had to present proposals for what they wanted to do before we appointed them. And we had scientific review committees who reviewed their programs every four years—now I think it's every three years—on how they're doing. And if we should close a branch, we'd give them at least a year to get relocated. And we have closed three.

GC: So are you still associated with the Institute?

CB: No, I'm Medical Director Emeritus now.

GC: Emeritus? And so how involved are you still?

CB: Not much anymore, but, you know, if you stand back and look at this, this may be as important as being the Director of NCI for such a short period. So I lived in Switzerland for four and a half years and learned to ski at age 57.

GC: Did you really?

CB: Everybody goes skiing in Switzerland.

GC: Do you still ski?

CB: I can't now because I have an equilibrium problem. I had stomach cancer, and they took three-fourths of my stomach out five years ago. Then I got sepsis, and was nearly dead for four days. Then they gave me so many antibiotics, gentamicin knocked out my inner ear nerve endings. So I'm a little unsteady.

GC: Oh.

CB: So it's hard enough just to stay up on skis without that.

GC: Yes.

CB: This was downhill skiing. But I had fun. I'm glad my wife and I had fun skiing in different places. And I got interested in church architecture while I was in Europe. It's amazing, the beauty and the skill and the effort that went into some of these little monastery, well, not always little, monastery churches. The towns were little. It's amazing how some of these small towns have these beautiful monastery churches.Each Easter we'd take trips somewhere (Munich and the Romantic Road; Rome; Paris; and Vienna). Every weekend we would drive somewhere in either Switzerland or Germany or Austria just to see new places. We also had a marvelous trip to Egypt and an Aegean Sea cruise.

GC: So you've traveled quite a bit.

CB: And fortunately we traveled first class which made it a little nicer, especially on the 17-hour flights.

GC: I guess so. Can I get you to tell me a little bit about Dr. Heller? You said you knew him.

CB: Yes. I was Assistant Director. Well, Dr. Heller was another one of these persons that everybody liked. He got a lot done simply because people felt like helping him—as soon as they met him. Somehow, he had a knack for relating to people very effectively. I don't know whether you've read any of Dr. Shimkin's interesting personalized histories.

GC: Yes, *As Memory Serves*.

CB: That was another wonderful, colorful character that I was glad I to know. He thought that Dr. Heller hadn't done very much because he tended to keep the boat from rocking, rather than being adventuresome like Endicott. So, again, it illustrates that you can run organizations with very different styles. If you look at the things that happened in Heller's regime, they were very productive: the Clinical Center, getting that clinical program added to the NCI's programs; we had a big improvement in radiation therapy machines; certainly the Grants System was operating smoothly under Heller's regime; the budget grew considerably. Of course, he was the Director for twelve years, which is the longest.

GC: Yes, I think it doubled when he was director, the entire budget.

CB: Oh, yes. I think in his quiet way he was very productive, but not as flamboyant as Endicott. I had the pleasure of working with both of them, and they both were great bosses, and I thought very productive. Endicott was, I guess, a little more productive, certainly more interesting. Heller had come out of the Syphilis Program of World War II.

GC: He ran the PHS Venereal Disease Program.

CB: And let me say a word about the Tuskegee [Alabama] business. There has been concern that for some of these people, their treatment was withheld. Well, again, as is so often the case, people are taking events and putting them in an entirely different environment, and then raising questions, or damning something without ever appreciating the situation years before. So at that time, we did not know what the course of syphilis would be without treatment. Such information was needed especially before we had penicillin because the treatment with arsenicals was dangerous. Were the results better than no treatment? In a clinical trial the most important thing is to have good controls, as well as experimental groups—so at that time we didn't know that much about syphilis. With these patients that did not get treatment—we were trying to learn what is the course of the disease without treatment. Because the treatment we had in those days was pretty toxic. If you did really need to treat these people, you ought to know that, because you could kill some of them with the treatment. Of course after penicillin became available and demonstrated to be an effective treatment, it should not have been withheld from the control patients. As a medical student, I had a job in my senior year of administering what was called a five-day treatment for syphilis. We administered, intravenously, an arsenic compound called mapharsen, and that was toxic. I didn't lose any patients when I had the job, but the guy that had it the year before me lost one. So if you put this situation back in the perspective of that time, this was not a bad thing to do. Heller is such a wonderful, kindly gentleman that for him to get tarred with this brush is almost incongruent, you know. It doesn't fit Dr. Heller to portray him as a "baddie." I think it got exaggerated, and people are using our present conceptions to weigh something from a period where those conceptions are not illustrative of what happened. Well, anyway, when the war ended, Vannevar Bush had great proposals about how science should be continued to help not only with the war problems, but now the civilian problems. Did you ever read *Science, the Endless Frontier*?

GC: No.

CB: You ought to read that one.

GC: Okay, I'll write that one down, too.

CB: Vannevar Bush. Oh, that reminds me, I had a paper in the 40th anniversary issue of the *JNCI* that gives you a lot more information. It might be interesting for you.

GC: I'll go back and look at that. I think I have it.

CB: I've forgotten the year.

GC: I have that back there.

CB: You ought to read that because that gives you some background that we're discussing.

Anyway, Heller had headed the Syphilis Program for the Public Health Service, and he had done a good job, so he was appointed Director of the NCI even though he had not worked in the cancer field. Again, in those days, PHS officers were expected to be able to run anything.

GC: So this wasn't really unusual to put him into a position that he hadn't held.

CB: Not in those days. The belief was that a PHS officer being carefully selected can run anything, which is of course not quite true.

CB: So he had a gentle style, which, as I say, made most people want to help him do the job. Another point I thought in his favor is he was very good at adjudicating things. If somebody had a complaint or staff members would get into big arguments or something, he could always calm everybody down and resolve the issues peacefully. I think he had great skill in that, too. As you can see, I think I'm very fortunate in having had good bosses.

GC: It sounds like it.

CB: Well, NIH staff generally was excellent. My stint in Building 1 was with Dr. Smadel. He was a real tough guy. People were literally afraid of him. But we got along fine and he did his job well. I didn't have any trouble with him. He dressed me down once, but I'd made a mistake, so that was fair enough. So I learned to be a little tougher from him, which you've got to be sometimes. In fact, I told Rauscher one time he wasn't tough enough.

GC: Really? So what is your relationship with NCI now?

CB: My relationship?

GC: Yes.

CB: I'm a former Director.

GC: Right.

CB: That's about it.

GC: That's about it? No committees or anything like that?

CB: No.

GC: Okay.

CB: Each Director, when he was appointed, I volunteered if I could be of help, feel free to call on me, but they never did. That's understandable. People don't think about anything except their situation and looking ahead.

GC: Right.

CB: That's true generally. Heller was a warm family man, too. He had three sons. I think a couple of them are lawyers. Incidentally, I have two daughters of my own and three step-sons from two wives and a stepdaughter who's a lawyer. I have three teachers and one nurse, one lawyer, and one communications executive.

GC: You've got a big family.

CB: That doesn't need to go in this sort of thing, I suppose.

GC: Right. I don't think I'm going to put that in this biography. You've given me a lot to work with.

CB: The Ludwig Institute is probably all you need on that, but that's—really, as I say, I think gratifying, certainly. That was what was so distressing in the Health Resources Administration. You'd feel like you worked real hard, but you couldn't see that you had much effect on anything.

GC: The Health Resources Administration was part of HHS, or part of its predecessor?

CB: I think it was HEW then.

GC: HEW.

CB: It's still there under some other name.

GC: Okay.

CB: They've got a tough job because they get into all these health care hassles. Well, it's going to be interesting to see the discussion on health care in the next year.

GC: Yes, it will.

CB: I am, of course, quite concerned that we've destroyed some very good things in medicine, but the movements are inexorably moving to change the whole pattern. The worst thing is I think the problems really started when we allowed profit-making hospitals to come into being. You've got too much emphasis on the bottom line now, and you've got people making decisions that aren't trained to. I don't think that's been an improvement. It cuts costs a bit, but you cut the system, too, on its functions. A lot of the public is finally waking up that there's something wrong with the clerks telling them that, as they put it, that they can't have this treatment. They don't say that. They won't pay for it, which means a lot of people can't have it.

GC: Right, that's absolutely right, realistic.

CB: So, I don't know, that's going to be interesting to watch. Well, I'm wondering if there's anything else that would be helpful to you. You know Victoria Harden, I trust.

GC: Yes, I have spoken to her, been over to her office.

CB: Well, she was helpful in getting all these viruses-cancer transcripts done.

GC: She's wonderful.

CB: She's been very helpful. I helped support her nomination for the Cosmos Club. So I see a lot of her.

GC: Before I turn this tape recorder off, I'm just going to note for the record that I haven't had you sign a written release. But is it okay with you if I'm using this for the biographies I'm writing for the NCI?

CB: Yes, fine.

GC: Okay, and if we need written releases later, I'll take care of that.

CB: Okay.

GC: I'm going to stop the tape.