NCI ORAL HISTORY PROJECT

INTERVIEWS WITH

C. GORDON ZUBROD, M.D.

May 27, 1997

and

June 27, 1997

History Associates Incorporated
5 Choke Cherry Road, Suite 280
Rockville, Maryland 20850-4004
(301) 670-0076

National Cancer Institute Oral History Project

Interview with C. Gordon Zubrod, M.D.

conducted on May 27, 1997, by Gretchen A. Case

at Dr. Zubrod's home in Chevy Chase, Maryland

May Interview

National Cancer Institute Oral History Project
Interview with C. Gordon Zubrod, M.D.
conducted on May 27, 1997, by Gretchen A. Case
at Dr. Zubrod's home in Chevy Chase, Maryland

GC: Would you just start by saying your name for me, for the tape.

GZ: Charles Gordon Zubrod.

GC: Okay. And today is the 27th of May, and this is Gretchen Case talking with Dr. Zubrod.

You said you had an outline and you wanted to kind of stick to that. What would you like me to start with?

GZ: Well, unless you have some procedure that you want to follow.

GC: We usually start by kind of talking about what you did up to the time you came to the Institute, just a brief outline of where you came from before you came to the NCI.

GZ: I went to Holy Cross College and then to Medical School at the College of Physicians and Surgeons, which is the medical school of Columbia University.

Then I interned at Presbyterian Hospital, which is the hospital connected with Columbia, and got slightly interested in medical research. There was a great research team in the Department of Medicine at P and S. Then the war came, and Dr. Jim Shannon was organizing a team to study the chemotherapy of malaria, the major health problem facing the Allied forces. I was invited to join the team as a young clinician. They were going to do clinical research, that is the effect of drugs on malaria and how to prevent it in the soldiers, but more importantly in a sense they really started modern pharmacology, and they got a wonderful team together. In war time they could bring together all the people in the country. It wasn't just the clinical pharmacologic studies, but there were basic studies on an animal model for the effectiveness of new drugs in bird malaria. And there were chemists—the best chemists in the country were synthesizing drugs. So it was a fabulous team.

They stayed together for three and a half years, found a way to prevent malaria in the armed forces, which was a big gain over the Axis forces because they didn't have that knowledge.

Then we went on to a study of new drugs and discovered chloroquine, which became the major drug in the treatment of malaria all over the world. But there was a rapid resistance developing, and now it's almost useless. We thought we were beating malaria for all time.

GC: Oh, really?

GZ: Unfortunately, that team was disbanded at the end of the war and now the government has tried to drum up a new team to face the problem of chemotherapy resistance.

After the war I got a fellowship at Hopkins in the departments of Pharmacology and Medicine, with the idea of going on with my studies, my interests, in chemotherapy of infectious diseases. These were bacterial diseases, especially pneumococcus.

After my three-year fellowship, I joined the faculty of Hopkins for four years. Then, in 1954, Dr. Shannon, who had moved from his post at—this work we did was all carried out at the Goldwater Hospital in New York City, which is part of New York University.

GC: The Goldwater Hospital?

GZ: The Goldwater Hospital. And after the war, Jim went to the Squibb Institute, just for a few years, and then he was off at a major post, Scientific Director of the Heart Institute. He brought with him many of his team from the malaria days, national scientists who are really outstanding. He offered me a job, but I didn't want to go to the Heart Institute. But he did offer me a job in 1954, and I came to the National Cancer Institute in October 1954.

GC: And you came in as Clinical Director? Is that right?

GZ: Well, I came in—it was a quick transformation—I came in as head of the medical branch—General Medicine they call it, General Medicine Branch, but I was soon named Clinical Director as soon as they could process the papers, a post I stayed in from 1954 until 1961.

It was an interesting time because at that time, in the early 1950s, there was a vast transformation of American science going on. The use of radioisotopes had been discovered, which led of course to the advances and the development of molecular biology. At the same time—the malaria work had been carried out under the OSRD—are you familiar with that?

GC: Yes.

GZ: All the contracts—OSRD had dozens of major contracts—were transferred over to the NIH. That was largely the basis for the large grants program that developed. Shannon was determined to develop a research base in the universities, and took this opportunity to convert all those contracts into an expanded grants program. He and Cassius Van Slyke worked hard in developing the research base through grants to universities. So there was a lot going on at that time when I came in.

As you asked, I became Clinical Director in '54, and I moved on to other posts later, but I need to develop the history of that a little bit, so I'll wait and go on with that later.

Let me make a few comments on the status of research at the time I came to NCI. Of course, the labs—there were many major labs at NCI then, they were chiefly interested in the biology of animal tumors and genetics, their characteristics, and they had a number of people who were outstanding authorities on animal tumors.

Leukemia animal research was very good under Lloyd Law and Abe Goldin, and other than animal tumor research, there was excellent work ongoing concerning the isolation and purification of amino acids. That was the major occupation of the biochemistry group at the Cancer Institute.

There was wonderful work going on, basic research, in virology, which you've discussed with Carl Baker. And also in that same group was some fantastically important work on tissue culture by Wilton Earle, which I'm sure Carl must have mentioned, which led to the ability to grow viruses in tissue culture that led to the polio vaccine and other vaccines that could be developed in vitro.

There are excellent scientists in the laboratories, but I couldn't see, even in retrospect, any overall strategy of what their research objectives were.
The same thing was true in clinical research. The Clinical Center had opened in 1953, and much of the first year, much of my early time there, was spent with the other clinical directors in trying to organize the hospital so it would be a unified hospital and not just a series of isolated services.

Again, as I said, the physicians, who had been recruited in a hurry under the draft to try to get some coverage for the patients—and they had their individual objectives research-wise, but there was no coordinated or overall plan or strategy of where they were trying to go.

The two things that interested me as a chemotherapist were studies of choriocarcinoma and leukemia. A year or so after I arrived, Roy Hertz, Min Chiu Li, and Paul Condit undertook the chemotherapy of choriocarcinoma. Are you familiar with that story?

GC: A little bit, but if you want to talk about it—

GZ: Well, Roy had an animal model that uncovered the activity of the antifolic drug, methotrexate against certain components of the developing embryo, and he reasoned that this ought to work in choriocarcinoma, which develops from the placenta in humans. There are debates about who deserves the credit, but in any event, Roy certainly had made the basic observation. Paul Condit had been studying the safety of large doses of methotrexate intravenously, and I believe Min Chiu Li saw the opportunity to try his techniques on choriocarcinoma patients and convinced Dr. Hertz. They started treating patients with metastatic choriocarcinoma and soon demonstrated the first instance of a cure of a metastatic cancer by means of drugs.

It was greeted with howls of derision when Herst first announced this, but it's proven to be true. Now metastatic choriocarcinoma is virtually 100 percent curable. So that was an interesting introduction to the successful chemotherapy of cancer. I had nothing to do with it except applaud.

GZ: As a chemotherapist, the other program that interested me was acute leukemia. Jim Holland was there when I came, but unfortunately right before I arrived he had already accepted a job at Roswell Park Memorial Institute in charge of their hematology service. But we had a month together before he left, and we discussed the chemotherapy of acute leukemia.

There were three drugs at that time that were somewhat active in childhood acute leukemia. Unfortunately, the disease always came back. But we talked about a research plan, which we adopted, and he and I agreed that we would continue it after he went to Buffalo. We set up a research plan and a protocol, followed this exactly, and this was the beginning of the first cooperative group in America, which eventually, when we added other hematologists, became Acute Leukemia B. So that was the start, and I'll come back to the acute leukemia story in a moment.

GC: Okay.

GZ: I wanted to speak a little bit about some of the forces and the individuals that developed the Cancer Institute.

GC: Great.

GZ: Internally at NCI, Bo Mider, [G. Burroughs Mider]—I don't know if you've run into his name—was the major force in developing both laboratory and clinical research.

GC: I've run into his name. What did you say his nickname was?

GZ: They call him Bo. His middle name was Burroughs, but he didn't use it, and was always called Bo Mider. He was the Scientific Director, and I reported directly to him as Clinical Director because he had charge of the whole budget and the entire responsibility for all intramural research within the Cancer Institute. Bo was an extraordinarily honest, capable individual with an astonishing knowledge of cancer. He was my tutor, really, bringing me into this new field. I had never done anything in cancer research before. My tutor was a very gentle, honest man who played the major role in developing the early part of intramural research at the NCI.

Later, internally, Ken Endicott, the Director, was a strong force in the way in which we developed, not only the Institute itself, but in the broad programs throughout the country that the Institute was interested in promulgating. Ken Endicott was a very dear friend and a highly intelligent man, and also was a major positive force.

Dr. Mider was interested in the effects of tumors on the metabolism of the host, and why cancer patients developed anemia, why they lost weight, and so on. He had been talking with Dr. Nathaniel Berlin. Have you met with him?

GC: No, I haven't.

GZ: Do you know about him at all?

GC: Tell me a little bit about him—not enough, obviously.

GZ: After he received his M.D., he went to Berkeley and worked under John Lawrence, who was the pioneer in radioisotope studies in man, received his Ph.D. from Berkeley, and became an authority on the use of isotopes for the study of red cells and the study of many metabolic processes. Bo Mider was eager to get him because he could see his usefulness in developing studies on the impact of cancer on metabolism of the patient. But Nat decided to go to England for a year on a fellowship, and then was picked up by the Navy in the draft—the draft was still on—so he spent a year or so in the Navy where he worked at the Pentagon in charge of nuclear accidents.

GC: Oh, really?

GZ: He had charge of a plane—he didn't fly it but he had it ready for any nuclear accident in the United States. They packed it with everything they would need to take care of the casualties. So that was Nat's job in the Navy down at the Pentagon.

As soon as he got out of the Navy, Bo and I talked him into coming to the Cancer Institute. And he developed the Metabolism Branch, which in short order became the outstanding branch in the whole NIH, in my opinion, and mostly I think because he recruited bright young men as clinical associates, and brought them up in the use of radioisotopes and molecular biology. He has written a history, incidentally, of what happened to all his clinical associates. It's a fascinating document. You ought to get a copy.

GC: Okay, yes.

GZ: They really have been outstanding. One of them is a Nobel Laureate, several of them are in the National Academy of Sciences, and all of them are in major positions in cancer centers throughout the country. He was a wonderful teacher and a scientist. We're still very close. He comes up here and we have dinner together and so on. But he's one you must talk to, and he'll give you a lot of information that I can't.

GC: Does he still live in this area?

GZ: No. He's—it's a long story. He's at the University of Miami Medical School now, but retired. He travels a lot and is often up here and sees a lot of his friends at the NCI.

He was a major influence in the development of the directions the Cancer Institute went, and later provided leadership in the introduction of more basic studies in molecular biology throughout the Institute, which was of course nonexistent before radioisotopes came along.

Great leadership came from Jim Shannon benefitting all the Institutes, including NCI, because he had wonderful relationships with John Fogarty and Lister Hill, who ran the Appropriations Committees in the House and the Senate. They got along well together, and they could shape our budget in ways that Jim wanted, so he provided the Cancer Institute a lot of budget help.

So those are a few comments on the internal dynamics at NCI in the mid 1950s.

GC: Yes.

GZ: Externally there were also factors that developed the Cancer Institute, perhaps not in the way that NIH or NCI wished. One must first mention the role of Sidney Farber, Lee Clark, and especially Mary Lasker who was perhaps, outside of Jim Shannon, the brightest individual I ever met. Mostly through Mary's ability to talk to Senators and House members and the Appropriations Committee, especially the Chief Clerks of the Committees, they literally developed an enlarged budget for the Cancer Institute, though not in the directions that NIH or the NCI wanted.

For example—I don't remember the exact year—we suddenly received $25 million for cancer chemotherapy. And everybody was uncertain of what to do with it. But Ken Endicott put it into what he called the CCNSC, the Cancer Chemotherapy National Service Center. Do you know much about that?

GC: A little bit.

GZ: There's a history written up in the *Cancer Chemotherapy Reports*. I guess I threw out all my reprints, so I don't have that anymore, but they're available in the Library. I think Joe Leiter, who later worked at the National Library of Medicine, was one of the authors of that history.

From my viewpoint, it was Ken Endicott who saw what could be done, and he put the money into the Cancer Chemotherapy National Service Center, the CCNSC. There were howls of outrage from all scientists that you couldn't cure cancer by means of money, especially money that the government would decide how to spend. The basic scientists at the universities, at NIH, at the NCI were all much against it. CCNSC was placed entirely outside of the intramural operation and entirely outside of the grant operation. It was an entity by itself.

Ken Endicott took advantage of this. The CCNSC, as well as the grants for that matter, were completely off grounds for any intramural scientists. You couldn't talk to anybody, you couldn't participate in any of their deliberations, you couldn't use any of their services, and so on. It was really a Berlin Wall.

I don't know through what accidents, I soon had a foot in both camps. First, somebody appointed me, without my asking for it, to one of the pharmacology study sections that dealt with cancer chemotherapy. I served for three years, and it was apparent that the study sections would never fund any clinical operation, funding only laboratory operations. So when we came along with the cooperative group idea, there was no way to get them funded.

Ken Endicott went to the Department, I guess, got permission to use this money for contracts, which was entirely outside of the grant operation. So by means of contracts, the CCNSC could do many things nationwide that could not have been done through the grant operation.

There were several panels within the CCNSC; for animals, for drug production, and so on, and they'd pass out animals or drugs on demand to scientists in the laboratories around the country, which was a great help for them and saved them a lot of money. But clinical trials were a problem. So he called on Dr. Isadore Ravdin, known as Ike Ravdin, who was at that time probably the premiere surgeon in the United States—he'd operate on all of the important people; they considered him the last resort. He operated on John Foster Dulles, for example, when he had carcinoma of the colon.

Ike was Chairman of the Clinical Panel. He was a surgeon and interested in chemotherapy, and we were pretty good friends, so he called on me to help. I advised him on what to do and recommended the appointment of all my friends whom I knew were interested in the scientific aspects of valid clinical trials.

Some of them, like Jim Holland, were interested in cancer, but others were interested in the broad problem of clinical trials in man; how you made comparisons, what were the ethical problems, what was the scientific rationale for undertaking a study, how you organized the biostatistics, how you organized the record keeping, and so on. These were physicians/scientists who were familiar with these approaches, and I knew from my work with them that they would make major contributions to the design of clinical trials.

At first the Panel funded the Leukemia B Cooperative Group that Jim and I had founded, and we added on other members, and it became an extensive activity. Then Sloan-Kettering developed Leukemia A Group under Joe Burchenal, a large cooperative group which was top notch and largely in the West, and in New York. So the first cooperative groups that were funded dealt with acute childhood leukemia.

The Clinical Trials Panel then set up a protocol for the study of non-leukemic cancers: Hodgkin Disease, breast cancer, and melanoma. We immediately set out to see what we could do in the chemotherapy of solid tumors, and a lot of interested physicians joined. This became the Eastern Solid Tumor Group, which then was the major body that studied chemotherapy and treatment generally of the solid tumors, and they're still going strong.

Ravdin sent out a general invitation to universities, saying, we want to build up clinical trials for cancer, and we were quickly engulfed by applications from universities because, of course, they too couldn't get financial support through grants from the study sections.

To handle this flood, Ravdin developed the Experimental Design Committee and made me Chairman, and all the applications came through our committee. I recruited some members from NIH, others from outside. Marvin Schneiderman was the biostatistician.

We set up stringent criteria for the approval and payment of these awards. Thus we were enabled to insist on a uniform set of standards for clinical trials of chemotherapy of cancer throughout the country. This pattern has been maintained ever since. As a matter of fact, this pattern of carefully controlled clinical trials was not employed for treatment of other diseases at that time, except at Hopkins where we had a similar committee for trials of chemotherapy of pneumonia in the 1940s. The British had done the pioneering work, splendid trials of the chemotherapy of pulmonary tuberculosis.

Without claiming credit for it, I believe this panel of Ravdin's really influenced clinical trials not only of drugs, but of surgical procedures and so on throughout the country, although I don't think anybody ever gave Ravdin's committee credit for it. Now you never see a paper in the *New England Journal* unless these criteria are followed, criteria that have been improved on I'm sure in many ways over our innocent beginnings. So that's the story about CCNSC. Now throughout the country and the world there are large numbers of cooperative groups for studies of cancer treatment.

Well, let me come back to a cure of acute leukemia.

GC: Yes. I'm very interested in that.

GZ: You know, a book has been written about that. Perhaps you've seen it.

GC: Which one is that?

GZ: It's *The Cure of Acute Childhood Leukemia* by John Laszlo, who was one of our clinical associates and went on to a distinguished career at Duke and later became one of the executives in the American Cancer Society. He decided to write up what happened in the cure of leukemia, and I'd suggest you obtain this book from the library. It's Rutgers University Press, New Brunswick, New Jersey, 1995. And Laszlo, L-a-s-z-l-o, is the author.

It covers in detail what happened at NCI and nationally in the cure of leukemia. The book was based upon telephone conversations with John by several of us, and he quotes people word-for-word of what they think happened. There's some debate as to who did what, but Parts Two and Three are important.

My role in this was as follows. In 1961, Ken Endicott became the Director after Bo Mider moved over to work in Shannon's office as the Scientific Director for the whole NIH. Ken Endicott made me the Scientific Director, which gave me responsibility for the whole budget of the intramural program and its conduct. I had some thoughts about what to do, and first turned to the possibility that acute leukemia of childhood might be curable.

At that time, there were five drugs that could cause complete remission in acute leukemia in childhood. But they never stayed free of disease for more than a year or so, and then leukemia would recur. From my experience in the malaria program where we had a team that called on the best scientists in the country to attack the problem of malaria, I thought it was a good time with this many drugs, active by themselves, to consider exploring the usefulness of combinations. Some of them had been used together, only two at a time. But everybody was afraid of the toxicity of two drugs together, and more than two drugs seemed out of the question. So I thought that with this background of what had been accomplished in the malaria program, I would form a task force that would tackle the problem of cure of acute childhood leukemia, and ask the best people in the country to come together.

I presented this idea to Ken, and he said, "Fine." He had a lot of friends at IBM, especially Manny Piore, who was one of their executives, and Ken added, "Let's go up and see Manny," because he knew that they had used task forces. We talked with their people, asking what they did when they had a major problem for IBM, which was to pick the best people in the company, detach them from their usual activities, and pull them together and tell them to work together as long as they needed either to solve the problem or decide that it was insoluble.

While this was an attractive approach, most of the people we needed were physicians who were taking care of patients. You could not detach them from their daily activities. So we modified this to the extent that we would meet about once or twice a month and everybody would come together to discuss the data, what were the best things to do. I was then presented with the problem of funding from the money I controlled to implement their recommendations, which was easy to do.

We had a great team. We worked together for three years, and we decided to try drug combinations, as well as much other ancillary research.

One of the big obstacles was that these drugs depressed the bone marrow so that patients become anemic; they have no white cells and thus get infections; they have no platelets and start hemorrhaging. Many of the children died of infection or from hemorrhage before the drugs could eliminate the leukemic cells. We had the idea—and J Freireich and Tom Frei deserve the credit—they designed, with the help of Mr. Judkins from IBM whose son had leukemia, a centrifuge with which you could put the centrifuge in line with a donor and remove only the platelets, returning the rest of the blood to the donor. Thus you could harvest vast quantities of platelets. Usually one needed about fifteen units of blood in order to get enough platelets to prevent the hemorrhage, but with the centrifuge you could get it from one donor instead of fifteen. This allowed heavy doses of drugs without worrying about hemorrhage. Similarly with infection, protective devices and preventive chemotherapy kept infections from occurring. That allowed substantial chemotherapy without the fear of the patient dying of infection or hemorrhage before the drugs could take their full effect.

Based on these findings, Frei and Freireich decided to give four drugs all at once. This was the so-called VAMP Program, V-A-M-P, which are the acronyms

for the four drugs involved. You'll see this all in Laszlo's book.
For the first—they treated thirteen patients, thirteen children with acute lymphocytic leukemia, and two were cured. These were really almost the first cure—I mean, there are stories of isolated cures in the literature, but here was a deliberate attempt to cure which was accomplished.

Based on that finding, everybody took off after that, and it was soon apparent that with the use of platelets, prevention of infection, and these four drugs at once, you could cure children with acute lymphocytic leukemia. Of course many improvements came later with different combinations, different intensities of treatment, and so on. But generally it's now believed that about 75 percent of children with acute lymphocytic leukemia are cured by extension of these basic observations.

There was one problem that we didn't solve, but Don Pinkel of St. Jude's did, that concerned the leukemic cells that invaded the brain. The chemotherapy agents couldn't reach them, even if you injected them into the spinal fluid. But he found that by total brain irradiation, you could prevent or cure this brain leukemia. So this was added on to the chemotherapy, and it was of great benefit in achieving high cure rates.

Well, based on this, NCI applied the same reasoning and logic to Hodgkin

Disease, to the many other lymphomas, and found that these also were curable, not as readily as childhood leukemia, but certainly in a high percentage of instances. The cooperative groups went on to use the same principles in the treatment of other childhood tumors; neuroblastomas, osteosarcomas, and so on—and even adult tumors, such as testicular cancer which was once highly fatal, is now curable in most instances by means of application of these principles.

It looked as though what are called the "fast-growing tumors," the tumors that divide quickly and multiply quickly, were highly susceptible to this regime of chemotherapy. But when it was applied to the slower growing tumors, breast cancer, lung cancer, pancreatic cancer, and so on, it did not accomplish the same miraculous results. That's still a big problem of course in cancer. These common, slowly growing cancers are sometimes susceptible to chemotherapy, but not with any degree of frequency.

We abandoned the task force because the job was done. We formed a similar group called the Atlanta Group, which concerned itself with basic discussions of how to get at the chemotherapy of solid tumors, but we never made much progress, and it was disbanded.

Somewhere about this time the CCNSC underwent an attack by the Woolridge

Committee. That's the Woolridge of the TRW Combine. And they accused CCNSC of mismanagement, wasting money, and so on. So Shannon asked me to chair an internal investigating committee composed of Carl Baker, David Rall, Saul Schepartz, and Lou Carrese.

[End Side A, Tape 1]

[Begin Side B, Tape 1]

GZ: The five of us were completely detached from our regular duties, and we met every day and looked at all the evidence. We found the charges to be unfounded. As a matter of fact, the Woolridge Committee had never met anybody within the CCNSC or within the intramural area to ask questions. They just talked to the university side, to scientists in other institutes about what they thought about CCNSC. The charges were false, but it was apparent to us that NCI had become unwieldy and needed to be restructured. Though Dr. Shannon hadn't asked us, we decided to suggest restructuring of the whole Institute.

GC: The whole Institute?

GZ: Yes. So we recommended four divisions, which you probably are familiar with.

There was Treatment, Basic Biology and Clinics, Virology, and Grants. And we would coalesce appropriate contracts, intramural operations, laboratories, clinics, in one of these divisions. Dick Rauscher was to become head of the Virology group, Nat Berlin head of the Biology and Clinics, Palmer Saunders head of Grants and Contracts, and I was to head what was to be called the Cancer Treatment Division.

We presented this to Dr. Shannon, and he liked it, and to the Advisory Council of the Cancer Institute, and they also bought it. I don't know what's happened since then.

CCNSC was incorporated into my shop in the Treatment program. I set about reorganizing the whole drug development program where not only CCNSC, but intramural physicians and scientists, participated in the discussions of where we should go with drug development. That worked very well, and I think this basic pattern still exists in the Treatment Division.

There was also an outside committee under Dr. Richardson, the great pharmacologist from Emory, who repeated what we did as an outside group, and they came up with the same conclusion. Their report was buried and never circulated. Perhaps Dr. Shannon feared that it would anger the outside scientific world. See if you can find the Richardson Report in the NCI files.

GC: Hard to find?

GZ: I'll bet it's been thrown out.

GC: Okay.

GZ: In order to monitor and advise the four divisions, Endicott set up a scientific directorate, made me chairman, and we reviewed the operations of all four divisions, their budgets, their contracts, and we actually had to act on their contracts and approve them before they would go out. So that was another thing that the Division Chiefs got involved in.

Now Mary Lasker comes into the picture again—an interesting story. Ted Kennedy, Mary Lasker, Lee Clark, and senators from Texas had an idea of pumping a lot of money into the Cancer Institute budget. Former President Nixon got word of it and ran away with it. Do you know about this?

GC: Yes, a little bit, but I'd like to hear your side of the story.

GZ: The day before the State of the Union message by the President, Jesse Steinfeld, who had been one of our senior physicians at NCI and was now Surgeon General of the Public Health Service, called me up and said, "Gordon, tomorrow you're going to get $100 million!"

[Laughter]

GZ: "What are you going to do with it?" So I said, "Well, we'll see if that comes about." The idea was that Lasker, Farber, Clark, et al., were impressed with what had been accomplished in cancer care and treatment and research by the cancer centers. But these were all in free-standing places like NCI, Farber's Jimmy Fund, M. D. Anderson, and Memorial Sloan-Kettering. One of the reasons for this new money was to develop fifteen new cancer centers within universities. The plan went ahead, and I think it was a great move. The planning involved NCI, but mostly people from the outside. We had endless meetings. I was put on only one committee, fortunately. Of course the universities played the major role in this, but Lee Clark and Farber and Lasker through the appropriation process had their hands firmly on how it was going to operate.

Fifteen new centers developed, funded by NCI and the universities. This has been a great boost to cancer research and care and cancer control and epidemiology in the universities.

So really, this outside influence really determined a lot of the future of NCI activities, in a very favorable way, I think.

GC: By outside influence, you mean Mary Lasker—

GZ: Mary Lasker and her team.

GC: And the whole National Cancer Act that came about.

GZ: Yes. Let's take time out here for a breather. I'll show you—

[Brief break]

GZ: I went to the signing of the Cancer Act by Former President Nixon. I met with him afterwards, and he gave me one of the pens that he used to sign the Act—

GC: Oh, my goodness!

GZ: —and I lost it. My son found it last year—I don't know where. And he set it up for me.

[Shows interviewer framed arrangement of pen and photos described below]

GC: Look at that!

GZ: My son Justin wrote to the White House, he wrote to some of his friends in Congress and so on, and he got all these things. This is a picture of President Nixon signing it. This is the pen that he gave me, and this is the first page of the Act.

GC: Oh, my gosh. That's beautiful.

GZ: So it really happened.

GC: That's really great!

GZ: He said he has a good fellow who frames things—

GC: Your son?

GZ: —for his customers, so he gave it to him and said, "Pretty it up." [Laughter] So I was pleased to get that last Christmas.

GC: You must have been so pleased that he found that pen for you.

GZ: Yes. I must—I haven't asked him where he found it, but . . . I don't know, maybe he talked somebody into giving him another one, but—

GC: So you were there for the signing of the Act?

GZ: Yes, but I wasn't in the picture.

GC: What was that like? What was that day like?

GZ: Oh, I don't know, everybody came to it, Mary, and there were a lot of NCI people in the room, but mostly bureaucratic types and all the people from the Department—at that time it was Health, Education, and Welfare. There are a bunch of Russians in the photograph, as a matter of fact.

GC: Oh, really?

GZ: They just happened to be visiting at that time. There were about five or six Russians in the back row there. [Laughter] Well, anyway, that was the Cancer Act.

GC: Did people know at that time how big this was going to be? Do you think there was a sense that this was really going to—

GZ: I think so, I think so. I think we were realizing then it was time to spread what the cancer institutes had learned about coordinated care, about coordinated care and research, and coordinated basic research with clinical activities, cancer control, and epidemiology. Yes, I think we all felt that it was a wonderful move.

But of course the basic scientists were screaming and still are.

GC: Just the idea of government funding? Was that what they were upset about?

GZ: They just don't like government telling them what to do. I don't blame them . . . but they were wrong, and I know by how many have been incorporated into Cancer Center activities.

[Laughter]

GZ: That brings me to the end of my participation at NCI. In 1974 there were low salaries for government scientists because you couldn't receive more than anyone in the House of Representatives received as salary, and they always kept it unofficially low. And I had four children in college at once, which is a great way to reduce all your assets.

[Laughter]

GZ: So my wife and I decided I'd better get another job, and I retired from NCI and went to direct the cancer center at the University of Miami Medical School. I spent seventeen years at the University of Miami before I completely retired.

My wife and I had a wonderful time. She loved to swim, swam every day, and we played tennis as often as the weather was suitable. And she was relieved of all the care of our big family and chauffeuring and all that sort of thing. So we had wonderful times together. Then the happy times began to end because she began showing signs of memory loss and poor judgment, and it was soon clear she had Alzheimer's Disease. I took care of her for years, but finally it got so bad that I decided to move back here because I have two daughters living nearby. And we battled it out for about another year, but then it was becoming quite dangerous for her, so we got the family together and looked at all the nursing homes here and found a beautiful one out in Olney, where she entered and is still there—has not shown much progression, so I'm beginning to wonder about the diagnosis. But anyway, she's very happy there, and we go see her and go out to lunch with her every week. She likes the people there and many of the activities that they have. She exercises a lot, and swims occasionally, as often as she can. She won a medal in the Senior Olympics in the 50-yard freestyle.

GC: Oh, my gosh!

GZ: So she's having a great time, and she's not progressing.

GC: Well, that's good.

GZ: There are a couple of minor points relating to the foreign involvement in research by NCI. This is very extensive, and you probably have to get the full story from the epidemiologists and some of the pathologists. The best person to talk to—I'm sure you know—is Greg O'Conor.

Greg was a pathologist, and he goes to the same Catholic parish that I do, so I see him often. He became quite interested in African lymphoma. Have you heard of this, the Burkitt's Lymphoma?

GC: Yes.

GZ: So he went to Uganda and worked with Dennis Burkitt, and made basic descriptions of the pathology and pathological evolution of Burkitt's Lymphoma, which occurs in children as a big, lumpy jaw, and is apparently a virus-induced cancer. There was great interest in this all over the world, and people began flocking to Dennis Burkitt's clinic. Then Dennis was suddenly recalled—he worked for the British government, as did so many of the Ugandan physicians who were chairmen at the medical school—and he had to go back, and asked NCI if we'd take over the clinic for him. So NCI set up a unit there. Paul Carbone, I think, was the one who was doing much of it. Do you know Paul?

He's at the University of Wisconsin.

GC: I know the name. I've seen his name all over the place.

GZ: We got agreement from Dr. Endicott and Dr. Shannon and sent over Public Health Service offices to run this. John Ziegler who is at the University of California-San Francisco, and Chuck Vogel who was later with me at the University of Miami, but left to go into private activities. They had an exciting time. I went there once, because I was remotely responsible for what was going on; I thought I'd better see it so I'd know a little more about it. It was a wonderful unit. Chuck Vogel found that in addition to the lymphomas, there were many patients with Kaposi's Sarcoma, and of course, this is one of the tumors that is now known to be common in the AIDS patients.

Kaposi's Sarcoma in the early '70s was regarded as a disease of older men living around the Mediterranean Basin, usually Jewish, and with x-ray it disappeared. But this Kaposi's in Africa was fulminating and fatal, and nothing seemed to touch it.

Chuck studied the basic chemotherapy of Kaposi's and published the classic studies on the chemotherapy of Kaposi's Sarcoma.

Later, when AIDS came along, about ten years after Chuck Vogel was there, it became apparent that this was related somehow. So in retrospect they wished they had studied the Kaposi's a little bit differently.

But anyway, that was an important activity of the Cancer Institute. And Greg O'Conor who was there for a year or so could probably tell you all about it. Have you talked with him at all?

GC: No, I haven't.

GZ: But he also a feel for a lot of the foreign adventures of the Cancer Institute, our involvement with the French Cancer Institute at Lyon. I think he was over there for awhile, or at least he used to visit for long stretches, and he was involved in some of the other foreign activities. I think you might look at a whole section on the foreign tentacles of NCI and what they did and what they accomplished.

Some of the pathologists and epidemiologists were interested in studies in Africa, in the Caribbean Island of Curaçao, in Hawaii, in Japan, and so on. If you were writing this up, I would think a separate section on the foreign involvements and the foreign productivity of NCI's activities might be worthwhile.

GC: Yes, it sounds like it.

GZ: One other thing concerned our adventures in Russia, which you probably heard about. In the early '70s, Brezhnev and Nixon met in Russia and decided it was time to get together on a number of things and try to do something about the Cold War. They decided that science should be the first step, and that of all these, cancer research would be the best place to make a start.

The White House staff got in touch with me and asked me to head up a team to go to Russia and see what we could do to help them and learn what we could from them. It was around 1972 that I assembled a team and we went to Moscow—Drs. Wassermann, Burchenal, Selawry, Golden, and myself. And we spent a couple of weeks there and pounded out an agreement, and it was around cancer chemotherapy.

It was exciting—for example, on the way over, when we landed in Paris at Orly Airport, the pilot came on the intercom and said, "Dr. Zubrod, the White House is on the phone. You'd better come up here."

[Laughter]

GZ: By the time I got there, the connection was lost, but when we got to the hotel, it was President Nixon, and he called me from Camp David saying, "Now, please, do everything you can to be friendly with the Russian scientists, and learn what you can do to help the Russians, and I will appreciate it very much."

GC: Had you spoken with him before? Was it the first time you'd spoken to him personally?

GZ: I met with him twice personally, once at the signing of the Cancer Act and again when Fort Detrick was turned over to NCI.

GC: Okay.

GZ: So anyway, we had a good time in Russia. They rolled out the red carpet, and we had great fun.

GC: I'll bet.

GZ: They treated us so royally that I felt, as leader of the group, I had to reciprocate. So I went to see the U.S. Ambassador and got past the Marine guards, and said, "We've got to do something to show our good will towards the Russians. The President wants it." He said, "Okay. I'll put on a little afternoon session at my home," which was a beautiful place. We went there and we got all the top Russians together. They were delighted to have Russians all over the American Ambassador's home—

GC: [Laughter] I'll bet!

GZ: —and be given champagne and vodka. And everybody loved that.

And when I got back—two things: one, the White House said they wanted me and the team to come out to San Clemente and present the results to President Nixon. We were all geared up for that, but the President got some kind of emergency and they called it off, which was a great loss for us. But also waiting for me was a bill from the State Department for the soirée.

GC: So they charged you!

GZ: I sent it off to NIH, and they mumbled a bit. They said, "Zubrod, this will use up half of our NIH entertainment budget for the year."

[Laughter]

GZ: I said, "Well, you don't want me to complain to the White House, do you?"

[Laughter]

GZ: So they paid it, but they grumbled for a long time.

GC: Oh, my gosh! [Laughter]

GZ: I suppose there wasn't much entertainment at NIH that year after that.

GC: I guess not.

[Laughter]

GZ: In the next year the Russians came over to see us and insisted on seeing what American home life was like, and they descended on our home and were great fun. They danced with the children, they were very much interested in our refrigerator and the dryers and things like that. They went poking around, opening everything.

[Laughter]

GZ: But they were nice people. We got to know and like them very much. As a result—not of the visit to my house, but—they proposed to the White House that we come back for a return visit the following year. But this time, they said, you must bring your wives. NIH had to swallow pretty hard on that one, but the White House prevailed, and so we went over with our wives, and they were an important addition.

My wife insisted that it wasn't our scientific arrangements that helped cool off the war between Russia and the United States; it was the nice social events that they took part in while they were in Moscow and St. Petersburg.

[Laughter]

GZ: While that was very amusing, it did lead to other scientific exchanges between the United States and Russia, and maybe played a tiny role in softening the bad feelings.

GC: It sounds like it went well.

GZ: Yes, it did. And I don't know whether it still continues. It possibly does.

The only other thing I mentioned about meeting President Nixon was the time he turned over Fort Detrick to the Cancer Institute, which has become very important as the Frederick laboratories. We all piled out there, and all the big wigs from the Department and the Senate and the House were there, and Nixon was charming—we sat around a table with him and chatted. Each of us made two-minute presentations about the Cancer Institute, and then he gave his speech out in front of all the troops, about turning swords into plowshares and spears into pruning hooks, and so on. And this was a great gesture for the

United States to get rid of bacterial warfare preparations and use the space for peaceful purposes. He turned over the whole operation to the Cancer Institute, which I think has been of great benefit to the NCI. This is another one of those unexpected external influences that resulted in formation of the present-day Cancer Institute.

GC: Now, on that, did you have any warning that that was going to happen, that Fort Detrick was going to be turned over?

GZ: Oh, yes. They just said, this is what it's all about. But the President was charming, I must say. We talked and joked about football and baseball and important things like that.

[Laughter]

GZ: Well, that's about all I have, Gretchen.

GC: Okay. Well, there were a few things I wanted to go back to.

GZ: Sure.

GC: You were talking about the Woolridge Committee, that they attacked the NCI for wasting money on the CCNSC and that the charges were false. But do you have an idea of where they came from in the first place? You said it was maybe scientists at universities. Why would they have made those kind of charges?

GZ: I really don't know. I think the scientists were jealous of the prerogatives of a basic scientist and were insistent that only the most fundamental basic research could ever solve cancer problems. Well, this is obviously false, because we cured leukemia without any basic research at all. [Laughter] We were just plugging away at clinical leads, you know, and the same for Hodgkin Disease, testicular cancer, all the childhood cancers, and so on. So I think their fears are misplaced, but they speak for themselves.

GC: But that's maybe what that grew out of?

GZ: I suppose so. I don't know the background of that. You might call Mr. Woolridge and ask him.

[Laughter]

GC: That's an idea. Now, you brought in Dr. Frei and Dr. Freireich to the Institute.

GZ: Yes.

GC: Why did you bring those two together? How did you know them? How did you hear about them?

GZ: Oh. Well, there was one episode of my life that I skipped over because it was so brief and so disastrous. I left Hopkins in 1953 when the closest friend of mine, Dr. Phil Tumulty, became Chairman of Medicine at the medical school of St. Louis University.

GC: Tumulty?

GZ: Tumulty, T-u-m-u-l-t-y. I had been at prep school with him, and then when I went back to Hopkins after the war, he was the Chief Resident on Medicine. We hadn't seen each other for fifteen years after prep school, but we were very close friends. Our families became closely linked together and had strong Catholic loyalties to one another, and we both wanted to go to a Catholic environment. He was offered the Chairmanship of Medicine at the University of St. Louis and pleaded with me to come with him to be in charge of the research, and I did. And it turned out to be a mistake. We lasted only a year because imposing Hopkins-style medicine in the medical school was a financial disaster for them, and the practitioners and many of the staff turned against us and stopped admitting patients, and the income of the hospital dropped precipitously, and the University president made Phil Tumulty resign, and I resigned in protest.

Much of my summers were spent in my avocation, which is marine biology. I used to go up to a number of marine labs, but the main one was at Mount Desert Island Biological Laboratories, which was near Bar Harbor on Mount Desert Island.

[Points at painting on wall]

GZ: That's a picture of our back yard. We owned a cottage up there. We had lots of blue lupine, a characteristic flower of the north. Our children commissioned the painting on our fiftieth wedding anniversary. It was while I was there in the summer of 1953, while I was at St. Louis, I received a call from Phil Tumulty, "I've been fired," and so we didn't know what to do. I turned to Bob Berliner, who was a close friend. You probably don't know him; he was Scientific Director of the Heart Institute and later Scientific Director of NIH, and then became Dean at Yale. He was in the malaria program, so we've been close friends since about 1937, I guess. He happened to be at the lab at that time, too, so he talked to Jim Shannon and Jim said, "Well, we have a job that needs to be done at the Cancer Institute, and why doesn't Gordon consider that." And so I did, and that's how I got to NCI was through resigning from St. Louis, with help from Bob Berliner and Jim Shannon. Then Bo Mider flew up to Bar Harbor. We had lunch together, and he offered me the job as Clinical Director. So, not knowing anything about it, without much thought except getting my wife's approval, I accepted and off we went, to complete the disastrous year.

GC: It sounds like it.

GZ: Tumulty had made Frei our Chief Resident at St. Louis University. The hospital never had a house staff; they used medical students to run the wards. In the first year, Dr. Tumulty assembled a full house staff, including a Chief Resident. Tom Frei, who had been in Pathology at Washington University, became our Chief Resident. I had worked with Tom in clinical trials research of pneumococcal pneumonia while at St. Louis. When Tom resigned because of Phil's firing, I offered him a job to run the leukemia program succeeding Jim Holland.

As to J Freireich—the draft was on—Freireich was in the fellowship up at Boston University under Joe Ross, and he called and said, "Freireich is in trouble with the draft. Have you got a job?" I said, "Sure." So he came down, and Frei and Freireich immediately made a team, a wonderful team, and they are the ones responsible for the cure of acute leukemia. See Laszlo's book!

[Laughter]

GC: What was the atmosphere like when you worked on these teams with Frei and Freireich? Was it a very comfortable atmosphere? Was it very competitive? What was it like working in the late '50s and early '60s at NCI?

GZ: Well, you ought to talk to Tom Frei, because you'll get different views of that from Frei and Freireich in their write-ups here.

GC: Right. I'm talking with them next week, actually.

GZ: You are?

GC: Yes.

GZ: Well, Frei just sent me a paper he gave at Freireich's seventieth birthday festschrift down at Houston, and he sent it to me for my critique. His paper goes into that in great detail.

GC: Oh, really?

GZ: It was pretty inflammatory in a way because Freireich was hard-driving but highly innovative, and Tom was always calm and kept a lid on things. But together they made a good team. Ask Tom for his speech that he gave at

Houston. It's very amusing, and also I think quite accurate.

I don't think there was much competitiveness. There may have been a few outside clinicians, but the task force allowed people to get things off their chest, you know, and arrive at reasonable plans for what to do.

Inside, I think, there was a time of great excitement; every day some really astonishing thing happening in the way of progress. We all used to go on rounds together and discuss all these things, what the opportunities might be and what comes next and so on. So I don't remember much competitiveness. I remember very fiery conversations, you know, but I think it was more out of the joy of the hunt.

GC: And Dr. [Vincent T.] DeVita worked with you, too. Is that right?

GZ: He came in as a clinical associate, yes, and participated as a clinical associate in some of the early studies in the chemotherapy of Hodgkin Disease, and eventually set up—although the initial work for that was done by Frei—major studies within NCI itself that showed how much could be accomplished by applying the same principles, although different drugs, to the treatment of Hodgkin Disease. He made a fine career for himself out of the treatment and cure of Hodgkin Disease and other lymphomas.

And then, of course, he became the Director after I left. I was never the Director, but when I retired in 1974, he succeeded me as head of the Treatment Program, and then when Ken Endicott left to take another job in the Public Health Service, Vince became Director.

GC: It was 1980.

GZ: Was it?

GC: Yes.

GZ: Yes, I guess so. When I retired I separated myself completely from science and medicine. While my wife was getting sick and needed to be watched, I started writing a biography, myself, my family, and I discovered I didn't know anything about my antecedents, my ancestors, and so I became a genealogist, and I've been really working intensively on the genealogy of my family. I've traced them back to about 1500 in Germany.

GC: Oh, really?

GZ: I've been over to Germany three times to try to find my relatives. And it's been good. While I'm now an enforced bachelor, it's something to occupy the hours.

About a year ago I realized I was doing lots of genealogy with little progress on my biography. I abandoned genealogy for awhile and finished my book. I just turned it over to the publisher a few weeks ago, and it will be published in August.

GC: How exciting!

GZ: I'll send you a copy.

GC: I would love that! Who's publishing it?

GZ: It's privately published by Gateway Press in Baltimore; by Ann Hughes who runs the Press and is highly professional. The book should be out in August. But it's for family, so that our children and grandchildren and great-grandchildren will know something about their family and their remote ancestors.

GC: Right.

GZ: There's not too much in it about the NCI or my career, but more on our family life together.

GC: Well, that's what's really important, right?

GZ: Yes. Well, actually, one of my children saw an early draft of the first few chapters, and said, "You've got to make it your spiritual journey and not your scientific journey." So that's what I've been doing. So I don't know how much it would interest the citizens of the scientific world.

[End Side B, Tape 1]

[Begin Side A, Tape 2]

GC: When you first started at the NCI, Dr. Heller was the Director; is that right?

GZ: Yes.

GC: What do you remember about working with him?

GZ: Well, he was a very nice person. We got along well, and he was helpful to me. He was much interested in the American Cancer Society (ACS) and spent his time in interaction with them and helping them with the coordination of NCI and ACS plans. He gave the responsibility for the Institute to Dr. Mider and to Ralph Meador, who was head of the Grants Program. And he did not get heavily involved in the day-to-day activities of what was going on within the Institute. I'm sure he knew about them and I think Meador and Mider kept him up to date and asked him for final decisions. He was a nice, gentle person, and we had pleasant relationships.

GC: And then you've talked about Dr. Endicott, that you enjoyed working with him.

GZ: Oh, yes! He was wonderful, a true Westerner, very bright, acted like a farmer. He had a big farm.

GC: Oh, he did?

GZ: Yes. And he'd come to work with his boots on and mud all over them.

[Laughter]

GC: Would he really?

GZ: Yes. He was full of wonderful stories, had a quick wit. He and Van Slyke got along well together. Van Slyke was in charge of the Grants Program, in the front office for the whole NIH extramural operation—a highly effective leader.

Some of those early Public Health Service officers were great people, and Endicott was one of the greatest.

GC: I've only heard good things about him.

GZ: Yes. He was so effective in dealing with Congress. And again, he worked with the chief clerks of the Appropriations Committee, and they were good pals. They would settle the language of the appropriations and how much went where and so on.

But he was very smart, full of common sense, and extraordinarily helpful in advising me in which way to go on major things. I thoroughly enjoyed my contacts with him. He's the best Institute Director NCI had.

Do you know what he looked like?

GC: I do. I've seen pictures of him.

GZ: You've seen pictures of him—a forceful, wonderful person.

GC: He looked like he was a very happy person. In all his pictures he's got a very good smile.

And then Dr. Baker came next.

GZ: Yes. After Endicott left, the Directorship was vacant for quite a while. I think Carl agreed to take it, but I don't think his real interest was in that, and I think he decided to do something else.

GC: The way he told it, he stepped down after the National Cancer Act was passed because it was clear that they were going to do things differently.

GZ: I see.

GC: Because it became a Presidential appointment.

GZ: Yes.

GC: And so he stepped down. And then they appointed Rauscher, I guess.

GZ: Yes.

GC: You were still there while Rauscher was there, is that right? For a couple of years?

GZ: Do you know the year that . . . I don't think so.

GC: Let me see. Rauscher started in '72.

GZ: I was there until '74, so I must have. Rauscher and I got along well together. He died fairly young as I remember. He developed a wonderful virology program, both nationally and within NCI.

GC: Did you have anything to do with the Virus Cancer Program? Were you involved in that at all?

GZ: Probably Carl Baker discussed this with you, but shortly after I became Scientific Director, Jack Dalton, our Chief electron microscopist, discovered some particles, virus particles—he thought they were virus particles—in urine of children with acute leukemia. And they resembled particles that had been demonstrated in a cow leukemia in Scandinavia. Ken Endicott and I went down to the Department of Agriculture and told them what Dalton had found, and they pleaded with Ken not to mention the finding to anybody because it would frighten every mother in the country and destroy the milk industry. So they urged us to continue the research. And they said they'd work to get us some extra money. (Carl Baker and I disagree on where that money came from.) Anyway, I was suddenly presented with an extra $10 million in my Scientific Director's budget to increase virology research on milk and in leukemia.

Jack Dalton soon discovered the observations were artifacts, and the milk industry and the Department of Agriculture breathed a sigh of relief! But we got the money anyway, and Dr. Endicott asked me to present him with a plan for its utilization. I consulted heavily with Dr. Ray Bryan in developing a strategy.

At that time the polio vaccine was showing that polio was not going to be much of a problem anymore. There were a lot of basic virology scientists who were looking for a new challenge, so we decided to present this to them as an invitation to take grants or contracts to get on with the virology of leukemia and other tumors. We spent some of the money on that. But a lot of it I gave to Rauscher, who was head of the Virology Division, and he expanded the activities there. And you have all this from Carl.

GC: From a lot of different sources, yes.

GZ: That's what happened with my involvement in virology. But I didn't have much to do with it except I wanted Rauscher to develop a program, which he did in a beautiful fashion. One of the stars he had there was Ray Bryan, who had done a lot of very basic research on Rous Sarcoma and other animal tumors, which were extraordinarily helpful in understanding the biology of viral carcinogenesis.

When I was Clinical Director there were two people, Sarah Stewart and Bernice Eddy from another institute, who made an astonishing discovery. There was a leukemia in mice that was very confusing. By tissue culture Sarah showed that there were two viruses involved, which she demonstrated by growing the viruses in cultures, proving there was a mixture of two viruses and if you cultured them and separated them, you could understand the discrepancies. I believe that was an important advance that Sarah and Bernice produced. One of the viruses is known as the polyoma virus because it produces as many different types of tumors.

GC: Did you work very closely with her? You knew her very well?

GZ: No. Bo Mider did. He was concerned about the validity of her finding. And he brought in outside virologists, and the virologists and pathologists and Sarah Stewart went over and over the slides until they found out she was right. Carl Baker can tell you all about that. I believe she made extraordinary contributions toward understanding mouse leukemia and in the process discovered the polyoma virus.

GC: And that's written up everywhere as one of the very big advances.

GZ: Yes, she deserved a lot of credit. And Bernice Eddy worked with her and contributed substantially.

GC: What do you think was your greatest accomplishment while you were at the NCI? What are you the most proud of, or what do you feel made the most impact while you were there? Does anything stand out to you?

GZ: Well, I don't know. I don't like to take credit for anything. It starts too many fights.

[Laughter]

GZ: So I never put my name on any papers coming from Task Force activities.

GC: Oh, you didn't?

GZ: Because I think that someone in administrative authority should never do this. Well, to put it another way, the thing I had the most fun at I think was the concept of the vision of a task force, that we had the elements of a cure of leukemia available to us if we just used them, and the idea of pulling together a task force that would go over all these things, make decisions as to the next best step, and then implementing them with my budget and whatever administrative authorities I supposedly had, to bring them about. I think that was the thing that gave me the most satisfaction and joy.

GC: And it turned out so well; it worked.

GZ: It worked.

GC: Now, I had another question about that. We were talking about choriocarcinoma. You said that it was when Roy Hertz and Min Chiu Li, their work was derided, there was a great outcry against it. Did I get that right?

GZ: I think the medical profession was so imbued with the idea that metastatic cancer was incurable that when Roy Hertz made the claim that, "I've cured metastatic choriocarcinoma," they disbelieved and just laughed. I think it was that kind of thing. This happens in science all the time; when something truly revolutionary occurs, all the old people who have been working at it say it's impossible.

[Brief interruption]

GC: One thing that I've come across a lot with the National Cancer Act is that people who wanted to pass the National Cancer Act were talking about it in the same terms as putting a man on the moon or building the bomb, and a lot of scientists reacted with, you can't talk about cancer in those terms, you can't talk about curing it by 1976, which was something that was thrown around a lot then. How did you feel when you were hearing these kind of things like, "we're going to cure cancer in five years, we're going to cure it by the bicentennial."? What was the general feeling at NCI? Was it true that scientists were really saying, you can't talk about cancer in these terms?

GZ: Yes, it was really true. I think it's wrong to put a time frame on success, but in terms of the accomplishment of the objective, ultimately, I was all for it. As a youngster, when I was an intern and resident, then later when I was on the faculty at Hopkins, I was involved with the penicillin research, which repeated what happened in the malaria program. The British called together their best people and took this observation which had been overlooked for ten or fifteen years, and developed it into penicillin and made it rapidly available throughout the world.

So when scientists say you can't organize an effort to do something in a short time, they're just wrong as can be. So with that experience and then my later experience with the malaria team, which in a short period of time protected the allied troops against malaria, in Burma, China, North Africa, Pacific Islands, and so on, it's just wrong that you can't take a health objective and succeed by pulling the right people together and giving them all the resources they need and telling them to find the solution. The Leukemia Task Force did the same thing. Of course, you have to be able to identify that there exist sufficient leads to make the effort feasible. When scientists say you can never do these things by an organized effort in a short time, my whole spirit revolts against what is obviously wrong-headed.

Now, to say that you're going to do it in two years or five years or ten years is not right. I believe a great deal more could have been done in the AIDS program if it hadn't been taken over by the politicians and public pressure and forced them into scientific—I won't say scientific—but practical decisions as to the design of clinical trials. I think they're suffering from it now as so little progress has been made.

GC: So I guess there are two sides to having politicians or the government involved in research, is that you do get the funding and the help, but you also have their agenda.

GZ: They had the AIDS victim playing a major role in what should be done with the research, which is nice public relations and nice in a humanitarian way, but unwise as far as clinical research goes.

GC: And is that true with these cancer groups?

GZ: With what?

GC: There are a lot of cancer victims, too, that are very involved in how they think research should be going. I mean, that's kind of what Mary Lasker was doing in a way, not that she was a victim, but she was trying to—

GZ: Her husband was a victim.

GC: Right.

GZ: She was so smart. She didn't miss a trick. But she didn't interfere with what needed to be done. She used to invite me to luncheons at her home—and I don't know if you know Mrs. Blair; she was Mrs. Lasker's protegé and has played some roles since I've left in some of the Advisory Council's activities in the Cancer Institute—and they have a beautiful home here in Washington, and Mary would have luncheons there for us. She was very astute, as I say, one of the most brilliant individuals I ever met. But she didn't interfere with what the science should be. She just wanted to know, "What do we need? I'll get you the authority and money to do it." But that's different from the politics of some of our groups that want this done or that done.

GC: Okay. Is there anyone else you can think of that I should talk to? You've mentioned a few names of people that I should get in touch with.

GZ: I jotted down a few. You have Frei and Freireich, I guess.

GC: Yes.

GZ: Greg O'Conor.

GC: Greg O'Conor.

GZ: John Ziegler, who's head of the unit out in Uganda, but you might be able to get most of that from O'Conor, but Ziegler was the clinician there and the one who did a lot of the work.

And Charles Vogel—I don't know where he is now, but he would be easy to find—was also there with him, and he was a very sharp observer and has a lot of interesting things to say.

I mentioned Palmer Saunders—I think he's still alive, down in Texas, on the Grants Program. Also Paul Condit in Hawaii about choriocarcinoma history; John Edgecomb about NCI's foreign activities; Ti Li Loo, Bethesda, about Frei and Freireich; Lewis Thomas, a pathologist about Dr. Red Stewart (who won't be interviewed).

GC: Okay.

GZ: And you have Berlin, right? You're going to—do you have his address and number and everything?

GC: I don't. I may be able to get it from NIH, unless you have it.

GZ: I can give it to you.

GC: Okay.

GZ: And also, one of my closest friends [later, off the tape, Dr. Zubrod names Alfred Ketcham] who was in charge of the surgery at NIH, and developed surgery there. He was in charge of surgery at NCI, but also the Chief Surgeon for NIH. And he developed the immunology approach to cancer, which Steve Rosenberg later took over. I think he would—and I can get you his address and number if you want him.

GC: Okay.

GZ: I think those are the major ones. I don't know if there's any point in talking to Dan Nathans of Hopkins. He was one of Berlin's protegés who had received the Nobel Prize for his work in molecular biology. But it would be interesting to talk to him about the early development of molecular biology within the Cancer Institute. He's at Hopkins.

I think those are the major ones. Were you going to talk to Vince DeVita?

GC: Yes, I'm talking to him next week.

GZ: Oh, good. Well, give my regards.

GC: I will. I'll do that.

GZ: I like him very much.

GC: And Dr. Upton. I don't know if you knew him.

GZ: Who was that?

GC: Dr. Upton. He was another one of the Directors, Arthur Upton.

GZ: Oh, Art Upton.

GC: Yes. He came after you left probably.

GZ: Oh, I didn't realize that.

GC: I'm going to talk to him, as well.

GZ: One of the interesting sidelights of that—he was at Oak Ridge, and that's where the idea of bone marrow transplant science developed in animals, which was based on work of a lady scientist at NCI, whose name I cannot come up with—I'm sorry. But you can ask him about it.

But we used to go down to Oak Ridge to talk about bone marrow transplant in animals and what it meant for humans. And of course later Don Thomas and Joe Ferrebee at Harvard, and then Mary Imogene at Bassett Hospital in Cooperstown developed the bone marrow transplant in leukemics, and then as Don Thomas went out to Washington, I guess he got the Nobel Prize for that, too, didn't he?

GC: I don't know.

GZ: I think so, for bone marrow transplant. He did a wonderful job. But that started when Art Upton was in the middle of that at Oak Ridge. We used to go down there about once a month to talk over this problem with the Oak Ridge people.

GC: Oh, really?

GZ: Yes. I had to get all sorts of Q clearances to go into that sacred place, I guess, although it was way after the war.

GC: You still do, I think.

GZ: But they were still afraid, I guess, of the Russians getting information, which they had all along anyway.

[Laughter]

GC: Well, have I missed anything? Is there anything else you would like me to ask?

GZ: I don't think so.

GC: Okay. Well then I'm going to end this interview with Dr. Zubrod. It's May 27, 1997.

[End of Interview]