

NCI ORAL HISTORY PROJECT

INTERVIEW WITH

VINCENT T. DEVITA, Jr., M.D.

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**National Cancer Institute Oral History Project
Interview with Vincent T. DeVita, Jr., M.D.,
conducted on June 5, 1997, by Gretchen A. Case
at Dr. DeVita's office at the Yale Cancer Center**

GC: Will you say your name for the tape recorder?

VD: I'm Dr. Vincent DeVita.

GC: Okay. And this is June 5th. It's about 2:30 p.m., Gretchen Case talking with Dr. DeVita at his office at the Yale Cancer Center.

The first question I usually ask people is how they came to the Institute, what your background was, your education, and what brought you to the Institute.

VD: Well, I went to Medical School at George Washington University in Washington, D.C. And curiously enough, the students and house staff at GW were not told about the NIH. At other universities, their trainees were often told to go spend a couple of years at the NIH; that was a way of spending time in the service and also being around some very elegant research in facilities that didn't exist in most places.

GW was a peculiar school. It's almost like it was afraid of losing its house staff, so I didn't really find out about it as an alternative to the draft until I went away on my internship at Michigan. In medical school, I spent a summer at the Mount Desert Island Biological Laboratory with the then-professor of physiology at G.W., Adrian Hogben. I

met Dr. David Rall and Dr. Gordon Zubrod there. They were both doing research at NCI. They told me about the NIH, and suggested I should apply.

I had actually done very well in medical school, in pharmacology, and Dr. Rall and Dr. Zubrod were basically pharmacologists . . . As a matter of fact, I remember opening my National Board grades on a dock in Mount Desert Island Biological Laboratory, and I got a rather high score, and they were impressed with me. One thing led to another, and I applied.

Then I finished school and went to Michigan. You had to come apply to NIH after your internship at that time, and I applied to the Heart and the Cancer Institute and was accepted in the Cancer Institute, although I had been interested in cardiology. I was strongly thinking of becoming a cardiologist and had actually done some research and published a couple of papers in cardiology.

Once I was in the Cancer Institute, I got hooked. So I went to the Laboratory of Chemical Pharmacology, as it was called at that time, with Dr. David Rall as the Chief of that Laboratory, and did my first work there.

We were called Clinical Associates at that time, and we used to spend a year in the lab and a year on the floor. The first year was on the medical floors in the Medicine Branch. In that year, I got interested in Hodgkin's disease, and then spent the second year in the laboratory.

Then I decided I was going to get another year of residency. It wasn't absolutely required, but I looked for residency programs. Dr. Rall did not think it was a good idea for me to do this. He wanted me to stay. I called Yale and Duke and applied for their residencies, and was asked to come for an interview at Duke, and did so. Then I was sitting in my lab at the NIH when I got a call from Dr. Paul Beeson, who was a famous professor of medicine here at Yale, and he called up and said, "This is Dr. Beeson at Yale," as if he had to tell me it was Dr. Beeson at Yale, and offered me the residency. I was really rather impressed and I accepted it.

What the NCI then did was to say, all right, we'll pay your way as a resident. Residents at that time probably made \$2,000 a year salary. I made \$1,800 as an intern. And I was making \$7,000 or \$8,000 in the Public Health Service, and so they offered to let me go on the NIH salary. It was really more than I could resist, and it was a way of having me come back, because I had to pay back.

I was going to stay at Yale for a fellowship, as well, but after I got here I realized that what I was doing at the NIH was so much more advanced in the cancer field than what was going on here at Yale, that I just decided to go back. I went back and became a Senior Investigator in the Medicine Branch, and then the Chief of the Solid Tumor Service, and then ultimately the Chief of the Medicine Branch, where I had done my early training.

I went to the College of William and Mary in Virginia, which is the place where all the NCI records are catalogued, in the library of the College of William and Mary.

GC: So you've already given your records to them?

VD: Yes. They wrote me some time ago when I was still Director of the Cancer Institute and said, "Why don't you send us your papers? You're one of our distinguished alumni," and so forth. And I said, "Well, that's very flattering, but frankly, the history of the war on cancer in which I am deeply involved is as important, certainly; would you catalogue the whole thing?" And they said they would.

When I was given the job as Director of the Division of Cancer Treatment and succeeded Gordon Zubrod, one day, probably around 1976, somebody walked into my office and said, "Well, we're cleaning out files, and we're going to throw these things out. We just wanted to know if you saw anything that you wanted in there." I looked at them, and I was quite stunned. They were all the memos and internal documents that had to do with the establishment of the Cancer Drug Development Program. And there were some very exciting exchanges of information. It was a very controversial program. There were people. A former Dean at Yale, for example, who's still alive and walks these halls who couldn't stand the thought of this going on at NIH and tried his best to stop it. Zubrod who was ordinarily a very distinguished person and a very careful, conservative person, exchanging memos dealing with this. There was a lot of blood-letting going on and I said, "My God, these are valuable; how can they throw them out?" So I kept them. And when the library wrote to me later and said, "We could catalogue your papers," I started by sending this whole bunch of files down.

And then I went through other files. I had an assistant who worked with me, who did the work on our text book, and I gave her the job of methodically going through NCI records,

copying relevant files that had to do with the Cancer Program, the Cancer Act, Mary Lasker, all the Chairmen of the President's Cancer Panel, anything that was relevant to the war on cancer, and gradually copied as many files as we possibly could and sent them down to the College of William and Mary.

I got a letter from them William and Mary only a year ago that they had now catalogued them so that they're accessible. I also have transcripts of Mary Lasker's oral history, as well. They're all really accessible. All I have to do is find the time to write a book on the war on cancer.

I've outlined the book, and I've done a few sample pieces. What they tried to throw away would have been irreplaceable information, because the Drug Development Program has been very successful, and it was Dr. Zubrod who really started and saved it.

GC: That's amazing that it came so close to being thrown out.

VD: The government is funny that way. They don't necessarily archive everything they have. So anything that was about to be tossed, I usually copied, and then I methodically copied a lot of other files. So I do have a lot of records.

At any rate, let me just tell you what I was thinking because I have a peculiar relationship with the origins of the National Cancer Program.

GC: Okay.

VD: When I was Chief of Medicine, and this was now 1970 I believe, I got a call from a doctor named Rita Kelly in Boston. She's now passed away. She was taking care of a patient, Colonel Luke Quinn. Colonel Luke Quinn, she explained to me, was a lobbyist for the American Cancer Society, and he had been diagnosed and operated on by Dr. Claude Welch, a very famous gall bladder surgeon in Boston, and was found to have "gall bladder cancer," which is a very unusual cancer. He was not resectable, and so he was obviously going to die of this cancer. She was starting him on the drug, 5-fluorouracil. He wanted to return to Washington because he had a lot of work to do and he wanted to be in his apartment, and would I take care of him? I said, "No, I don't take care of gall bladder cancer, and I'm doing research on lymphomas." I had just published the MOPP paper for Hodgkin's Disease, and we had published a very similar paper for the non-Hodgkin's lymphoma.

She said, well, she was sorry to hear that and that she'd find somebody else, and she hung up. Then I get a call from Sidney Farber, who the Dana-Farber Cancer Center is named after. Sidney was a hard case. He was a tough guy. He called me up and said I had to see this guy. Why they wanted me to see him, I don't know. But there weren't many oncologists around at that time in the Washington area.

But they put a lot of pressure on me; I also got a call from Mary Lasker. So I finally said, "All right, I'll see this guy." So they sent him down to me. He came in to see me, and I did a physical exam on him, and he had enlarged lymph glands underneath his arms. And I said, "This is crazy. Gall bladder cancer? Nobody has ever seen gall bladder cancer metastasize to axillary lymph nodes." And so we began to suspect that they had made a misdiagnosis, and they had. He turned out to have a lymphoma—could you

believe it? So we treated him with the right therapy for his lymphoma, he went into a complete remission, and he, Sydney Farber, and Mary Lasker were so astonished that it had in a way a very important impact on their decision to go ahead and have the Yarborough Committee commissioned to look at the whole issue of starting the war on cancer. Mary Lasker felt chemotherapy must have just come alive, and was ready for its participation in the war on cancer.

Colonel Quinn actually wrote the Act. He was a lobbyist for the American Cancer Society, but he really was part of Mary Lasker's organization, which she later explained to me, in much detail, which is also very interesting. And Luke Quinn really reported to Mary Lasker, and he really—though he was a non-government employee—wrote the Act for the committee that was doing this. That's how Congress worked then.

Not only did he write the Act, but he interviewed all the witnesses to be sure that they could testify in an appropriate way before the Committee. For example, he interviewed Tom Frei, and determined that Frei would not be a good witness, and so did not have Frei go on and testify in favor of the Act. He wanted a certain kind of person that he knew would carry over to the Congress and so forth. So he was an extraordinarily powerful man, it turned out.

He ultimately died of his cancer—he relapsed some years later, but not until after the Cancer Act was written and passed, and he had played a major role in this. And Mary Lasker was so impressed, that the people who had cured the first human cancers with drugs were nominated for the Lasker Prize and won. A whole bunch of us shared the Lasker Prize. Zubrod also won the Special Public Health Service Award in that year.

But it really came from Luke Quinn being treated, and folks being surprised that you actually could treat cancer with chemotherapy; it was so new at that time. And so there I was; a little bit involved in stimulating the birth of the Cancer Act.

I didn't like the Cancer Act at all when it was passed, like most people at the NIH. We all thought it was crazy to do this, leave us alone, let us do our research, and don't do anything, and so forth. And then subsequently I became the Director of the Cancer Program and I ran it, so it was kind of ironic that I had the hook-up in 1970, and then I became the Director of the Cancer Program ten years later; kind of interesting.

GC: What was your objection—what was the general objection to the Cancer Act?

VD: Well, we didn't like being told by politicians what we should do, and the idea was that you didn't buy ideas with money—*money does not buy ideas*. That was the standard thought. You can't just pour money into something.

Well, of course, that turns out to be totally wrong: money does buy ideas. And what money did with the Cancer Program, what it does whenever you put money into a program, is it attracts brilliant scientists who come into the field, they set their laboratories up, and they generate ideas.

You can't go out to somebody and say, "Here's a million dollars, give me an idea." But if you put that person to work, they generate ideas, those ideas generate new information, progress, and so forth. So I think the standard academic response was frankly the

standard academic response to a lot of things, that no, we're too above the fray to let this happen.

I don't know whether Dr. Zubrod told you, but during those early years of chemotherapy and drug development, the academic community just berated Zubrod and Frei and Freireich—"we" the next generation, got away a little bit easier.

I used to go to Atlantic City to the Clinical Meetings, and watch William Dameshek, a famous professor of hematology, now dead. He used to run a group called the Blood Club. Every year he would set up a meeting, the whole purpose of which was to embarrass Frei and Freireich. He'd get them into some panel discussion. He and his friends were so appalled that combinations of cancer drugs were being given to people, and that these guys would talk about "cure"—you can't cure leukemia. So as trainees, we used to go and listen to these guys [Frei and Freireich] getting beat up. They held their own, and of course, they were right, in that leukemia has been cured and all these other things have happened.

I grew up, medically speaking, in that environment. I was a Clinical Associate when Frei was the Chief of the Medicine Branch and Freireich was the Chief of the Leukemia Service, and both these guys had some very strong influence on me because I saw them defying traditional medicine.

I came to Yale after that and did my residency here, and people here at Yale made fun of these people openly. I would say something about, "we should give platelet transfusions to patients bleeding to death," and they would say, "Platelet transfusions don't work!"

And I'd say, "But I saw them work at NCI! I saw them giving platelets to people to stop bleeding!" "No, you have a short white coat on, you can't possibly know that kind of thing." That's why I left Yale.

As a matter of fact, the Head of Hematology here at the time, Stuart Finch, tried to persuade me not to send my patients to Paul Calabrese who was the first Chief of Medical Oncology in the country, at Yale. Finch tried to persuade me not to send my patients with Hodgkin's Disease to him because he'd give them "those combinations of drugs." He didn't even know that I was that guy who had started the Combination Program on Hodgkin's Disease. So I just said, the hell with this, and went back.

That was the academic environment. The academic environment often reacted negatively to new advances in the cancer field. So they didn't like cancer drug development. Zubrod fought the battle almost singlehandedly, using Frei and Freireich as people to get the job done, but they didn't have the smoothness that Zubrod had. Zubrod was a very distinguished person in everything he did—Frei and Freireich were considered wild. They were doing all kinds of things. They were enthusiastic. They actually said, "cure," and things like that.

Even so, when the Cancer Act came along, we were saying, well, you know, we're going to be told by politicians how we're going to do our science, and we know you can't buy ideas with money and so forth, and then gradually everybody came around. I think Frei and Freireich and myself and Zubrod all ultimately bought into it.

But in the beginning, I can remember the reaction to it. It was standard at the NIH to be against it. The NIH Director was against it. And anybody up the line from the NIH was against it. Nobody wanted it. Mary Lasker, almost single-handedly, pushed this thing through. And she's a really great lady, and she really should get the credit.

GC: Did you know Mary Lasker fairly well?

VD: Yes, I did. I got to know her very well. Subsequently, when Zubrod retired, I was appointed Director by Dick Rauscher—his name was Frank Rauscher, but we called him Dick. He was a Ph.D. virologist. But he didn't know squat about anything clinical. He appointed me over a lot of Associate Directors in between, which was a problem.

I was, however, a pretty well-known clinician even then. I think he wanted somebody around him who would give him clinical advice. So I spent a lot of time around Dick, and I testified in Congress for Dick Rauscher.

But Mary Lasker had an organization—I'll tell you about it when we get to the appropriate point—but one part of that organizational process was her annual trip to Washington during the appropriations time. She used to stay at Deeda Blair's house. She would have lunches at Deeda's house and she would go down to the Congress and visit various people who had been marked as friends or foes for her by Luke Quinn and other people in Congress. She would try to convince people both at the lunch and during those visits that there was progress being made and she would ask for what we thought were outrageous amounts of money. She would get half, usually, of what she asked for.

But she needed somebody with her. Rauscher couldn't go with her because he didn't know anything about clinical things, so he'd send me. I was constantly going to luncheons at Deeda Blair's house and going to the Congress. By the way, it was probably wrong for me to go to the Congress because I was a government employee. I should not have been down there lobbying the Congressmen to give us more money than the President asked for. We did it, but later on I realized how irregular it was for me to be going around the Executive Branch. Mary was the one that was going, she said, but I was there for information purposes.

During those times, I would get some time with her, and one day we went to see a Congressman who was identified as not a friend of the Cancer Institute and Cancer Program, and he made us wait. He made us wait for forty-five minutes to an hour. And during that time, Mary sort of opened up on this—she explained to me how she had set up her organization. It was extraordinary, an extraordinary piece of work this woman did to exert her influence on biomedical research in Congress. So I got to know her quite well, and I was very fond of her.

The first time that she came to see me as Director of the Treatment Division, I said to people, "I'm not going to be pushed around by Mary Lasker, so she can come see me and so forth." She came into the office, and fifteen minutes later I was in her pocket.

VD: She was just a charming woman, and she made sense. The thing that got you was that she made sense. If you wanted to be against what she wanted, you'd have to really go against logic, and it was hard to do. A lot of people did it, but I had a hard time doing it, so I didn't have any trouble, you know, sort of running with her.

GC: What was Dr. Rauscher like?

VD: He was a very interesting guy. He succeeded Carl Baker, and he made a discovery that related to a leukemia virus that was important, so he was a very visible figure. The Viral Cancer Program was a big part of the whole program, as well. It was a new initiative at the Cancer Institute.

He was, however, not a very good administrator. And he had around him "The Palace Guard," a bunch of people that sort of protected him from people like me and others getting too much of what we wanted. They really were the people who ran the Institute. He smoked, drank—in fact, in the end he drank quite heavily, and that was a major problem with him later.

He and the Palace Guard would go to lunch every day to a place called the Red Crown down on Wisconsin Avenue, and they would have two or three martinis and smoke their brains out and come back. We all knew that you couldn't go to Rauscher in the afternoon and talk serious business with him, unless you wanted to pull something over his eyes, because he'd had enough martinis that you couldn't deal with him on a serious issue. So we all saw him in the morning. And it was pretty well known.

And he hurt himself physically very badly, because he ultimately developed high blood pressure. He was only 62, I think, when he died.

But he was a nice person, and I think that he believed in the program. Mary Lasker, who had pushed Carl Baker out because he was not dynamic enough and wouldn't do all the things that the program wanted, was responsible in part for having Rauscher in the job, and I think he was very accommodating. But he was a Ph.D., and he was making only \$36,000 a year as Director of the Cancer Institute. At that time it was about an \$800,000 a year program, and he eventually had to leave to support his family, so he left and became Executive Vice President of the American Cancer Society.

Arthur Upton was appointed to be his successor. Now, Arthur was a pathologist, very quiet, and as you probably know because you've interviewed him, a very, very lovely man, but he hated the job. He used to tell me he died a thousand deaths every time he had to testify before Congress.

So he took me with him every time and, as with Dick Rauscher, I did most of the testifying. I also prepared his testimony and so forth. So I was almost Director for the Cancer Institute for an additional six years or so before I actually became Director.

Arthur and I were two very different kinds of people. One day he called me. I forgot what the issue was, but it was a very important issue. Something had happened and information had leaked out. On the phone I used foul language, and I said (at a minimum), "What the hell is going on up there?" There was silence at the end of the phone after I did this. He was in shock, I thought, because he was a very quiet person who would never do this sort of thing.

We got along fine, though. I adjusted to him and he adjusted to me. He only stayed two and a half years. And after that, I was appointed Director.

One of the things I found out after I was appointed was that the secretary to Rauscher, then to Upton, and to me only for a brief while, Phoebe Dunn, used an unusual procedure in the Office of the Director. When the Director talked to anybody, Phoebe was on the other line taking notes. So they're around someplace. I may even have some of the notes that Phoebe Dunn took of every conversation.

When I found that out, I realized that when I had blurted out, saying "What the . . ." to Arthur, that Phoebe was taking notes and writing all this down on the other end of the line. And they never told anybody. She told me after I got there, because when I was starting to make phone calls, she would say to me, "You know, I'm on the other end of the line, and I have to take these notes." And I said, "Why?" And she said, "Well, because that's the way we've always done it with all the Directors." I said, "Stop immediately." From that point on it stopped. She said she did it because she had to know what commitments the Director was making and all that sort of stuff. This was because a lot of the Palace Guard used to make the decisions, and they didn't want him to give anything away on the telephone. So we stopped the practice.

And she smoked, like the rest of them. Because of the relationship to the Palace Guard, she would not do what she was told. I mean, there was a guy there who was an Assistant Director, and he was a good friend of hers. I didn't think very much of him, and I didn't like him representing the Cancer Institute. She would schedule him to represent me when I wasn't there. I said to her, "You know, Phoebe, it isn't good. I know the NIH

doesn't like him there, they don't feel very good about him and so forth, so I'd rather not, so let's make sure that we don't do that again." I would go away, and he'd be over there representing me.

We finally decided that the best thing to do was for her to go. I mean, having a smoking secretary in the Office of the Director, and who was very open about it—she'd put on a cigarette holder and smoke at her desk—was not good form. But Phoebe was recording a great deal of what went on between Directors and everybody else, and apparently it started with Rauscher at least, and went through Arthur Upton's tenure, and stopped when I found out. Nobody has ever done it since then. It probably was illegal to record conversations like that without having somebody on the other end know you were doing that. Maybe because it wasn't taped, you could get away with it.

GC: It's interesting, though.

VD: I liked Dick Rauscher a lot, and I felt sorry for some of the things that he did; he was not a strong Director. But it was mostly the Palace Guard who ran the place. And when I got there, I had to deal with the Palace Guard, and I eventually just replaced them all.

I hired Dr. Jane Henny as my special assistant. We used to joke about the fact that the elevator was her office. She was in my old office as special assistant, and then she used to come up in the elevators, so I said I was going to put a desk in the elevator, and you can just go up and down in the elevator.

Later she became the Deputy Director of the Cancer Institute, and she's now Vice Chancellor, I think, at New Mexico University School of Medicine. She's also been Deputy Commissioner of the FDA. She was the person I relied on, and we gradually replaced all the other people with a whole new cast of characters, and set up a new system of governance through an Executive Committee that exists even today in a slightly different format with Klausner. It was a very effective way of governing and a more rational way of making decisions than had been done before.

GC: While we're talking about this, how did you become Director? Were you in the right place at the right time? How did you come to their attention?

VD: Yes, I think you always are, in the sense, for these jobs. I had been Chief of Medicine who had made what turned out to be a fairly major discovery in terms of the use of combination chemotherapy for Hodgkin's Disease, which had followed on the heels of Frei and Freireich doing the same thing with leukemia, so I was very visible.

I was a reasonably articulate person, and at that time was very active. So when they were looking for somebody to replace Gordon Zubrod, I appealed to Rauscher, so there I was. I was now one of the Division Directors at a time when Upton was actually given the job. My name had come up in the search process, even then, but I was told I was considered a little too cocky—at that time I must have been. I got the job when I was 44, so I was probably about 41 at that time, which would have been pretty young for that job.

When Upton left, everybody knew that I had basically been running the Cancer Institute because he hated it. I calculated I was spending about a third of my time running the

Cancer Institute. When he left, I was made the Acting Director. I was Acting Director for about five months while they went through the search process. And this time they offered me the office . . . It was near the end of the Carter Administration when it happened. I was appointed by Jimmy Carter, and Pat Harris was the Secretary. And then in came Ronald Reagan. And the Republicans really threw virtually everybody appointed by Carter out. They got Fredrickson at NIH and everybody on up.

Elizabeth Dole was actually looking after both Fredrickson and myself in terms of reappointment in the Reagan Administration. Something happened, and Fredrickson got it. However, I survived.

But it was a tough time because two Senators began to hold hearings as they now controlled the Senate. First Paula Hawkins, and then Orrin Hatch. Hatch had a staff member who was basically trying to get me out of there. I had to prove I was a registered Republican. I had to prove I had contributed money to the Republican Party. And then Hatch and I became good friends, thanks to David Kessler, who was the only reasonable person on his staff at the time. I was reappointed by Reagan, and I was reappointed by Reagan a second time.

I made up my mind I was going to leave—after a while, having testified before Congress for Rauscher and for Upton and then eight and a half years for myself, I was really getting weary of that sort of stuff. And I left—I thought about it for a year or so before I left—and on August 19, 1988, I told everybody I was leaving, and I left by September 1.

By the way, I had copied Zubrod in that sense. Zubrod one day announced he was leaving, and "poof" he was gone. And he chose to do it that way because he didn't want to be a lame duck. I actually asked a special favor to muster me out of the Commissioned Corps very quickly, and they did. I announced I was leaving to go to Memorial Sloan-Kettering, and two weeks later, I was out. And Memorial had made a deal with me where they would put me on a six-month sabbatical starting September 1, 1988, and I would report to them six months later, which I did. But I had six months to sort of collect my thoughts.

Now, it was a very easy decision to make to go from Director of the Treatment Division to Director of the NCI because I had been doing a lot of it. It was not easy for me to go from Chief of Medicine to Director of the Treatment Division. When Rauscher offered me that job, I think I walked around the campus for hours every day for a month because the Medicine Branch Chief position was arguably the best job of its kind in the world. At that time we had 26 beds, enough laboratories, and we didn't have to apply for grants or anything. If you had one thing that limited you, it was between your ears. And you could just do new, adventurous things. And we did.

In the Medicine Branch, we had a group of five of us; myself, George Canelos, Phil Schein, Bruce Chabner, and Bob Young, who are all now very prominent people in the field. We were the group, the Medicine Branch. We started lymphoma programs, ovarian cancer programs, breast cancer programs, things that are still here today, and that group was very, very exciting.

And all of a sudden I'm being asked to become what was an administrative job, and became Director of the Treatment Division and Clinical Director at the same time. It was a very tough decision for me, and I took a long, long walk every day just to try to sort out—and then I decided that I would do it.

I was going through a very bad time at that time, as well. My son, in 1972, was diagnosed with aplastic anemia. I brought him in to see Dr. Ronald Yankee, and the diagnosis was made, and Yankee put him in a laminar air flow room, which he never left. He lived in the laminar air flow room for about eight years. He participated in the decision about me becoming the Director, advised me to take it, and died about six months later.

So when I was trying to make the decision to become Director of the Treatment Division, it was very early in the time frame of his illness that this was all happening. He had been diagnosed in '72; I was given the job as Director of the Treatment Division in '74. It was an awkward situation because he was now in a Laminar Air Flow Room in a branch that I was now the supervisor. There was a scandal of sorts actually years later when a woman doctor, who is now dead, who did not like how I handled the branch, got hold of a reporter, Judy Randall, and gave her a whole big story about how my son was getting special treatment. It was in all the newspapers all over the country.

When they made the decision to appoint me, he was still alive, so they knew the situation there, as well.

GC: Did you see yourself at the NCI as mainly a researcher or an administrator?

VD: Well, you know, I spent from '63 until '74 primarily doing research. We did a lot of good things in lymphoma that turned out to be very durable. It sort of became the foundation stone of medical oncology, combination chemotherapy in adult tumors. So that's the dominant period. From '74 until I left in '88, I was doing both. Because I had lived there all my life, I could stay involved in the research program. I did this by having people to work through. I had Su Hubbard, who I hired from Yale. She was the first chemotherapy research nurse in the country. We brought her down there, set that program up and established chemotherapy research nurses. She became a data manager for me so that when I was in the Treatment Division, she would organize the data and so forth.

Later I had Richard Fisher as an intermediate. I used to actually use both Richard Fisher and Susan Hubbard. And I got myself in a little trouble because this was a time that people started to think about falsification of data, and if you were too far removed from what you were doing, you were in danger of getting caught in something like this.

So I used to ask Richard Fisher for the same set of data that I would ask Susan Hubbard. I would get two sets of data, and then I could compare notes to be sure I was not getting misinformation. Then Fisher found out, got very angry with me, and accused me of not trusting him. He then left, Susan became a Program Director on her own, and I had Dan Longo, who was a person I had trained. He worked with me for many years. We all set up new studies, some of which are still going on. So I always stayed involved with someone, much like I think the current NCI Director and Harold Varmus were trying to do in their own laboratories when they became Directors.

It was easier to do if you had "lived" in the Institution. For example, it's hard for me to start my own clinical research program here at Yale because I haven't been here that long, I don't have the data base and so forth, and I have to push aside young people who need to advance to do it. So it was easier for me to stay involved in research as Director of the NCI than it is for me to do it here at Yale. And I miss that.

A lot of what I did administratively was research related. And the ability to be in contact with patients, to see what was going on, was very useful as the Director because you could get a human picture to what you were doing. When I got depressed because things were going badly in any way, I'd go over to the hospital and make rounds. People would say, "You must be crazy. You get depressed with administration and then you go over and make rounds with the cancer patients?" And I'd say, yes, because it brings you back to reality and you can see what can be done and what can't be done and so forth.

I had been offered Deans' jobs and was offered a chance to consider the Dean's job here. I've always turned them down, and very quickly. I've never had the desire to be a Dean. People said to me, "Well, you've been Director of the Cancer Institute, you've been Physician in Chief at Memorial Sloan-Kettering. Aren't those like Deans' jobs?" I say, "Well, there's a big difference. The big difference is that at NCI, MSKCC, and here, you're administering over the field that you grew up in." In other words, if you're making administrative decisions, it's always to support research of one sort or another in the cancer field. When you become a Dean, then you become the person who solves problems in the cancer field, the heart field, the orthopedic field, OB-GYN field, the ophthalmology field, the otolaryngology field. Then you become truly administrative

because you cannot know enough about all those fields to be able to make those kinds of decisions. But I knew a lot about cancer. Nothing ever passed across my desk that didn't involve a scientific decision of one sort or another. Building a building is a scientific decision because you have to build it for a purpose, not just a building that's an architectural experience.

The job I have now is more like that, in the sense that I still make decisions that are going to affect science of cancer, clinical decisions that relate to cancer. So I've always kept my administrative jobs in the cancer field.

And I enjoyed it. I was pretty good at it, actually. I have no trouble making decisions. I'm an open person, and after people got used to that, they liked it and trusted me, and I didn't have much trouble.

What got me after a while was testifying before Congress because I did it starting in 1974 and I did it until mid-1988; for almost fifteen years. It got old. I would go down there, and the Congressmen would say, "Dr. DeVita, it's so good to see you again. You've been around a long time, old friend," and all this sort of stuff. But they'd ask you the same questions they always asked, and I'd have to give them the same answer—you know when you're posturing. It's an interesting experience, and I met some very interesting Congressmen, and I formed a lot of opinions about members of Congress and how Congress works and so forth. But after a while, I said to myself, I think I should go.

By the way, there is a problem moving up the chain of a big Institution like that and being at the top. You really can't go back into the organization.

At a university you can. And that's what people do. They make Chairman of the department for five or ten years, then they give it up, and somebody else takes the Chair's job, and you're the professor and you have tenure, you do what you want to do.

But you can't do that at the NIH. Basically, the only way to do that is to stop at the level of the Chief of a Branch and stay there because then you can stay there forever, or a section Chief or something like that. Once you've been Director, it's very hard to get back.

Don Fredrickson did it, but he did it after a number of steps. He went from Director of the Heart Institute to Director of the NIH, and then he went out and became the President of the Howard Hughes Medical Institute. He left that and came back to NIH as a Fellow at the Heart Institute, which he's still doing, I understand.

I was a Presidential appointee, I had lived through Carter and two of Reagan's Administrations, I knew George Bush personally—I didn't have any reason to suspect that it wouldn't have been okay to stay with George Bush. But ultimately, at some point in time, as a Presidential appointee, somebody is going to appoint somebody else. So there's no security forever in that kind of a job.

I had thought about it for years, and I waited for the right opportunity, and when it came along, I said, okay, I'll do it. But I enjoyed it a lot, and it was a rare privilege, I think, to have a job like that where you can sit on top of your field and watch what's going on, and have some part in building the program, and so on.

GC: Was there any question in accepting the appointment when you got it?

VD: Not for that one, but for the previous one, yes. It was hard for me to take Zubrod's job, but not difficult at all to take the Director's job.

GC: What did you inherit when you came? You had already been working as Director, basically, as you said, under the—

VD: Well, I inherited a troubled program in many ways because there were a lot of things that had gotten old and bad that needed to be reshaped; for example, the Virus Cancer Program, which history will record as a very, very important program.

GC: I'm going to ask you this question again on the other side.

GC: Okay, we're on.

VD: In 1974, the Institute began to get bad press that the war on cancer was a failure, because Mary Lasker and people like Mary had promised that cancer would be controlled or cured by the Bicentennial, which was kind of silly. We knew she said it, but nobody really thought anybody took it seriously. The research programs were barely set up by then. So the Institute began to get hairy in 1974. When I became Director in 1980, the flow of funds had begun to slow down as a result. There was a scandal a week about the mammography program that had been set up, about the Virus Cancer Program, people giving money to their friends, and all kinds of investigations were going on. So I inherited

an Institution that was administratively in very bad shape. They had not set up any systems for governance, and I had to sort of reshape the entire program.

I actually wrote a few papers about it, and you're better off reading the papers than hearing me tell about it. It's called "Governance of Science at the National Cancer Institute." There are several papers about what we did because actually I thought it was complex enough to sit down and write it down in the form of a paper explaining to people what we were doing under the heading of the "Governance of Science at NCI."

I remember the title very well because Don Fredrickson didn't want me to use that title, he didn't like it. And I said I had to do it because that's really what it was all about.

So I reorganized all the divisions, I got rid of all the senior staff, and I brought in all new division directors. By the time I got through, I had reappointed almost all of the department chairs, and so forth. So it was a complete overhaul, including the establishment of boards, the Boards of Scientific Counselors for each of the divisions, again, which NIH didn't want us to do because we would have more boards than any other institute, but we were much, much bigger than the other institutes. And a process for reviewing contracts at those boards, because there was a problem with contract awards; people were giving them to their friends, and big lab chiefs like George Todaro had a \$13 million budget. This was the way they kept control of important scientists as supporters, by sharing the intramural largess.

I had actually known about this because I was in the Institute. So when I got in there, I cleaned the whole thing up. One scientist was so incensed that I cut his budget from

\$13.5 million down to \$3.6 million, and moved him out to Frederick, Maryland, that he left. It was a good thing because there was bloody warfare between the viral oncologists going on. The viral oncologists hated each other internally. They would go and contaminate each other's cultures at night.

GC: Oh, my gosh!

VD: Yes. I mean, this was bad, bad stuff for a very good program. So what it needed was a huge administrative clean-up, and we did it. We did it much the way Klausner is doing what he's doing now. We came in there and just took everything and changed it, brought in new advisors, new structures, everything. My *modus operandi* was based on the fact that it's harder to hit a moving target, so I kept moving. I actually kept a list of all the things I did, and I have it around someplace, just to keep track of how I changed things, including the papers that we wrote. It's very tedious for me to describe it to you, and I won't, because it would bore you half to death anyhow. But we really overhauled the Cancer Institute.

And what came out of it was an open way of doing business that people trusted. I took the scientists who were big critics of the Drug Development Program, Charlie Heidelberger and Henry Kaplan, for example, two brilliant men, both dead now, and put them on advisory boards. I put them on the Boards of Scientific Counselors that overlooked the Drug Development Program. People thought I was nuts. I said, "These are very smart men. I don't think they're angry at us because they don't believe in the quality of what we're doing, but because they're never let in." Once you put them on,

they were brilliant. I mean, they just did what they normally did, which was to give you the best ideas they had, help you renovate programs, and so forth.

I smile because Rick Klausner was sitting in that chair a week or so ago and we were joking about it. He is doing exactly what I did. I mean, you need to bring in people who don't have any old ties to programs, just sort of rip it apart, and start all over again.

But in the end, the product I got was a sense that you can trust me. I think the scientists tell me now, told me then, what they missed was that they had somebody they could trust, and that the system was open, and you could go into the system and argue your case, and that things were done on the table and not under the table, and so on.

I worked for fourteen months, seven days a week, twelve to sixteen hours a day. And I never went home, and neither did the people I brought in to do it. We had 1,100 contracts at the Cancer Institute when I took over. We went through every single one of them, one by one, for about a month, and made 20 percent cuts, closed others down and so forth, and reprogrammed over \$200 million in one year.

The Viral Cancer Program which I think is one of the greatest contributions the Cancer Institute has made to science—seven Nobel Prizes have come out of it—was considered a piece of garbage. And it didn't deserve that criticism.

I have a letter from Jim Watson who said, "Would you come up and give a talk? We're dedicating a lab at Cold Spring Harbor, and would you address this issue of why people think this program which has great science is really a piece of garbage?" And the answer

was it was a contract program and NIH does not like contracts—then or now—and not only that, but it was a contract program that was governed by people who did it behind closed doors.

Like I told Rick, there is a tendency to do things behind closed doors. In the end it will burn you. So even when they were doing good things, the fact that they were doing them behind closed doors and the perception that they were giving monies to their friends, which in some cases was true, or they're giving too much money to do a job, there's a big problem.

We converted it to grants. And it was very controversial with our own virologists who controlled the contracts. That's why they were upset with me. George Todaro, Ed Skolnick, who's now at Merck—these guys didn't like me very much because I dared to come in and tinker with their private property.

But frankly, we made it a better program, it cost us half as much, and again, people said, hallelujah, somebody is finally listening and doing it.

We did a lot of things like that. And it was an all-consuming job at first—two to four years—to get it organized. After that, it eased up a bit. By that time I had staff on board I could trust, and ran our divisions, and we made our decisions through the Executive Committee, we started the Prevention Program—there was no Prevention Program before 1984. We started the Chemoprevention Program, at that time, and a lot of programs like that that didn't exist.

I hired Peter Greenwald to be my Deputy. He came down and interviewed for the job. I offered him the job and he accepted. And as we were walking across the campus to show him the housing where he still lives on the campus, and I was talking about the reorganization of the Prevention Program and what I wanted to do, he said, "You know, that's a better job for me." I said, "You know, you're right. Why don't we give you that job." So we switched jobs halfway to his house, and he took that job. I had somebody in the program as Acting Director who wanted the job, but he was a very negative sort of person. The guy who didn't get the job hasn't spoken to me now for twenty-five years. He at one time was a friend.

So there is the fall-out of being Director, is that when you make decisions that have to be made, you lose a friend if you had a friend, often. It's sad, but it happens that way.

So Peter Greenwald came in and conceptualized the Prevention Program better than anybody I'd ever seen before or since. He is not in good graces with the current Director, and I don't know what's going to happen with him, but you shouldn't lose sight of the fact of what he did.

I regret that I didn't spend more time with Zubrod after I became Director of the Institute and call on him for advice. But it's kind of hard. You have to be careful of your relationships and so on. But I thought he was a great man, and there's nothing that he ever did near the end of his career that would ever compensate for the things he did early in his career. We owe him with the Drug Development Program for cancer, basically.

So I think Greenwald ought to get a lot of credit. I feel very proud that I brought Peter Greenwald there because I think he's made a major contribution. We have big prevention programs now all over the world that came out of that program.

We got rid of the Chemical Testing Program. There are political overtones to this, and I can't go into all the details—but there were scientists who were in league with members of Congress who believed that chemicals were the major cause of cancer and the carcinogen screen was all important. And they used every miserable tactic you can possibly use to influence the Director to fund these programs over other kinds of programs. I was threatened by them. So was Arthur Upton. He told me after the fact.

GC: He told me about that.

VD: I was threatened by Sammy Epstein. I was taken out to dinner and told in no uncertain terms that if I didn't cooperate, that I would be, you know—[draws finger across throat]—that would be the end of it. And what I did was get rid of the program.

I remember when I went to Dr. Fredrickson and said, "I'm going to transfer this program to NIEHS." It cost \$45 million, it had 80 people in it. That was a lot of money in those days. It still is. And he said, "You're nuts!" I said, "Yes, I'm going to do it and I'll get away with it because it's the right thing to do, it will stop us from hemorrhaging into this program." What they wanted us to do was keep putting every increase we got into this program. I said, "The hell with it. We'll get rid of it." He said, "You'll never get away with it." I said, "I'm going to propose it to the National Cancer Advisory Board, and I'm going to do it."

It was the only time he ever came to the National Cancer Advisory Board. He was an ex-officio member, but he never came. He wanted to see whether I could get away with it. We got away with it. One board member who was very much involved in it got very upset, and when the vote went the wrong way, he got up and walked out of the room and never came back. But we got rid of it. The atmosphere of the place changed overnight, virtually, because we no longer had this program bleeding us to death, and we gave it to—David Rall, who had brought me to the NIH, was now the Director of the NIEHS, and the program is no longer really in existence anymore. It's been taken over by better things.

But there were things like that, changing the Viral Cancer Program, getting rid of the Chemical Testing Program. I took the Drug Development Program—that was one of the reasons why I didn't do much with Zubrod; we took it from about \$90 million down to \$68 million because it was starting to do things that should be done by drug companies who are now making money on cancer drugs. So we cleaned that out. We changed the tumor screens. I had to can my best friend because there was a great deal of resistance to changing the screens we were using for selecting drugs because the people liked the old way—and the old way was no longer the right way to do it. We did a lot of work to decide how to do it, and then we changed it, I turned around and walked away, and the old guard slipped right back to the old way. And so I said to the supervisor, "The guy who did that, remove him and get somebody else." And he said, "I'll fix it, I'll take responsibility for it." So they went through the same process again, changed it again, he walked away, the guy did it again. So we got rid of all of them. He didn't speak to me either.

But it made the screen much better. Drugs like Taxol came out of the new screen, you know. The point is these were all things that had to be done, they made the Institute better, and I think this is where Rauscher had failed. He didn't know how to do this sort of thing. And then Upton hated it. It wasn't that he didn't know. He saw some of these problems. He tried to change the Viral Cancer Program. He got rid of John Moloney on Christmas Eve.

John Moloney was the guy who deserves a lot of credit for the good things, but he was also a guy who was a tough old Irish kid, who did a lot of back-room smoking and drinking and giving contracts, despite the fact he did wonderful things in that program that led to these Nobel Prizes, he wasn't trusted.

But firing him on Christmas Eve was not exactly a good move. Arthur Upton damn near did himself in on that, and it didn't fix the program because that wasn't what the problem was. The problem was the program needed to be converted to a different mechanism somehow.

So, I must say, I had fun doing these things. I don't mean the distasteful parts that come with every job, but I had fun changing the Institute and then standing back and looking at it and saying, now we're a complete Institute. You know, we had treatment programs where they should be, doing what they should do, we had the Virus Cancer Program doing wonderful things, we had a Cancer Prevention Program, we started Cancer Prevention Clinical Trials, we built the Cancer Information Service up. I started PDQ,

all those things were all part of that, and they were fun. But after all these years it started to wear on me a bit and that is when I decided I'd go.

GC: When you first came to the Cancer Institute in '63, did you have goals in mind? Did you think you were going to go all the way to the Directorship? Did you know what you were coming in to do?

VD: No, as a matter of fact, I always thought I would go onto private practice, actually.

GC: Oh, really?

VD: Yes. I came in as Clinical Associate. I lived in an apartment building, Lakeside Terrace—I think it still exists over there—they were brand new at the time. I remember talking to Steve Epstein at the Heart Institute. He and I were standing by the pool thinking this is great, you could make as much as \$10,000 a year being at the NIH. That was considered a lot of money in those days.

But I actually thought I would go ultimately into practice. But then once you're involved in a major discovery, you're hooked, you're finished for the rest of your life. You know, once we did MOMP and then MOPP and it worked, we saw what could happen, and you can't leave it. From that point on, I was never even tempted to go into practice. I would have never gone into practice. I see the compromises that are made in private practice, and I'm appalled. I wouldn't have survived in private practice. The influence of Frei and Freireich and Zubrod was extraordinary. These guys poisoned your mind for anything except the excitement that everything was possible. I came to Yale—I loved Paul Beeson,

the guy who brought me here. He's a great man, still is. He's 89 years old, and he was here a little while ago. We dedicated a unit to him.

Frei and Freireich violated every principle that Beeson stood for in using drugs, and it puzzled me. I told Dr. Beeson this. It's no accident he was a great man. I explained this to him, that I was struggling with it. They were telling me to do things that I heard that he wouldn't allow. And he said, "Let me tell you a funny story. I made rounds on a visit to India." Beeson was an infectious disease expert. And he said, there was a ward full of typhoid fever patients. And he said, "The resident knew who I was"—obviously was awed that Professor Beeson, the great professor and the editor of the major medical text, was there and was going to take him around and show him typhoid fever. There were dozens of cases on the floor.

And he got to the first case and he said, "Here's a typical case of typhoid fever, rose spots, all these things," and he said, "This next one is an atypical case, and the next one is an atypical case, and the next one is an atypical case." Then he got to a typical case. And then he went through another ten atypical cases. And so Beeson, being a logical guy, said to him, "We've seen fifteen cases, and three typical cases. How can that be?" He said, "Well, Dr. Beeson, the typical cases are the way you described it in your textbook." And Beeson said, "Do you know how many cases of typhoid fever I have seen in my entire life? Three, before I came here today." So this guy was seeing every day many more cases of typhoid fever than the great professor had ever seen. The great professor didn't claim to see more. The resident still called the typical cases—you know, the minority cases were "the typical" and the majority "the atypical." He didn't see what his own eyes were telling him.

That's what Freireich and Frei taught me basically. There was no such thing as unquestioned truths in medicine. When you looked back to the truths that were being passed on, they were often silly, they were based on no information, they were based on practice and tradition, and so on, and had nothing to do with reality. They taught you to violate every rule known to mankind if you thought you were pursuing something that was worthwhile.

And they were considered a crazy team, Frei and Freireich. They were erratic people on top of being scientists who thought up different things. They had a party every year in the fall. As a Clinical Associate, I arrived in July and by September went to the boss's party. I walked in with my wife, and it looked like some place down in Hades.

The first thing I saw was Tom Frei walking out of a room with one of the technicians slung over his shoulder. Tom Frei could walk on his hands—and the next thing I see is Tom Frei walking on his hands.

Then there's J Freireich pouring everybody martinis, and ultimately sitting on a table and collapsing the table and getting drunk and falling into the bathtub.

Nat Berlin, who was then the Clinical Director, asked if I would help carry him to his car. So we carried him to his car, and Nat Berlin (who was a little guy, so I was carrying most of the weight) kept mumbling over and over and over again, "We'll never be able to make rounds with clinical associates again."

VD: And we dumped J Freireich in his car, his wife took him home, and the next morning I went to the hospital early because I had sick patients, and there was Freireich walking around the hospital doing all these things.

And my colleagues and I—Jack Moxley and George Cannelos—used to go to this one meeting they had every week. We referred to it as the "Society of Jabbering Idiots." And we would actually goad J Freireich, because we thought we were on the side of the angels, because we came from academic institutions and J was widely thought to be bizarre. This went on for about six months. In fact one time I was told by J later that J thought I was going to punch him at rounds one day. We had gotten in some argument and he said, "I thought DeVita was going to punch me."

But about six months into this thing I said to myself, "Jesus, you know, this guy is always right!" And it was really quite astonishing. They had been being portrayed as crazy by the standard academicians. But these guys were right. They were breaking new ground. People were going into remission. I'd never seen that before. Nobody ever told me you could do this sort of thing.

Frei wasn't much with patients, but Freireich was one hell of a doctor. I would order a test on a patient, and before I could find out the results, I'd get a call from Freireich telling me what the results were and what I should do. He was always on top of everything. So they were in the end extraordinary people.

There is a guy, Evan Hersch, who put together the recent festschrift for Freireich, at the M.D. Anderson. I was on rounds one day with Freireich and Hersch, and Freireich called him a murderer—called him a murderer! You know, he had neglected something on a patient and Freireich in front of everybody called Evan a murderer. Evan cried, just broke down in tears and cried.

But a few months later Evan was irretrievably in Freireich's camp as one of his students, and Freireich is still his hero. We all were brought in, told they were nuts, urged to attack them and disparage them. In the end you realize you're dealing with great men and you accepted it. And their legacy is a very strong legacy.

Once you get into that environment, it's very hard to change. So there's no way any of us were going to leave any kind of environment like that.

At that time, there were very few places like the NIH anyhow. Now universities have become much more flexible.

But I credit them with changing my attitude in a way that made me the kind of person I am. I found for many, many years after that, at the NIH as Director, I always considered part of my job as providing an umbrella for adventurous people. For example, when Rauscher appointed me Director of the Treatment Division, he called me in his office one day and he said, "There's a problem here." I said, "What?" He said, "Freireich is losing all his grants"—Freireich had now left and gone off to M.D. Anderson—"He's losing all his grants. People think he's nuts. They don't fund him because they don't like his personality. Would you look over his grants and see what can be done?" Later we

started the Outstanding Investigator Grant, which is a seven-year grant that was peer-reviewed through the mail. It was not reviewed person-to-person, and that was partly because of the experience with Freireich, because people would sit around the table at the study section—I would go and listen to them review it, and they would say, "Boy, that's a good project. Boy, is Freireich a maniac! Yeah! Yeah!" and the scores would go "poof"; nothing to do with science, they'd just do it because someone considered him a maniac.

But it was Rauscher who said to me, "Do something to protect him," and I did. I give Rauscher a lot of credit for realizing that however bizarre somebody can be, if they're doing good science, you should let them do their good science.

There's another guy who was strange, who made some contributions, Isaac Djerassi. Are you going to interview Isaac Djerassi?

GC: No, but I know that name.

VD: D-j-e-r-a-s-s-i. But Rauscher said, "Djerassi is losing his grants, too. Go figure out what the hell is it." So I said, "Okay, I'll go see him." So I went to Philadelphia. I'd met him; we shared the Lasker Prize together. I sat down with him, and I said, "Isaac, I'm here to try to figure out how the hell we can prevent you from losing your grants." He said, "I don't know what it is, Vince. I did all these good things. The site visitors came to me and"—complained something about methotrexate, which was a drug he was working with. And he said, "I said to them, I AM methotrexate!" and I said, "Oh, God! Alright, Isaac. The solution is never submit a grant that requires a site visit because if

you're going to do that face-to-face, you'll never get a grant, it doesn't matter how good it is." So his future grants were all [?] so that he would be reviewed without needing a site visit. And he kept his grants for a while.

But this is part of the background noise that you don't hear about the process. I mean, grants are lost by good people who tend to be a little out on the edge of the envelope. And sometimes they're lost even though the science and the quality of what they're doing is extremely good. And sometimes people get grants because they stay within the center of the field and they're rock-ribbed conservative, quality scientists. They get funded for years and years and years, and they haven't made a major discovery in their lives.

Freireich was responsible for platelet transfusions, for laminar air flow rooms, for the first combination chemotherapy for leukemia, for God knows what else. I mean, this guy has made a huge contribution. When you talk to most people, they still think he's nuts.

GC: Really?

VD: Zubrod liked J and backed J. And Tom covered J a lot. Tom and J are very close, but Tom was a bit more of a politician. When J got in trouble, Tom would go in and smooth the waters a little bit. And then Zubrod would come in and protect them. And that's where I learned to be the umbrella for people who needed protection.

It's not usual to find brilliant scientists who are totally well-balanced, lovely, wonderful people. My standard is George Vande Woude. He's the head of the NCI Basic Research Program in Frederick, Maryland. George is an absolutely delightful, nice

person who never caused trouble in the entire time I've known him, and he's a brilliant scientist. So I told him about this. I said, "George, you're my standard. I judge people on Vande Woude units."

So this is partly what you do in these jobs. Your job is to make sure that good things happen to good people.

And I supported people, by the way, that I didn't like very much. There are a couple of scientists who I still don't like but who are very well-positioned today because of the support we gave them. And I think there are examples of Directors who don't do that, Deans who don't do that. In other words, if you like the person you support them, if you don't like them you don't support them. That's bad. You support good things, and you don't have to love people to support them.

My mother thought I was a slow student really because my mother was very traditional. A doctor went to medical school, bought a Cadillac, had a black bag, and made house calls. I kept going to another institution. She thought it was taking me a long time to graduate.

VD: I just had to explain to her that it was really good being at the NIH, this was a very important place. "When are you going to go into practice? Why are you driving a Volkswagen?"

GC: So Hodgkin's—were you handed Hodgkin's and told "work on this," or did you choose Hodgkin's?

VD: No. There was a patient that I took care of when I was a resident at D.C. General Hospital who had Hodgkin's Disease. He ultimately became a psychiatrist. And he failed and died of Hodgkin's Disease while I was at the NIH. He followed me from D.C. General to the NCI. So for some reason because of him I got interested in Hodgkin's Disease. Jack Moxley, who was a clinical associate with me, had a similar experience. We were both sort of interested in Hodgkin's Disease.

We came into this environment that I described to you where the combination chemotherapy of leukemia was brand new and Frei and Freireich were talking about curing it and so on. And we were taking care of Hodgkin's patients, and we couldn't see why the same principles didn't apply to Hodgkin's Disease.

We put this stuff together called MOMP, M-O-M-P, and went to Tom Frei, and said we wanted to do this. Paul Carbone, who was for many years my immediate superior—one of the persons I jumped over when I became the Treatment Division Director—Paul Carbone had a fit. He said, "You can't let these guys do that. They're very junior, and besides we have another study going on in the Cooperative Group," and so forth. To Frei's credit, he said, "Let them do it." And he said, "They have to answer their own phone calls and they have to take the referrals themselves and everything, can't use the resources of the office," and so forth. And so we put an advertisement out for it—we wrote the protocol—I have the original protocol—and we took fourteen patients in on the MOMP Program and we treated them. We agreed in advance I would be the presenter of

the first abstract, Moxley would be the senior author on the first paper when it came out for MOMP, and Frei was in the middle of this. Carbone was not a part of all this original study.

And then I had been working in the lab on a drug called procarbazine, then known as ibenzmethazine. It was unique in the sense that it seemed to be almost only effective in Hodgkin's Disease.

Ron Yankee and I were also working on the mouse, model L 12 10 leukemia, looking at the kinetics of the marrow as well as the kinetics of the tumor. All this was aimed at trying to come up with the proper schedule to administer drugs to humans.

We were using weird schedules in those days. For solid tumors you gave drugs by mouth every day for six weeks and then you stopped. Nothing good ever happened when we gave drugs by mouth every day for six weeks. So we converted the solid tumor into the leukemia-type approach by putting together cycles of chemotherapy timed on the basis of bone marrow recovery rates, and, using principles developed by Howard Skipper in the mouse for combination chemotherapy, and when we had enough experience with procarbazine, we switched into MOPP and called it Combination 2. We weren't clever at naming things. It was Frei who named it MOPP.

GC: Oh, really?

VD: Yes. They'd put together VAMP and BIKE, and because these acronyms were quick, people could remember them. But I called it Combination 2. And so Frei said, "Right. It's MOPP." And it's been forever MOPP.

In fact, I've had a little jousting with Frei about names over the years. Every time he names something, it's usually scientifically incorrect. Okay? He named chemotherapy that's given first as the treatment for localized disease, Neoadjuvant Chemotherapy. The proper term for it is Primary Chemotherapy. So I put Primary (Neoadjuvant) Chemotherapy in papers. And I'd even write a paragraph or two about how Neoadjuvant is not the correct term; Primary is the correct term. I'd lose every time. He's got this gift for picking the name that sticks, so no matter what I do, his name always sticks. And he named it MOPP.

And so we began to admit patients for the MOPP Program. It was quite obvious right away that MOPP was good. Only one patient was put on the MOPP Program while Tom was at NCI—he left to go the M.D. Anderson. Sy Perry then took his job.

Perry kept saying to me, "Publish it! Publish it!" I said, "I want to wait for survival data." Because in those days, we didn't even have survival data or relapse-free survival data on solid tumors because nobody ever had anything that worked. So I waited for about four years. In 1967 I published the abstract for cancer meetings. And it caused quite a scene.

I remember being torn apart by David Karnofski, a chemotherapy guru, because I dared to use terminology from leukemia like "complete response." It had never been used in solid

tumors because it didn't occur. They were very upset that I had usurped their terminology. It was really sort of an awkward experience.

Tom Frei ran something called the Lymphoma Task Force, established by Gordon Zubrod, and he asked me to make a presentation about MOMP to the Lymphoma Task Force. He brought all the moguls in and went around the table—it was very clever. He went around the table and asked everybody to tell them what they were doing. And they were all doing the same thing, comparing Drug A to a similar Drug B, and that sort of thing. And then he said, "Vince, why don't you tell them what we do"—this is when the MOMP Program was going on—and so I presented it. A very famous professor, Wayne Rundles, now dead, actually turned pale, and he raised his hand—I was only about three feet from him—and he raised his hand and he said, "Dr. DeVita, do your patients speak to you when you do this to them?" It just totally rattled me because I was just a young whipper-snapper.

Many years later he invited me to give grand rounds at Duke, where he was professor, on Hodgkin's Disease. And I said, when I got up, at the end of my talk, I said, "Oh, yes, Dr. Rundles, you asked me some years ago whether my patients speak to me when I do this to them, and the answer is 'yes, for a lot longer.'"

GC: Good for you!

VD: But we were friends after that. He didn't believe that our initial research was real in the beginning, and we were forced to do in one of those cooperative groups, randomized comparisons between his drug, nitrogen mustard, versus MOPP. And after it was

over—MOPP proved superior by a good margin—he said to me, "Why did you do that study?" I said, "We did it because you told us that the single agents were really just as good." "Oh, I did? Okay."

Anyway, it was very clever the way Frei set it up. But there was a lot of resistance to it because this was a radical departure from the standard way of treating solid tumors.

And then when the paper was published in 1970 in the *Annals of Internal Medicine*, it is to this day the most cited paper in the history of the *Annals of Internal Medicine*. And it's in the top few most-cited papers in the oncology field. So what it did was strike a chord. There was an absence of any kind of therapy like this.

While we were doing this, we did the same thing with diffuse large-cell lymphomas and reported it in *Lancet*. We called it *The Cure of Advanced Diffuse Large Cell Lymphoma*. And people again thought we were a little bit crazy, but I had learned this from Frei and Freireich.

And so that's how it happened. I was in the right environment, we had the model of acute leukemia, we had Skipper's work going on, and then we did the necessary legwork to create the information on the schedule and so forth. We did it, and Frei provided the cover.

Well, Freireich will tell you the story about choriocarcinoma. It was the first adult cancer that was ever cured with drugs. It was a rare cancer. And the guy who did that was a man named M.C. Li. M.C. Li was also a strange bird. I met him on a number of

occasions before he died, but I never got to know him very well. But J will tell you he was kind of weird. For J to tell you he was weird, he had to be pretty weird. J used to tell me, "He's not the kind of guy you'd want to go to to have your cancer taken care of."

But for reasons I won't go into in detail, he got this idea about methotrexate in choriocarcinoma because he had seen something happen in another tumor. And he gave methotrexate in an entirely different way than anybody ever gave it.

J Freireich, went to M.C. Li and said, "You know, M.C., you're crazy; you don't give it that way. Trust me. I know how to do it. You give it every day by mouth," blah, blah, blah. M.C. said, "I'm going to do it this way." And the first three patients he treated went into complete remission. One of them until recently was alive and died of old age. Those three people were cured.

After the first patient who went into remission, there was a Grand Rounds held at the Medicine Branch, and Gordon Zubrod was the speaker, and the subject was "Spontaneous Regression of Cancer." They didn't believe a patient went into remission with methotrexate—it must have been a spontaneous remission; nobody ever saw this before.

Roy Hersh, who was the chief of the Endocrine Branch, called him in and said, "You can't do that anymore." He said, "If you want to do that anymore, you're going to have to give up the Chief of the Service position." So M.C. said, "Okay, I'll give it up."

He treated a dozen or so patients, and the results were the same—toxicity was fairly severe—and they fired him! They warned him that he just can't do this kind of outrageous thing, so they fired him. And he went to Memorial Hospital.

Subsequently, when the Lasker Award was given for Hodgkin's Disease, leukemia, and choriocarcinoma, Hersh shared the prize with Li. In fact it's called the Hersh method of treating choriocarcinoma.

I've never been around Freireich when that's said that he doesn't get up and say, "The Li-Hersh way"—he knew it was M.C. Li who did it and got canned for doing it.

The interesting thing is the first cure of testicular cancer was also done by M.C. Li, with a triple-drug combination—it was not a high cure rate, it was about a 20 percent complete remission rate, but those patients lived free of disease. He was fired from Memorial Hospital for doing that, and then went into practice in Minneola, Long Island, where he just practiced Oncology and then retired in San Diego, and has since died. He did get recognition when he shared the Lasker Prize, but he was fired by the guy he shared the prize with.

Once we were really certain that we had something unique, we put together an array of studies using MOPP and a different set of circumstances, and stuck to them. We just did them—we ultimately wound up randomly allocating early-stage patients to either MOPP or to radiotherapy, a study that had many ethical problems at that time. It took us ten years to complete it. But we stuck with it, and so the use of the MOPP Program represents a continuum from putting it together and testing it for the first time in

advanced Hodgkin's disease, looking at maintenance therapy with MOPP, reporting the side effects of chemotherapy.

So you look at MOPP, and you see a package of studies that we put together over 29 years ago. It takes a long time. I use the development of MOPP as an example that if you want to be a clinical investigator, you have to be patient and live a long time. Because 29 years is longer than most people stay in an institution or a field.