

Upton, Arthur C. 1997

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National Cancer Institute Oral History Project

Interview with Arthur C. Upton, M.D.

conducted June 4, 1997, by Gretchen A. Case

at Dr. Upton's home in New York City, New York

GC I usually start out by asking what you did up to the point that you came to the National Cancer Institute.

AU Let me begin then by going back in time. I graduated from the Medical School at University of Michigan in 1946 thinking that I would go into internal medicine. As a little boy, I watched my mother nursed through pneumonia in the days before antibiotics. She was quite ill, and in those days, pneumonia was often a fatal disease. And I saw a physician come to the house each day with his little black bag. And she survived. I think she was one of the first people in Ann Arbor, where we grew up, to have the sulfonamides. But she survived. And witnessing the contribution that her physician could make to her life and to the life of the family motivated me to want to be a physician. So I went to medical school and discovered after I graduated that although I enjoyed working with patients, helping people, I felt all too often impotent when it came to trying to intervene in a way that was really useful, so that after my internship I determined that I would be happier in the laboratory trying to understand the diseases better so I would be able to prevent them or treat them more effectively. So I left the bedside, went into pathology, took a residency at University of Michigan Hospital, and at the end of my residency had the opportunity to do some full-time research at Oak Ridge, the Oak Ridge National Laboratory. That appealed to me very much because I was really interested in research, and in those days the NIH was really not well-developed at the time. The only way one could do research was either if one had an independent income or if one could sandwich it in somehow, nights or on the weekends, between one's paid job duties. The opportunity to go to Oak Ridge and work in the National Laboratory full-time in research *on a salary* was too good to pass up. So we moved from Ann Arbor to Oak Ridge. And I thought I would be there for a couple of years, then back into a medical school somewhere as a member of the faculty. But I got involved in very exciting experiments, and two years became nearly two decades. So we were there eighteen years.

And as I got into my forties, I realized that if I were ever going back to medical school, which had been my ambition, I shouldn't put it off indefinitely. Then the chance came to go to Brook, the State University of New York at Stony Brook, which was starting a new health science center. And I had the opportunity to go there as the Chairman of the Department of Pathology, and was inveigled into serving on the committee to help select the Dean for Basic Sciences, and eventually was inveigled into actually moving from the Chairmanship of Pathology into the Deanship, which I did. And that was exciting because I had the chance to recruit a faculty and build a school—there were no buildings, no students when I went there—

GC Oh, really? Just nothing, just an idea?

AU Nothing, an idea. And it was very exciting to work with the Vice President and other deans in shaping a program. We put up buildings, hired a faculty, recruited students, and got things going—the School of Medicine, the School of Dentistry, the Schools of Nursing, Allied Health Professions, and Social Welfare—so that was very creative.

But after that all came to pass, toward 1975—I went there in '69—it then became more of a managerial task. The most creative aspects had been accomplished. I decided then that I really didn't want to manage the thing, I wanted to get back in the laboratory and return to research. So I resigned my deanship and went to Brookhaven National Laboratory on a sabbatical, then back into the Department as Professor of Pathology, and then I received a call one day from Ivan Bennett who said that he was Chairman of the Search Committee for the Directorship of NCI, and would I be interested. Well, my career really had been spent looking at mechanisms of carcinogenesis, the effects of radiation and the effects of chemicals that resembled radiation in carcinogenicity. So I thought, well, I'd served on committees, advisory committees to NCI, and that appealed to me very much. So we went to NCI in 1977. And that was a very exciting opportunity. I enjoyed working with colleagues there, working with various panels, boards, committees, that support the work of the Cancer Institute. But the job, I quickly discovered, was more politics and administration and less science than I'd expected. And I'm basically an academic person, a research person. So after about six months I realized that to do justice to the demands of that job, I couldn't go to seminars, I couldn't stay abreast with literature that I was trying to follow. I was like a tree pulled up out of the soil, my roots were drying up. It wasn't science, for me. And so I realized I wouldn't be able to stay there indefinitely. And the opportunity came to move back to an academic post at New York University, and I took that job. Long answer to a short question, but that's the background.

GC Ivan Bennett, you said, contacted you?

AU Ivan Bennett was the Vice President and Provost for Medicine at NYU. He was the Chairman of the Search Committee that had been formed, I guess, by Secretary Califano, the Secretary of HEW at the time. And it was he who contacted me at Stony Brook. And it was he who brought me back to NYU, actually.

GC Oh, really?

AU Yes.

GC Do you know why you came to their attention, or did you expect this kind of honor?

AU I had, as I say, served on a number of committees through the years, advisory committees to the National Cancer Institute. I had been involved in cancer research, particularly with radiation, but also to some extent chemical carcinogenesis, had served on advisory committees for the American Cancer Society. I'd risen to prominence, so to speak, in the field of cancer research. Perhaps that's why they went after me; I really don't know.

GC Were you surprised when they called or when they contacted you?

AU I think I was, yes. I don't recall—that goes back, golly, more than twenty years, about twenty years now, 1977—I don't remember whether I was aware that my predecessor had left the Directorship. I think I followed Rauscher. I may have known that Dick had left the Directorship—I don't recall. But it was a very appealing opportunity, and I'm very happy that I had the opportunity to serve as the Director. It was an educational experience. I never doubted that I was doing something useful and important. It was just that I was, to some extent, a square peg in a round hole.

GC Sure. Did you know Dr. Rauscher at all?

AU Yes, I worked with Dr. Rauscher, as I say, through advisory committees. And my research on radiation leukemogenesis led to me want to know more about his work with viral leukemogenesis, the Rauscher Leukemia Virus. So we had met one another. We had probably spoken together. I followed his work very closely.

GC What kind of person was he? How would you describe him?

AU He was a warm, down-to-earth person, very companionable individual. I didn't know Dick terribly well. But on those occasions that I was with him, I enjoyed being with him. He was good fun. And he was a thoughtful and knowledgeable scientist, a substantial person.

GC There was an Acting Director in place when you came in. Is that right? Guy Newell?

AU I think so, yes. I remember Guy Newell, and he was, I believe, Acting Director. And he became my Deputy.

GC He became your Deputy?

AU I believe so, yes. That's my recollection.

GC What was that transition like? When you got to the Institute, what was waiting for you? What projects did you first have to pay attention to, or what issues did you first have to pay attention to?

AU My recollection is that it was a smooth transition. The members of the so-called Executive Staff were cordial and supportive. I don't recall walking into any serious organizational or administrative problems. If there were problems, I didn't have any dumped on my lap the day I got there. One of the issues that did arise fairly soon after I arrived on the scene had to do with concerns in the scientific community. And actually, I was made aware of this when I became known as Dr. Rauscher's successor, before I was actually there. People were writing to me and phoning me, making me aware of their concerns. The level of peer review, the quality of peer review, perceived to be given to NCI-supported contracts, did not measure up the peer review that was given to grants, investigator-initiated grants. And there was concern that a small number of planners at NCI were dealing out sums of money to their buddies and making awards that really wouldn't have been made had those allocations been subjected to the rigorous sort of peer review that investigator-initiated grants received. I was not really in a position to judge the validity of those allegations. But I felt obligated to investigate the matter, and we did do so, and this led us to conclude that there really needed to be a separation of authority between those members of the Institute staff who supported intramural research and research funded through contracts, and those who were responsible for investigator-initiated grants. There was a potential conflict of interest. And so we set about to reorganize the Institute, so to speak. I think that was a somewhat unpopular move in the beginning. A great many people argued that this was not justified, would be counter-productive. And I can remember even, not being dissuaded, but being urged to go cautiously, by Benno Schmidt, who was the Chairman of the President's Panel, and by Paul Marks, who was a member of the Panel. We did go ahead and make the move, and almost immediately thereafter, everybody seemed delighted.

AU It's like pulling a sore tooth, you know, you hate to go through it, but once you get rid of it—

GC They were fine.

AU —you feel better.

GC Right.

AU And I'll never forget, there was all sorts of apprehension before the decision was made, but after it was made, everybody looked back and said, well, it was obviously the thing that had to be done.

GC That must have made you feel good.

AU Well, that did feel good, yes. But I think this came at a time when the budget was not growing as fast, and I think in retrospect, it probably was a good thing to do, it was the right thing to have done. I do think that there is a place for central planning, but I'm also convinced that the collective intelligence of the scientific community as expressed through investigator-initiated proposals is a powerful force, and there needs to be an appropriate balance between the two. And prior to the reorganization, that balance may not have been ideal. It may never be ideal, but I think we moved in the direction of a better balance. Aside from that problem, I don't remember other major issues that I had to deal with. Well, there were concerns—always had been, always probably will be—about the balance of effort across the different research thrusts. There were those in the public health community who considered the emphasis at NCI to be misplaced and that so much effort was going into research on treatment and so seemingly little into research on methods for preventing the disease. I can remember one amusing confrontation. I was sitting in my office one day and Richard Peto came to see me. Richard is a very gifted, accomplished, epidemiologist from England—a very bright person, but also outspoken. And he berated me for not putting all of our money into tobacco.

GC Really?

AU He said, "We know that cigarette smoking is the leading avoidable cause of cancer in this country. You ought to take your money and spend it to do something about that major problem." It didn't seem to faze him that we had other things to worry about, too. We knew that cigarettes caused cancer. We were a research organization, and not a public health laboratory. But it was amusing. He was so critical, so belligerent, almost.

GC Really?

AU Yes. But this typified the tendency for people not to see the forest for the trees. People were focused on their own particular approach to the problem and couldn't see the need to really pursue all promising, feasible leads.

You can't abandon people who have cancer. If there's any way to intervene to rescue them from the disease, one needs to try to do so. Not that this means one shouldn't try to prevent it, too. You've got to try to do both.

GC Right.

AU And, again, I think putting our money out there, letting the investigator-initiated grants tell us where the promising opportunities are, is probably as good a way to do this as one can find—not to decide, "Okay, I know what needs to be done. Now you do it."

GC Which is what it sounds like Richard Peto wanted to do.

AU Richard Peto wanted to do—well, he made that case. I'm not sure if he were to have traded places with me, he would have chosen to do that.

GC Right.

AU He was arguing that point of view.

GC And he was at the NCI?

AU No.

GC Or he was outside?

AU I believe he's at Oxford—I can't remember for sure where Peto is situated. I believe he's at Oxford. He and Richard Doll, a famous British epidemiologist, developed an assessment back in the late-'70s, early-'80s, tried to identify and rank the avoidable causes of cancer. They published an article in which they allocate, as best they can, the cancer burden over the various causes that they felt they could identify, and it was a very influential, useful, scholarly piece of work—not necessarily correct, but very helpful. And, as I say, tobacco is at the top of the list. They said perhaps 25 percent of all cancers are caused by cigarette smoking or the use of tobacco. Diet, they said, might be implicated in 30 to 70 percent, but the data are kind of soft, hard to interpret. Tobacco, they thought, was much more direct and much easier in the sense that one could just confidently say that 25 percent are tobacco-related. He was a thoughtful, good person. But I'll never forget that confrontation. I had another confrontation shortly after I arrived there. Sheldon Samuels, who was with the AFL-CIO, came to see me and said that the Labor Movement was very disenchanted with NCI, that NCI had not looked carefully at the occupational risks that many workers experienced. The Labor Movement was going to oppose NCI if NCI didn't do something to help workers. I'd never before—I think Samuels came to see me before Peto did—I'd never before had someone come and hit me over the head with a two-by-four, figuratively—

GC Right.

AU —and Samuels did. He was very confrontational. And I said, "Wait a moment, I'm just new on the scene here. Give me a chance, so to speak." So we formed an Occupational Cancer Task Force. Joe Fraumeni was asked to chair it. And it came up with a thoughtful review of occupational cancer hazards and constructive suggestions as to ways one could enlarge our knowledge through research and also try to do something about those problems. I'm happy to report that Samuels eventually ceased to be disenchanted with us, became a staunch ally. I think he eventually wound up as a member of the National Cancer Advisory Board—I can't remember but—

GC Oh, really?

AU Yes. But that was very interesting to me that Labor had a sour taste in its mouth, or at least so Samuels said, and we were able to respond to their concerns and—I won't say satisfied them, but made them less disenchanted. I remember another interesting—talking about vignettes and problems—shortly after I arrived, I received a call that Mr. Califano, who was Secretary of Health, Education, and Welfare, wanted to see me. NCI at the time had a budget of about \$1 billion a year—it was by far the largest institute, budget-wise. Califano wanted to know what that money was buying. He was an attorney, he didn't know anything about cancer, so he said, didn't understand the Cancer program, so it was my task to go down and sell him, if you will; educate him. I'll never forget it. Donald Fredrickson was the Director of NIH—he had a government car; I didn't—but he and I were the only Presidential appointees on the NIH staff. So he and I went down in his car to meet the Secretary one afternoon, the Assistant Secretary, the Surgeon General—Julius Richmond would also attend the meeting. We were supposed to see Mr. Califano about four o'clock as I remember. We'd gotten down to his office in the Humphrey Building and then were informed that he was running a little late, that if we would be seated, he would see us shortly. We got into see him at six-thirty!

GC Oh, my gosh!

AU But he kept us until eight-thirty. He was an extraordinary person, just hungry for information, wanted to know everything. In the course of our briefing, I showed him a graph that Sir Richard Doll had published relating the risks of lung cancer in British physicians to the numbers of cigarettes smoked per day. I had explained to the Secretary that cigarettes were generally thought to be the major cause of lung cancer, that most lung cancers were seen in smokers, and that this curve in which the risk of lung cancer rose steeply with the number of cigarettes per day, showing no threshold anywhere, was very powerful evidence this was a cause/effect relationship, a dose response. And the dose response would suggest that it might take only one cigarette a day to double one's risk of lung cancer, or for that matter, the risk to a passive bystander who works in the same office or lives in the same house and inhales cigarette smoke second-hand day-in, day-out, might also experience an increase of cancer. Califano was fascinated! He had been a smoker, he had given up the habit, but this information was just electrifying, and he wanted more. We went back and briefed him again. We then learned, perhaps I told him—I can't remember who—not only did cigarettes cause lung cancer, about 100,000 deaths a year from lung cancer at that time, but probably as many deaths from heart disease and from emphysema and other lung conditions. Califano decided that really something needed to be done, that cigarettes should be public health enemy number one, smoking should be prohibited in all buildings that the Department was responsible for, except in designated areas, et cetera, et cetera. We went to Congress, had to testify before Congressional committees—I had to go along to back him up in his testimony. The cigarette companies sent their representatives to my office, Tobacco Institute. One of them was a biostatistician that I'd worked with at Oak Ridge, a very nice person, and he said, "Art, you know you can't believe these death-certificate data. Death certificates are unreliable. We really don't know that cigarettes cause cancer." I looked him in the eye, and I said, "Marvin, you know better than that. The evidence is overwhelming." And he sort of cleared his throat and quieted down. But those people told me that—they said, "Dr. Upton, you're playing with dynamite. We have powerful friends on the Hill. If you don't cease and desist, we'll take your budget away from you."

GC Wow!

AU Well, I reported this to Mr. Califano. He said, "You and I have the obligation to the American people to do what is right. We cannot be intimidated." But the tobacco folks called my house at night with threatening calls, my wife became frightened, we put in an unlisted telephone.

GC Oh, my gosh!

AU But Califano, bless his soul, had the courage of his convictions. I've often wondered since then whether this willingness to confront the tobacco interests might have been a factor in his ultimate dismissal. You know, Carter had a Saturday night massacre in which Califano was one of the casualties.

GC Oh, really?

AU Yes. I'll never forget that. Again, people were astonished. This did not seem like Mr. Carter's style. And Fredrickson and I were summoned to the White House the following—this happened I think on a Thursday or Friday, maybe even a Saturday—we were summoned for a meeting early the next week at the White House. I'll never forget going down in Don's car, went into a huge room, and there were a bunch of chairs in there. Don and I were fairly early; we sat close to the front. Eventually the room filled up. I think there were at the time maybe a thousand Presidential appointees in Washington. The room filled up. Mr. Carter arrived, and he was very agitated. He proceeded to apologize for having asked for the resignation of members of his cabinet. He proceeded to explain that it was incumbent on members of the Administration to speak with one voice, and if people didn't want to accept the policies of the Administration, they owed it to the Administration to resign. Well, this was all quite understandable and reasonable. He almost got down on his knees and begged us for forgiveness. He took his jacket off and flung it on the floor behind him. And after this meeting ended, it was dark, we got into Don's car to go back to NIH, I remember sitting next to Don in the back seat, the driver drove us up Wisconsin Avenue, I couldn't see Don's face—it was kind of dark—and I said, "Don, what did you make of that?" thinking he would say, "Well, it was not very Presidential . . . it was unseemly for a President to be so distressed at having to make a decision like that, apologize,"—but I'll never forget it—Don said, "Wasn't he magnificent?" I said, "What do you mean, Don?" He said, "He was so absolutely honest." Don was used to Washington politics, and Carter was a humble, kind, compassionate, and generous person. And it came through. I've often thought since my days in Washington about the good things that Carter has done, the Habitat for Humanity, the diplomatic initiatives in the interest of peace and human betterment. He really has been a very good person. I didn't think he was too Presidential in that meeting, but he was good, kind, honorable, and a compassionate human being—nothing sleazy about Jimmy Carter.

GC No, I don't think anybody could say that about him.

AU No. So I'm proud to have been a member of his Administration. He was a very fine person. Still is, of course. As far as I know.

GC Did you have much contact with him while you were at the NCI?

AU I perhaps was in his presence a couple of times. I had a chance to meet with him before I went to NCI. Earlier in the '70s, I was asked to be a member of an Energy Policy Study Group sponsored by the Ford Foundation that looked into nuclear energy as a source of energy as compared with fossil fuel energy and solar energy. And the report was ultimately produced, and we met with Carter in the Oval Office to present the report. I'll never forget it. It must have been maybe a year or several months before I went to NCI. I didn't have any idea at the time that I would actually be one of his appointees eventually. I'll never forget that—he had been an engineer, I guess he had been associated in some way with nuclear subs. He was very well-informed, thoughtful, his mind was keen, he followed the presentation very closely, asked good questions—he was impressive. I don't think I ever briefed him afterwards. I did, as I mentioned, brief the Secretary at one point.

GC How did the whole thing with the tobacco end up? They must have eventually stopped calling your house.

AU Well, I think Califano simply persisted, and the tobacco people must have decided it was a lost cause. They weren't going to change Upton or Califano. They couldn't intimidate us. They may have tried to put pressure on their friends in Congress to cut the budget, but if they did, I wasn't aware of it.

GC You did say, though, that the budget when you were in office was less. Was it shrinking?

AU My recollection is that it was about \$1 billion. But in the '70s, in the early years of the war on cancer, the budget grew very much more rapidly than it did in the '80s. I think the NIH budget itself didn't plateau, but it grew much more slowly in the early '80s. I think one of the things that Carter had to do was to try to come to grips with cost overruns, and so that there was perhaps not severe austerity, but at least a period of austerity. And NCI wasn't immune.

GC Did you have to make cuts?

AU I don't recall that we had to make cuts. We didn't grow as rapidly as we had grown in the '70s. Actually, I was there—I left in January 1980, so I shouldn't say we didn't grow. During the late '70s, we weren't growing as rapidly as we had in the early '70s. I don't have the numbers in front of me, but there were adjustments in the projected rate of increase. We kept up with inflation, I think. Not much more than that probably.

GC You came into office right after the 1976 target date that a lot of people were throwing out. They wanted to have cancer cured by 1976.

AU Yes.

GC You came in that next year. Was there a sense of frustration or why didn't we meet this target?

AU I think there was. I can remember Donald Kennedy, who was the Commissioner of the Food and Drug Administration at the time, comparing the war on cancer with the war in Vietnam. And we were not winning the war on cancer. And I can remember being at some pains to explain that the war on cancer could not be compared with an engineering enterprise like the moonshot. We did not have the fundamental science base that would allow us to engineer this successful outcome. What we needed to do was to build the base block by block through research over a broad spectrum of fronts, not that this meant we wouldn't ultimately be able to win the war, but we couldn't predict how soon that would happen, and it was a mistake to have envisaged that with \$X billions in X years, or \$X billions in Y years, you would have the answer. It simply wasn't realistic to think about it that way. But there were many people who had been led to believe that we would succeed much more rapidly than we had been able to succeed. And there are still people who believe that. And I think there are those who probably deliberately mislead the public, or try to do so, on grounds that this enables them to sell the program more effectively. If you don't promise something, people aren't going to want to buy, so you make a promise even though you can't deliver. I don't believe that. You have to be honest. To mislead people is not only dishonest and for that reason wrong, but in the long run it's self-defeating. People wake up eventually and no longer have any trust or respect for the people who have misled them.

GC Sure.

AU So I had to try to undo or correct the misapprehension.

GC How did you do that?

AU I guess I was making statements to people who would ask about it. I don't recall whether we produced a paper specifically to try to correct this misapprehension, but there were plenty of opportunities. We were challenged frequently.

GC By . . . ?

AU Well, as I say, I remember Kennedy making that statement. I can't recall precisely where he made it or when, but I can remember having to respond to it. And we were being criticized in the press from time to time, having to respond to inquiries, having to respond to challenges.

GC The tape is ending. Let's stop for a minute.

AU Okay.

GC Okay. We were talking about people's misapprehension about the—

AU War on cancer.

GC —war on cancer. Did you get inquiries from just the general public? Did you have a feeling that the American public was wondering what was going on?

AU Yes, yes, we did. One of the things I tried to do—I considered myself a public servant and obligated to have an open door. When people wanted to see me, I felt it was my duty to see them. I can remember surprising, I guess, some of my colleagues, secretaries, colleagues, because I would see people they considered erratic, unreasonable, not worth the trouble. I took exception to that point of view, and said, "Well, they're paying my salary. I'm here for a job to do and that's part of my job." But I remember all sorts of strange letters I would get. Some of them would say, "Dr. Upton, when are you going to admit that you know the cause and the cure of cancer? And you're just sitting on it so that you can continue to enjoy public support." A lot of paranoia out there, conspiracy theory.

AU And I can remember my astonishment at some of those letters when I first received them. I didn't get many, luckily, fortunately. There are not a lot of nuts out there these days, a lot of letter-writing nuts. But there are some.

GC What was a typical day like for you? What were your duties during the day? What time did you start work?

AU I lived on the campus.

GC You did?

AU Lived on—what is it? Cedar?

GC Yes, Cedar Lane.

AU Lived not next door to Building 31, but just a short distance away, so I could walk to work in five minutes. I can remember I used to climb the eleven flights of stairs. I wanted the exercise.

GC Did you really?

AU And people thought I was sort of an eccentric.

AU I never rode the elevator. It wasn't that I was afraid of the elevator; I just wanted to walk.

GC I've walked those stairs. Those are a lot of stairs.

AU A lot of steps. And I enjoyed it. I usually tried to get there by seven-thirty, and was usually in my office until six-thirty or seven in the evening. It was a full day. As I said earlier, I despaired that my day was occupied with matters that were—I considered important, my responsibility—but to the extent that I wasn't able to get into the laboratory, go to seminars, to get to the library and stay abreast with my field of science. How did I spend the time? I wish I had kept a diary. There were, of course, administrative meetings, meetings of Executive Committees, and meetings that involved other members of the Institute involved in various tasks, meetings with other agency heads. I can recall going to meet with Donald Kennedy at the FDA. We became aware that patients on clinical trials [at the NCI] were receiving Laetrile surreptitiously.

GC Oh, really?

AU Laetrile was being touted as the miracle drug. It had not been approved for use in this country by the FDA, so people were going to Mexico to get Laetrile. And some of the state legislatures were legalizing the use of the drug in particular states, so it was getting out of hand.

Laetrile had been tested in animal models by the NCI or contractors for the NCI, and had not shown any definitive promise in animal tests. So Dr. DeVita and his associates in the Treatment Division were not enthusiastic about using it in patients, or even trying it in patients.

Eventually, as I say, we became aware that patients in our trials were getting Laetrile on the side, and it might confound analysis of the results, and we thought, there isn't any way to deal with this problem other than a clinical trial.

There's always the concern that the animal results aren't predictive for man. There were anecdotal accounts of people who had gotten Laetrile and had gotten better. So I went finally to Donald Kennedy, put my cards on the table, and said, "We've got to do something, Don. This is getting out of hand. My colleagues in the Institute wouldn't normally consent to a clinical trial without some evidence from animal studies that there could be a beneficial effect, but they've come to the place now where they think it has to be done because of these circumstances beyond our control."

So Kennedy finally consented to this. A trial was conducted, and there were no beneficial effects. So the steam went out of the Laetrile movement. [Laughter]

GC Oh, really?

AU Yes. But it took a clinical trial. I don't consider it unethical. If we knew something wouldn't work, it would be unethical to treat patients with something we know is useless. But if there is any reason to think that we may have a beneficial outcome, the only way to find out, to my knowledge, is to do the experiment. So that was the justification. But that was an interesting experience.

GC I'm sure.

AU But these are the ways I spent my time. There were meetings with members of the Cancer Board, the Cancer Panel, meetings with staff, patients would come to see me—distraught patients, cry on my shoulder, "What can I possibly do, Dr. Upton? I've been through chemotherapy, et cetera, et cetera, and my physician says I'm not going to improve,"—quite a bit of hand-holding.

GC Were these patients who were being treated at the Clinical Center or patients who wished to be treated there?

AU Patients coming from all over the world who were just discouraged at their progress, felt desperate, came to see me in the hopes that I could pull a rabbit out of a hat.

GC What did you usually do? Could you refer them to someone in the Institute?

AU I tried to refer them—yes, I usually referred them to my clinical colleagues who had the expertise and the knowledge of who was doing what where, who could help them ascertain whether everything was being done that could reasonably be done. I wasn't a clinician; that was not something I could do.

GC Right.

AU I could help point them in a direction that might either be helpful, or at least be reassuring.

GC Did you ever follow their progress, check up on them later?

AU I don't remember that I did, no. Perhaps I did in some cases, but I can't recall now.

GC Did you spend any time at the Clinical Center?

AU I used to go there from time to time to meet with colleagues. And I remember one patient—a couple of patients actually—who were there as a result of my intervention whom I went to see now and again. I think one of the people I went to see was Hubert Humphrey.

GC Really?

AU Yes. My recollection is that he was in the Clinical Center at one point undergoing treatment as a cancer patient.

GC And had you known him, or were you there out of courtesy?

AU No, I'd admired him. His political philosophy was congenial to me. I admired his political accomplishments. I spent a fair amount of time testifying. I remember one occasion testifying before the McGovern Committee. Senator McGovern chaired a committee on agriculture and human nutrition. And there had been the allegation that the National Cancer Institute was not addressing nutrition adequately. You remember, I mentioned that Doll and Peto had inferred that anywhere from 30 to 70 percent of cancers might be the result of dietary factors. Guy Newell was an enormous help in canvassing the Institute, exploring the literature, arming me in the way to go to the McGovern Committee without making a big fool of myself, discrediting the Institute. But I'll never forget it. After testifying for maybe an hour or two, trying to explain the different lines of research that the Institute was pursuing that had to do with diet and cancer, nutrition and cancer, it wasn't feasible to equate the sums that we were spending, the percentage of the total budget, to the Doll/Peto 30 to 70 percent cause estimate. I think by grasping at straws we were able to argue that perhaps 10 percent or 15 percent of the budget was directed at nutrition-related problems, nothing like 30 or 70 percent.

AU And I'll never forget, at the end of that hearing, McGovern shook his head and said, "Doctor Upton, it looks to me as though we're losing the war on cancer." A rather distressing outcome. But there was just no way at that time that we were able to say we were spending as much on diet as the Doll/Peto analysis would argue we should. I'm still persuaded that diet is a very important risk factor, too much of the wrong things, too little of the right things, but that doesn't mean that we know how to do the research to find the answers or that by putting 30 to 70 percent of our budget into diet, we would be doing the right thing. McGovern was grandstanding, making a political statement. But it wasn't my happiest day, I tell you.

GC I guess not.

AU But I do feel that the issue of diet and cancer is important. And I did enunciate on behalf of the Institute, thanks to Guy Newell and the help of colleagues, what we considered at the time to be prudent dietary principles for minimizing one's risk of cancer. And I can remember at the time these were not popular. We were ahead of the curve. I can remember Bob Levy—I remember one cocktail party we went to, and Bob Levy who was the head of the Heart Institute, pouring cold water over some of our dietary principles on grounds that they weren't really scientifically sound. One of the things we were arguing at the time was that you should greatly restrict one's intake of dietary fat, that Americans were eating too much fat. And I can recall Levy thinking that that probably wasn't scientifically justified. Well, in retrospect, I think we were right.

GC Yes.

AU And I feel happy that what we said at the time has stood the test of time.

GC So it was the scientific community that wasn't accepting these measures of diet and prevention? Was it the public as well?

AU I don't—I think the nutritionists, by and large, were with us. But there were other people in the scientific community who weren't. You know, nutrition at that time hadn't caught on.

GC The idea of watching what you eat and doing a healthy diet?

AU Yes, it wasn't so popular at the time. Nutrition and fitness have since gained very much greater credibility and fashion. The Heart Institute came around in subsequent years and joined us in chanting "a low-fat diet." But low-fat, high-fiber, low-salt, et cetera, just seemed to be a little far out when we went to the McGovern Committee and put these principles out.

GC And now it just seems like common sense.

AU Now it seems like common sense. I can remember earlier in my career—I'll never forget it—I went to Oak Ridge back in 1951, worked with Jacob Furth, a wonderful experimental pathologist. Came into lunch one day, and he said, "Upton, there's a man named Ludwik Gross at the Bronx V.A. who says he has discovered a virus that causes leukemia in mice." And we looked at his papers. After a few months, Furth said, "You know, Gross may be right. The luminaries in cancer biology are impugning his motives and impugning his scientific honesty. But he may be right. We should get him to come to Oak Ridge and do his experiments in our laboratory where we can observe him carefully, and we should ask him to leave the mice with us." See, he injected newborn mice with cell-free extracts from leukemic tissue, leukemic mouse tissue. And the mice that he injected as newborns, then developed a higher incidence of leukemia. Now the controls were injected with a saline vehicle, and Gross was inferring that because these were cell-free extracts, they contained an infectious agent. So we invited him. And he injected the mice. We watched him. I'll never forget it. Furth ran around—Gross was such a dynamo! He came with his associate, worked very rapidly, and Furth was trying to take careful notes of everything he did—didn't have a tape recorder—

AU —or a video camera, and he almost threw up his hands at one point, it was so hard to follow Gross through all of his maneuvers. But at the end of the day, he was persuaded that Gross had done what he had said he would do, the real tricks. So Gross went back to the Bronx, left the animals with us. And a couple of months later, Furth left and went to Harvard, and left me in charge of the program. And as the infant mice grew to adulthood, some of them did develop leukemia. And I would see Gross at meetings, cancer meetings. He'd say, "What's happening?" And I'd say, "Dr. Gross, Dr. Furth has told me that I'm not to reveal anything until all of the animals have died and we can make a scientific comparison. I can't disclose any preliminary data. This is the compact, and you agreed to this." Well, Gross came to see me as one of his persecutors.

GC Oh, really?

AU He became very resentful. But finally, when all the animals had died, the results that he'd predicted were in fact results that we observed. He was vindicated, and we were able to publish this. And then he and I became friends. He saw that I wasn't his enemy, I wasn't one of his persecutors, I was really his champion. But I've seen and I can remember other—Charlotte Friend had a virus that caused leukemia. And I can remember going to meetings and Charlotte would present her work, and people would sort of scoff at it, and say, "after all, she's just working with mice; that doesn't have anything to do with human beings," or "she doesn't really have cancer; that's an infection that she's misdiagnosed as leukemia." So I've seen the skepticism that greets new ideas or unorthodox, innovative ideas. Scientists are trained to be skeptical, but very often this is carried to the extreme of closed-mindedness. And there's a balance.

GC Scientists are often very traditional, as well, right?

AU Exactly. So I wasn't astonished that my principles, dietary principles, were greeted with skepticism. I'd seen it before. Galileo had to suffer.

GC I think all of those great minds do.

AU You're right. I think so. Every new idea begins as a minority of one.

GC That's right. Is that your quote or is that a famous quote?

AU I don't recall. It's probably not mine. But I think it's true.

GC It's a good one. It is true. Was the NCI in general a good place for people who were creative or had new ideas?

AU I think the NCI, like the other Institutes, is a beautiful place for people who want to do research. One of the things that I instituted as a result of recommendations from outside was a system of peer review for the intramural program. We made it a policy that every intramural research program should undergo periodic peer review by an external peer review committee. The external committee wouldn't have veto power over the work, but would have the responsibility of critically evaluating the work and informing the Division Director so that he or she then would have the benefit of that independent insight. So I think that people at the NIH should be subjected to peer review. All of us need to have that sort of external criticism from time to time. The opportunities that exist there to do full-time research are marvelous, and one can be very creative. And people there have been very creative. I can remember a wonderful comparison that DeWitt Stetten made. DeWitt Stetten was the Deputy Director at NIH when I went there, a wonderful man. He published a little report entitled *The Cathedrals of Science*.

GC *The Cathedrals of Science*?

AU I believe that was the title. He said the scientific enterprise is like the building of a great cathedral. The scientist, like the cathedral builder, places block after block into place, and any one scientific report, just like any block of stone that goes into the cathedral, is not itself terribly impressive necessarily, but eventually, collectively, one sees a magnificent edifice that results from this laborious process. And it is the cathedral that one eventually has that is so marvelous. And I thought that was a beautiful, and a beautifully apt, analogy. And so I think one is, in a place like NIH, really building cathedrals. The labor of the individual scientists one by one enriches humanity enormously over the long-haul. One can't measure this step-by-step—that's not the appropriate yardstick—but step aside and look at what happens over a decade or the period of a generation. The contribution is really impressive. So it's money well-invested. A scientist really doesn't work for money. I remember a marvelous day at Oak Ridge back in the '60s. I had a visit from Isaac Berenblum, from the Weizman Institute. Berenblum was probably the leading cancer biologist of his day, a fabulous person. And as a young scientist, to have this luminary spend the whole day with me talking about *my* research, was just heady. We would discuss experiments and try to figure out what they meant where they led and so on—just a wonderful day. At the end of the day, we rose to leave the laboratory, and Berenblum looked up at me and grinned and said, "Upton, isn't it marvelous to be paid to play?"

GC Wow!

AU That's really what one is doing at NIH. People who are there are not there for the money or for the Nobel Prize. They're there because they're doing what they love to do, trying to discover something, trying to understand something. If you like to solve crossword puzzles, it's fun. And science is fun. And it's a great place. There are good people there doing good things.

GC Do you remember any major breakthroughs while you were there, any . . . ?

AU When I first went into science, my dad always used to say, "Artie, any breakthroughs?"

GC Oh, really?

AU Yes. And I confess, I don't remember any breakthroughs, no. I suppose if I were to go back carefully and review exactly what was being done at the time, there might have been some breakthroughs. I was there two and a half years; there could well have been some breakthroughs. None stand out immediately as I think about it.

GC Who did you work with on a daily basis? Who did you have contact with?

AU The person who served as my secretary at that time was a woman named Phoebe Dunn. She had been the secretary before.

GC For Dr. Newell?

AU For Newell, and I think also for Dr. Rauscher. She was an old-timer, so to speak, a gray-haired lady, very knowledgeable, very helpful because of her understanding of the system, the Institution, and the do's and don't's. And so I had daily interactions with Phoebe. Guy Newell, my Deputy; I worked closely with Guy on a daily basis. Cal Baldwin was the Executive Officer, and he and I were in virtually daily contact. There was another secretary in the office. Phoebe was the Executive Secretary, but there was another woman, a lovely woman, very nice person, a woman I would think in her forties perhaps, and I don't recall her name now, but she was very, very helpful, and enjoyable person to work with. And there were others on the administrative staff. I wish I could recall all their names now. I'm embarrassed to confess I don't. But I had almost daily interactions with them for one reason or another. I met regularly with the Division Directors. From time to time I would have to go over to—what is it, Building 1?

GC The administration building?

AU Yes, meeting with Dr. Fredrickson and other people in his administrative staff.

GC When you met with the Clinical Directors, what was the content of your meeting? Would they report to you? Did they make decisions and report them to you? Did you help make decisions on—

AU They were really—you're speaking, I think, primarily of DeVita in the Treatment Division.

GC Sure—for example.

AU Yes. They really managed their affairs on a day-to-day basis. I was involved really only in policy issues, really had no direct involvement in their day-to-day clinical problems. I'm not myself a clinician; I wouldn't presume to enter into clinical issues.

GC So these meetings were just to update you on what was going on in your divisions?

AU Primarily, yes. I have to confess now, I don't recall a specific agenda.

GC Okay. Well, you've torn through so many of my questions.

AU I'm sorry if I've been disappointingly—

GC No, no!

AU —superficial and brief.

GC No! Absolutely not, absolutely not! Okay. There's one thing I wanted to ask you about. The Special Virus Cancer Program—where did that stand when you were there?

AU My recollection is that we were spending about \$100 million a year on the Cancer/Virus—what was it called? I can't remember exactly.

GC Well, it changed names a few times. It might have—

AU Viral Carcinogenesis or something like that.

GC Yes, that sounds right.

AU Yes. My recollection is we were spending about \$100 million a year. And I don't recall that we changed that very much during my administration. There were, I think, two principal objectives. One was to really understand how the viruses were causing their effects, what were they doing? Another was to get some better understanding of the distribution of oncogenic viruses in different species; in rodents, in fowl, in canines, in primates, in humans. Were these tumor viruses that Moloney and Rauscher, Rous, others had found, anomalies of some kind, or were they really commonplace, and were they an important cause of human cancer? So these were the two objectives, and I think both were important, logical, reasonable objectives to be pursued. And I think that the insight that the tumor virus research has given us into the fundamental molecular biology of their activity has been phenomenally helpful. The discovery of the oncogenes, for example, we owe to the Virus Program, primarily. So I was called upon, I think, to justify—"Well, Dr. Upton, we don't know for sure that any important human cancers are caused by viruses—why can you spend \$100 million a year on this problem?" Well, I think, the pathway into the oncogenes is amply justified, pursuing the tumor virus research. We wouldn't be here without that.

GC Right. Did you make any changes in the Program as it stood?

AU I don't recall making fundamental changes, no. Perhaps if we were to say changes, there was an enormous contract-supported effort, and by dividing the responsibility in a way that separated responsibility for contracts from grants, that I think that tended to diminish some of the contract money. Money that would have gone to the programs for contracts went through the grant mechanism instead. That may have been the major change, I would guess, if there was a change. I don't recall for sure there was a change actually.

GC Okay.

AU I was not opposed to the program. I was not among those who criticized spending that money on viruses, because as I say, it's paid off in its molecular insights.

GC It did get a lot of criticism, though, right?

AU Enormous criticism, skepticism, and you were really going after issues that had no relevance to human cancer. One couldn't argue that there wasn't a fowl leukemia virus or a mouse leukemia virus, but that this had anything to do with human cancer was hotly contested at the time. Only now, looking back, can we say, well, gosh, where did the Rous virus come from? All these oncogenes owe their discovery, I think, to the initial observations on animal tumor viruses.

GC And that also led to Dr. Gallo's work on IL-2?

AU Yes, that's right, exactly.

GC And that was right about the time that you were in office, wasn't it, that Dr. Gallo was doing IL-2 and then the HTLV before it was HIV?

AU Yes, I think so. That may have come just before I arrived on the scene or about the same time.

GC Was Dr. Gallo still there, do you know?

AU Gallo was still there, yes.

GC Was he a big name like—

AU He was a big name. He was a target actually. A lot of people said Gallo was spending all this money in an enormous contract program, and if he were really critically peer-reviewed, he couldn't possibly justify this huge program. There were those pointing fingers at Gallo, I think. He's too fat a cat, too big a fat cat. And I had to protect him.

GC Did you pretty much protect the scientists in general at NCI from criticism, defend them from criticism?

AU Well, in a general way. I don't recall having to rise to the defense of individuals, but I had to defend the program. A billion a year is a lot of money.

GC That is a lot of money.

AU And as we said earlier, there were those who were arguing that the war on cancer wasn't being won, that there were huge sums of money going down the drain because they weren't getting anywhere.

GC A sense of frustration that—

AU A sense of frustration, a sense of resentment. People had the feeling that somehow they had been deceived into thinking that if they just spent that money, the problem would be solved, the problem would then go away. They spent the money, the problem hadn't been solved, the problem hadn't gone away, so someone had misled them. That was the concern. And I had to emphasize that if they had been misled, that this was unfortunate, but this was not our intention, that we certainly didn't want them to be misled any longer.

GC Right. We're at the end of the tape again.

AU Okay.

AU This morning you asked if I were surprised when I was invited to be a candidate for the Directorship of NCI.

GC Right.

AU I was surprised, but looking back, I perhaps wasn't a totally illogical candidate. I had served as the President of the American Association for Cancer Research back in the '60s, I think '63 to '64. And I had served on the Board of Scientific Counselors. The Division of Cancer Biology and Diagnosis, as it was called in those days, was under the directorship of a man named Berlin, Nat Berlin.

GC Yes, I'm going to be interviewing him!

AU Wonderful! He has now—he had moved to Florida, maybe he's still there.

GC He is, in fact.

AU And he had a Board of Scientific Counselors, it was called, an external advisory group, on which I served as a member. I may even have served as the Chairman of that Board at one point. So that I had a background. I was present in the room when Nixon signed the Cancer Act.

GC You were?

AU Yes, present at the White House at that meeting.

GC Wow!

AU So I'd been involved with NCI in the past. I did have some background that probably put me into the candidate pool. You were asking also about problems at the Institute. One of the things that I encountered in my time there was the, as I mentioned earlier this morning, concerns about the balance of effort. And one of the people on the Institute staff was a man named John Bailer. John was a biostatistician. He was the editor of the JNCI at the time I went to the NCI, a very thoughtful, creative, good scientist. As a biostatistician/epidemiologist, he was quite dissatisfied with what he thought was too much emphasis on treatment, insufficient emphasis on prevention, and campaigned for greater attention to prevention. We had at the time . . . and I believe we set up a program to explore approaches to prevention, as a result of Bailer's agitation for it. And I think it was useful. In that context I can remember being taken aback at one time when I approached Lester Breslow. Lester at the time was the Dean of the School of Public Health at UCLA, and had pioneered in studies of the extent to which one's health habits might influence one's health, disease patterns, and mortality. I remember asking Breslow if he would be—maybe it was Bailer who put me up to this; I can't recall why or how it happened—but I remember asking Breslow, whom I admired as a public health person and a scientist, if he would serve on the National Cancer Advisory Board. We didn't want to nominate him if he would be absolutely opposed to the idea. And I was astonished when he said that he didn't really think there was any point in his serving on the board, that NIH wasn't interested in public health, NIH was an ivory tower where people just went into the laboratory and turned their back on society. And I tried to dissuade him. We had at the time a budget that was labeled "control" separate from the budget that was labeled "research." And the control pocket was supposed to support demonstration and education and that sort of thing. It was more of a public health activity. And I told Breslow we needed him, that it was a mistake to throw up his hands and despair and write NIH off as hopeless, that if he really felt there were reforms needed, it was incumbent upon him to roll up his sleeves and help us accomplish them, that I would stand behind him. So I finally persuaded him to accept the nomination, and he was appointed to the Board. And we enjoyed working together. But that was another of the interesting insights into the history of NCI and how it was perceived.

GC From the . . . ?

AU In the public health community at the time, or at least by some members of the public health community. So these are things you prompted me to recall.

GC You said you were present at the signing.

AU Yes.

GC When Nixon signed the Act. What do you remember about that day?

AU I recall going to the White House for the occasion, being in a fairly large room full of people. I didn't meet Mr. Nixon, but I was able to watch him come into the room. I can't recall who introduced him or what he may have said. I do remember watching him sit down at a table and sign an Act—and I got a pen.

GC You did? You got a pen?

AU He gave out souvenir pens on the occasion. It wasn't the pen that he used, I suspect, but it was one of the many pens that were distributed among the people who witnessed. And it was fun. I was teaching at Stony Brook at the time, and flew to Washington on the shuttle, attended the signing, and then came back directly afterwards. I remember meeting on that occasion Phillip Strax, who was a radiologist who pioneered an xray mammography. I enjoyed meeting Strax. I hadn't met him before. We became friends and had a number of interactions in the years following.

GC How many people were there, would you say, at the signing?

AU Oh, dozens. I doubt if there were a hundred, but there were probably more than twenty.

GC What was the mood in the room? Was there a sense of excitement?

AU Yes, I think there was a great deal of excitement. People who were there, by and large, were upbeat, people who were concerned about the cancer problem, anxious to see more effort devoted to address the problem, and were really quite pleased that finally the effort to develop a national campaign would reach the stage where the President was signing an Act, declaring war on cancer. That was momentous—like Kennedy's deciding we should race the Russians to the moon, the same sort of an atmosphere, eager expectation. And as we've said earlier, probably misplaced or a wrong-headed notion that we were going to be able to lay out a strategy that would get us there in a predicted time. We weren't ready to do that. It wasn't like building a bomb, where they knew how to do it, and it was just a matter of development.

GC You talked earlier about how you were quite an expert on radiation as associated with cancer.

AU Yes, right.

GC When you were at the NCI, did you have a chance to use that knowledge or implement programs?

AU Not really, no. That was one of the things that disappointed me, I was so far away from the day-to-day science that I really didn't—before I went to NCI, I was involved in a study group—I mentioned John Bailar. In the 1960s and even more in the 1970s, early '70s, it became evident that the female breast was a sensitive target organ for radiation carcinogenesis. There had been epidemiological studies where women whose breasts had been irradiated, and certainly in the A-bomb survivor population, it was quite clear that the risk of breast cancer was high. And Phillip Strax, I mentioned meeting at the signing, had pioneered in x-ray mammography. The American Cancer Society and the National Cancer Institute in the early '70s, maybe mid '70s, had issued guidelines recommending that every woman of reproductive age should have a mammogram every year because that would be a way of detecting tiny little cancers in the breasts at a stage in their development when they weren't yet palpable or detectable in any other way, but still curable. Strax had pioneered in a study here in New York City, the HIP Study, Health Insurance Plan of New York Study, that had shown benefit, was the first to do so. Bailar sent up an alarm signal, just before I—oh, I guess about '75, '76, I don't remember exactly when—saying, "Wait a minute. We now have evidence that the breast is sensitive to radiation. How do you know if you're x-raying women's breasts every year that you're not going to cause more cancers that way than you're going to detect? You'll kill more women than you're going to rescue." Well, that was a fly in the ointment, and I was a member of a study group to look at the issue and try to decide whether Bailar's warning really was worth heeding. Was he crying a false alarm, or was there really a problem? And we recommended to NCI that women not be subjected to annual mammograms every year starting at age 25, but wait until, say, 50. The risk of breast cancer goes up steeply with age anyway. The radiosensitivity of the breast goes down with age.

GC Oh, it does?

AU Yes.

GC I didn't know that.

AU So if you postpone annual films until you're more likely to find something, not only is the benefit going to be greater, but the risk is going to be much smaller. So that prompted the NCI to backtrack and to change its guidelines. I've since remained interested in this problem, and I have published more recently on it, with colleagues. I think probably one can justify starting annual mammography at age 40 now, not waiting to 50, but probably not starting at age 20 unless there's a special risk factor. But I wasn't close enough to the firing line to be able to be actively involved in these matters when I was the Director.

GC Because, as you said, you were tied up with other—

AU Tied up with other things, concerns.

GC The political.

AU Right.

GC I read in one of the brief histories of the NCI that you nominated William Hueper for a Director's Award.

AU Yes, I was very happy to do so, correct.

GC Could you tell me about William Hueper and why you nominated him?

AU Sure. Bill, as he was known to me and his friends, was a stormy petrel, a man of enormous integrity, dedication to professional ethics and standards and scientific rigor, and was a pioneer in occupational health, deeply concerned about risks to workers, exposure to carcinogens in the workplace. And I don't recall any longer the nature of the controversy, but he was not appreciated in his day by the establishment. He worked in the Public Health Service. I don't recall whether he was on the NCI staff; he may well have been. But he was considered by some of the higher-ups as somewhat of an embarrassment, "he was always making trouble, stirring things up." And in retrospect, he was right more often than wrong. I won't say he was always right, but basically he was trying to do the right thing, and he should have been encouraged and supported and appreciated more than he was. I first met him personally in the mid '60s. He and I were together in a meeting in Lima, Peru, a cancer meeting. And we traveled—the party of us were at a symposium there, maybe a dozen or so invited participants—and journeyed from Lima to Cuzco, the Lost City of the Incas up in the Andes. It was a glorious experience! And I remember traveling with Bill and enjoying his companionship. He was an ebullient person full of humor and fun. And so at the end of his life, when he was old and frail and feeble, it gladdened my heart to be able to acknowledge his contributions with the Award—too little, too late, but better than nothing.

GC And he eventually became—they call him the grandfather or the father of the whole environmental carcinogenesis movement.

AU And I think he probably deserves—I won't say he's the father, but he certainly was one of the pioneers. He deserved a lot of credit. And, as we've said before, frequently new ideas are not popular.

GC Right, not until later.

AU Not until later.

GC In terms of your career or your scientific knowledge in general, did you feel that your time at NCI helped you, boosted your career, added to your wealth of knowledge?

AU Oh, undoubtedly. Yes. We live in a reductionist era. One of the curses of modern technology is that with the explosion of information, it's harder and harder to see the forest for the trees. To be an expert, to be competent, one must specialize because there's so much to know about even the smallest subject. But the more one specializes, the more one narrows down, the less one retains the broad perspective that one needs. And I think the opportunity to serve as the Director has given me a perspective which has enriched my life enormously. It probably cost me money. Had I stayed in my teaching post and continued to pay into TIAA-CREF, I'd have a greater retirement income than I have otherwise. I'm not aware that it boosted me career-wise at all. But it certainly made me a wiser human being. It brought me into contact with many people whom I would not otherwise have known and others I've worked with. And I'm grateful for this. I wouldn't be sitting here talking to you, for example—

GC That's true. What are you proudest of, of your time at the NCI?

AU I guess I'm simply proudest of the fact that I was able to help that institution. That's a marvelous institution. I don't think I left it any worse off than when I found it.

AU I don't think I damaged it. I think I was able to help it develop in ways that strengthened the institution. It's a world resource, and to have been able to serve the world, people alive today, alive yesterday, going to live tomorrow, in the way I did, is something I value very greatly. I'm proud to have had that opportunity.

GC What part of the job did you enjoy the most? Was there any part that was fun or especially pleasurable?

AU It was all pleasurable in a way. There were some difficult parts of the job. I didn't take kindly to being a political football. Going to testify and being berated, really on political grounds, was not pleasant. I remember feeling a sense of foreboding when I had to appear on the *Good Morning America* show one morning with Senator [Robert] Dole. He was not noted for his charity. He was a Republican, and I was serving under a Democratic President. I was expecting him to be nasty. He wasn't. He was quite reasonable. But there were occasions when I thought that I was being picked on, the Institute was being picked on, unfairly, and that was not easy to take.

GC By whom?

AU People who were trying to make political hay, by and large.

GC Anyone in particular?

AU I don't remember anyone in particular, no. But that's part of an education. That's the real world. And I didn't enjoy every moment of it, but looking back on it, I'm glad that I had that educational experience. I have an understanding which I would never have known and otherwise have.

GC An understanding of the political system?

AU Yes.

GC How many times did you end up testifying before Congress?

AU Oh, golly, I couldn't tell you. I would think perhaps at least once every other month, I would guess.

GC Oh, so it was very frequent.

AU Quite frequent, yes.

GC And would Dr. Fredrickson go down with you, or would it depend on the—

AU Not always. It depended on the situation. When I testified before the McGovern Committee, I don't remember that he was there. I think the people who went with me on that occasion were Guy Newell and Diane Fink.

GC And who was Diane Fink?

AU Diane Fink at the time was Director of the Division of Cancer Control. And she was the person whose division was primarily concerned with education, education and demonstration. And in the nutrition area, that was an important dimension. NCI was much more heavily involved in nutritional education rather than research on dietary risk factors, perhaps mistakenly, but that's the way it was then.

GC Unbalanced again, do you think?

AU Well, it's hard to know. If you were to leave it up to the scientific community and simply support the most promising applications that come in, as we tried to do, there wouldn't be a whole lot in nutrition now—or at least then. Things come and go. One has a new lead, and by gosh a field opens up and everybody jumps in. And then they work out that vein of ore, and they don't know where to go, so they go someplace else. When I was at the NCI, nutrition wasn't a terribly exciting area in research, not that it was zero, but you couldn't have spent a whole lot of money doing things that weren't scientifically creditable, not a whole lot of research that people knew how to do. Experiments people knew how to design then that would really get us anywhere.

GC This is a more general question, but at that time when you were in charge of the NCI, what was the American public's view towards cancer overall? Was it—when the NCI started, it was started out of a real atmosphere of fear, that there was nothing that could be done for cancer.

AU That's right.

GC And so they started the NCI saying, "We have to do something."

AU Yes.

GC In 1977, '78, '79, '80, what was the general feeling then?

AU I think it varied depending upon whom you spoke to. I think there was a great deal more willingness to consider cancer a potentially curable disease. There was more recognition of the fact that we knew how to detect certain forms early and needed to work at that effort, screening. As I mentioned to you earlier, there were some nutty ideas that came across my desk, ideas that we really knew how to cure cancer and were simply withholding that information. So I think depending on whom you asked in the population, you'd get different answers. But by and large, I think there was a great deal more enlightenment.

GC Do you think that was due to the NCI's efforts?

AU To some degree, surely. But the NCI was not the sole player. The American Cancer Society had been very active. When I went to the National Cancer Institute, there had come into existence cancer centers in most of the major teaching hospitals and medical schools that NCI was supporting. These didn't exist back in the '50s nor really until the Cancer Act. There wasn't enough money to support this kind of multi-disciplinary effort. But that made a difference.

GC The cancer centers?

AU Oh, yes. It brought together—first of all, the professional approach changed. Cancer now was recognized to be a disease in which a team effort was called for; the diagnostician, the internist, the surgeon, the chemotherapist, the clinical oncologist, the rehabilitation expert, cancer nursing, social workers to work with the family. This had not existed previously, and so there's, I think, a major change in the professional community and reaching out and into the general community.

GC Benno Schmidt wrote an article for the JNCI in 1977, and he was bringing up some of the concerns that people had brought to him. And one of them was that the cancer centers were getting too much funding and that there were better ways to spend that money. Did you ever hear a criticism like that?

AU Yes, I did.

GC Where did that come from? Why would people say that?

AU I'm trying to recall now how that came up. Well, let's look back. There was a great deal of concern when I arrived on the scene there that a small number of people on the inner circle were controlling a lot of money, that this \$1 billion was going into NCI and being handed out to favorite sons, and the cancer centers were being supported generously. They needed to be supported generously. But there were people trying to get their grants funded, not succeeding, and seeing money going into the cancer centers. "So here's Joe Doaks who's being supported through the cancer center, he's no better than I am, but he's got support, and here I am, I can't get my grant funded. If there were more equitable distribution, I'd get some money and he wouldn't get quite so much,"—that sort of sour grapes. I don't say that none of it was deserved, because I think there probably were instances in which people in the cancer centers were getting support who wouldn't have succeeded in getting their grants funded on the outside. And that's one of the things we tried to correct, making peer review a more regular and more equitable hurdle for everybody. We've seen that throughout the NIH system, so-called specialized centers, which were designed to bring together people of different disciplines and start getting them to work together in ways that would be fruitful, the whole being greater than the sum of the parts. These centers, which needed seed money and that sort of incentive to get going, eventually came to be supported simply because they were there, and often without adequate concern about the quality of what they were doing. So there had to be some correction. And I think that's happening.

GC Do you feel like you met your goals as Director? Did you have goals when you came in, or were you just coming in to serve?

AU I think basically I did meet my goals. I was going there primarily to try to do what needed to be done, and I believe that I did not fail. Others might have succeeded better. I can't point to a particular area where I think I fell a little short. I didn't go with a grand design to restructure and change things, and succeeded in doing that. I did make some changes, but this was not part of the preconceived plan.

GC Was it difficult for you to come in as someone who was not from within the NCI?

AU I think had I grown up in the family, as Vince DeVita, let's say, or Dick Rauscher, I would have had a research base in-house, could have kept one leg in the lab, or one finger in the lab, and would have been happier for that reason. It was never possible for me. And when I went to NIH, I severed all my connections with the laboratory. As I mentioned earlier, that was an unhappy situation for me. I didn't want to see that continue for the rest of my life. Apart from that, it was not difficult. People were friendly, very cooperative, very supportive. I never for a moment thought that I was swimming against the current. Have you talked to Cal Baldwin?

GC No.

AU He'd be somebody that would be very informative, I think. He was the Executive Officer when I went there.

GC Do you know where he is now?

AU He lives in the NIH area. I think I have a—I think he was the President of the NIH Alumni Association. I think I have his address.

GC Oh, really?

AU After we finish I can go look for it, unless you want me to do that now and turn the machine off.

GC No, it can wait until we're finished.

AU Alright.

GC When you left the NCI—we talked about this a little bit—you left because you had a chance to go back to academia?

AU Right.

GC Can you just tell me when you left and then bring me up to date a little bit?

AU Sure. I left in January of 1980 to succeed a man named Norton Nelson who had retired as the Chairman of the Department of Environmental Medicine at the New York University Medical Center. I became his successor as Professor and Chairman of Environmental Medicine and Director of the Institute of Environmental Medicine, a position I held for twelve years. I retired in 1992—in academic life you retire at age 70, and I turned 70 in February of '93. So I left in December of '92. We moved to Santa Fe. We had a daughter, a son-in-law, and five grandchildren in Santa Fe. My daughter and son-in-law are artists who work together. Everything they do, they do as a team. My wife is an artist, as you note, so it seemed like a nice place for us. Here we lived in one of NYU's apartments down in the Village, and on retirement we had to vacate that apartment. So we had really no other place to live in the city. We moved to Santa Fe, loved it there. My son-in-law developed a respiratory problem—the elevation in Santa Fe didn't agree with him. They moved to Texas. We didn't choose to move with them. But the opportunity presented itself to come back here in research half-time at the Robert Wood Johnson Medical School. So I took that job. So I'm now Clinical Professor of Environmental and Community Medicine at UMDNJ Robert Wood Johnson Medical School. I commute to Piscataway, New Jersey several days a week, which I'm enjoying very much. I'm a workaholic. I like to be in harness.

GC And you also do work for the Department of Energy? Is that right?

AU Well, the thing that brought us back was a cooperative agreement with DOE. DOE issued a request for proposals back in '94. As you may be aware, there are many, many contaminated nuclear weapon sites that DOE is responsible for, and the clean-up of those places is now estimated to cost hundreds of billions of dollars. And DOE despaired of being able to prioritize the effort in a way that would be adequate because of the suspicion and distrust that exists in the communities surrounding those sites. DOE operated in secrecy over the years; people don't have confidence that they can rely on DOE to do right by them. So DOE was advised by the National Academy of Sciences to turn out, turn outside to some independent institutional entity that could work with stakeholders at the different sites in assessing the risks and developing risk management options that would be understandable, credible, acceptable to all concerned. So several colleagues of mine, Bernie Goldstein, who's Chairman of the Environmental Community Health Center, UMD, Robert Wood Johnson; Gilbert Omenn, Dean of School of Public Health, University of Washington-Seattle; Chuck Powers from Boston; Jack Moore from Washington, asked me if I'd go in with them to formulate a response to DOE's proposal. And we did, and we were funded. So then it became necessary for me to decide whether to try to commute from Santa Fe or move back east—because Bernie Goldstein was the P.I., and he's based at UMDNJ. So we moved back. So here we are.

GC It sounds like you enjoy it here.

AU I love it, yes. My wife and I enjoy it in New York. We like the music, the art, the theater. It's an okay commute. I don't really enjoy driving an hour and a half each way to the Medical School, but I don't do it every day. And all in all, it's fun. Lots of people commute these days. I don't feel sorry for myself. You probably commute yourself.

GC I have quite the commute. It's not so good.

GC Well, you mentioned Cal Baldwin. Is there anyone else that you can think of that I should speak to at the NCI or that was at the NCI at the time?

AU Yes, Al Rabson.

GC I'm speaking to him next week.

AU Good, wonderful. Please give him my best.

GC I will, I'll do that.

AU He was the Director of the Division of Cancer Biology and Diagnosis, Nat Berlin's successor. And I think he was—he's a pathologist. He probably served alongside Bill Hueper or remembers Bill's days. He may not have gone that far back in time. I'm trying to remember the name of the elderly chairman of that pathology group—Red-somebody.

GC Harold Stewart?

AU Red Stewart, yes, Harold Stewart. Have you spoken to him? Is he still alive?

GC He is alive. He's—

AU You ought to talk to Red. He's a marvelous guy. I heard him give a Presidential address at the American Association for Cancer Research back in the '50s, a wonderful address, hard-hitting. I think it was Red's initiative that led us to award Hueper his prize.

GC Oh, really?

AU Yes. Red was one of the champions of that effort. I think Stewart, Rabson, Baldwin—I can't off the top of my head think—of course you might want—Don Fredrickson is still living in the area, probably.

GC Yes, he is, actually.

AU His perspectives would be of interest. He was not a member of the NCI staff, but he was certainly there during the years in which NCI rose to prominence. I attended a meeting last year, I guess it was maybe '95, in which Carl Baker was also present. Carl was Dick Rauscher's predecessor.

GC Right.

AU I don't know whether you're going to talk to Carl.

GC I've spoken to him once, and I plan to talk to him again actually.

AU Oh, good.

GC He's been very helpful.

AU I'm sure. He goes back into the very early days of the war against cancer.

GC He's one of the ones that kind of grew up in the NCI family.

AU That's right. I don't think of others on the scene now. You're probably going to talk to DeVita if you haven't already.

GC Tomorrow, actually.

AU Good, splendid.

GC Yes.

AU Someone else whom you might—have you talked to Benno Schmidt?

GC No.

AU I don't know whether Benno is still alive. He moved to Florida.

GC I don't know whether he is either.

AU I've lost touch with him. But Paul Marks, who is the President of the Memorial Sloan-Kettering Cancer Research Institute was on the President's Cancer Panel at the same time with Benno. So he'd have some interesting reminiscences to share with you.

GC Okay. We're at the end of a side. I don't know—have I covered everything? Do you have more to add?

AU I don't know that I could think of anything more? Do you have other questions?

GC Well, I'm going to stop the tape here, and I'll look.

GC Okay. We're talking about Mary Lasker.

AU Yes, you asked if I had met Mary Lasker. I did, indeed. She served on the National Cancer Advisory Board during the first year or two that I was at NCI. I think I met her initially at her apartment here in Manhattan when I was being considered as a nominee. A number of the people in the New York cancer community arranged to entertain me, look me over, I guess, and one of the places I recall visiting was Mrs. Lasker's apartment in the U.N. Plaza. It was quite an impressive place.

GC I'm sure.

GC And you said you knew Benno Schmidt, as well.

AU He was the Chairman of the Cancer Panel, so the Panel met once a month at NCI—usually at NCI, not always—so I had occasion to see him at least once a month. And quite often he would come in advance of the meeting so we would have a chance to put our heads together before the public meeting took place. So I got to know Benno reasonably well.

I remember dropping in on him in his office here in New York City on one or more occasions. A very impressive person.

GC What about Sidney Farber?

AU I met Dr. Farber back in the '50s at the cancer meetings. I never got to know him well, so I don't really have any personal anecdotes. I admire his pioneer efforts in chemotherapy. They changed the landscape. Leukemia in children used to be uniformly fatal, and we now rescue at least half the kids. And Farber was one of the first people to show us how to do it.

GC I spoke with Dr. Frei yesterday.

AU Good!

GC I don't know if you knew him or not.

AU I knew Tom Frei, yes, Emil—a nice man. Again, a very able person. He worked with Farber in the early days at Harvard. You've had a chance to sample a lot of interesting perspectives.

GC Yes.

AU And reminiscences.

GC Yes, absolutely.

AU Good.

GC Okay. One more story about going to China.

AU Right. It was my responsibility during the time I was at NCI to help open a Cooperative Cancer Research Program with the People's Republic of China. In advance of our arrival there, Dr. Li, who was a member of the NCI staff, went over and set up shop, so to speak. He came from a Chinese family and spoke the language, although he'd lived in the States and worked at Harvard. So we had our way paved for us, as it were, as we arrived in Beijing. But I was struck then by the primitive state of their city—very few cars, lots of bicycles—we didn't even find a typewriter at the Cancer Institute, and we took one along so that we could type up a transcript of our discussions, but we left it there in Beijing on loan to our colleagues in China since they didn't have one and had no prospect of getting one.

AU That was 1978.

GC No typewriter.

AU No typewriter—very primitive. I had to go back again about five years later, and I was astonished at the changes that had taken place in those five years. Back in '78 everyone was burning soft coal, there were no tall buildings in Beijing. When I went back it was a much more modern city, lots of skyscrapers and many more motor vehicles. People were much better dressed—a very different feeling in the city of Beijing.

GC Did you have any other overseas adventures while you were Director?

AU Yes. We went to Moscow on a couple of occasions for the Bi-national Cooperative Cancer Research Program there. That was very interesting. I can remember we had an outing from Moscow to a provincial capital, Suzdal', which was a day's drive from Moscow. My recollection is it's to the northwest of Moscow. And I was astonished leaving Moscow, which is a fairly well-developed city, to find myself driving for hours through what looked like forests and through tiny little villages that were extremely primitive and must not have changed much for centuries. Well, I'll never forget it—it was probably late-September when we visited—as we drove into Suzdal in a bus—our party and our Russian hosts were in a fairly sizeable bus—we rounded a curve in the road along the river, it began to snow. And there were old women dressed in black washing clothes in the river in the snow! And I thought, goodness, gracious, what a sight for 1978. You might have expected this in the fifteenth century, or maybe even the nineteenth century, but the twentieth century? It became clear to me then that the Soviet Union was far from a developed country in our sense of the word. The place where they put us up in Suzdal was a modern conference center that they built specifically for entertaining visitors from the developed world. It was rather shoddily built, but quite large. It had a sauna, it had a swimming pool, it had all the trappings of the developed world. The person that was in charge of our foreign relations at the time was a man named Saunders, Joe Saunders, a delightful person.

I'll never forget the dinner at which we were entertained by the director of the Russian program, who kept toasting me in vodka all evening. I don't think I've ever had as much vodka before or since. I don't know how I was able to stagger out of the dinner.

GC I guess you have to do as the Russians do, right?

AU Oh, my. It was a test, I tell you.

GC And that was also 1978 you said that that happened?

AU Probably '77 or '78, yes.

GC How long were you in Russia?

AU Not more than a week or two at a time, not a lengthy visit. But it was customary in those days to have a meeting probably once a year in which both sides would get together. There would be a scientific program in which we would share scientific developments and make plans for some sort of cooperative activity in the year to come or years to come. So we would meet alternately, as I recall, in the Soviet Union and in the States. It was always very interesting to visit the Soviet Union.

GC I'll bet. Was this the same trip that Dr. Zubrod would have gone on with you, because he mentioned—do you know Gordon Zubrod?

AU I know Gordon Zubrod, sure.

GC He mentioned trips over to Russia that he took, as well.

AU I don't remember that he was on this particular trip, but he was probably involved in one or more of the others—could have been involved in this one. Quite often not only would the Directors of the Institutes participate, but members of the scientific staff. One would put on a scientific program to review developments in the area of treatment or diagnosis or prevention, carcinogenesis. It was fun.

GC I bet. What an opportunity!

AU Yes, it was interesting.

GC Okay?

AU Okay? Good.