This is an interview with Dr. Giovanni Battista Gori, who had important roles at NCI, taken on April 25, 1996. The interviewer is Dr. Carl Baker, former Director of the National Cancer Institute.

Baker: Gio, we certainly thank you for your willingness to spend a little time and give us your thoughts about a number of things, including the Viruses and Cancer developments at NCI. But before we get to the questions I sent you, would you give us a little bit of your background on where you went to school and some of the jobs you've had and any other background?

Gori: Thank you, Carl. You know, this is not an imposition on me. I always remember fondly the time we spent together in the NCI and the opportunity that you gave me to be there because, as you remember, you were culpable for hiring me at the National Cancer Institute in 1968.

My career was checkered, as you can imagine. I grew up in Italy in a relatively well to do family.

I attended the lyceum in Italy, which is a sort of a preparatory school for universities, heavy in classics at that time, Latin and Greek and all that, and then I started university

towards a doctorate in biological sciences.

Baker: Which university?

Gori: I moved through three different universities. I was first enrolled in Rome and then, from Rome I moved closer to home which is Padua, and then I started my thesis there with a Professor in Botany who then was transferred to another university, Camerino; so I followed him to this beautiful university, 700 years old, up in the mountains in central Italy. It was like living in a convent, and I finished my doctorate there.

Baker: But I guess you got some sense of history at Padua?

Gori: Yes. Yes, indeed. I remember anatomy lessons in the theater that Fabricius built so many years ago. Yes, Padua, but Rome as well. Actually, of the three, Camerino is the one that preserved the old ways more than the others that are now big universities. It was almost like living in medieval times. I have very fond memories.

Two months after graduation I won a scholarship at the Instituto Superiore di Sanità in Rome, which is the central institute of health in Italy, sort of a miniature of NIH if you wish, and I worked there, first on antibiotics because my background in biology was in soil microbiology. Then I discovered viruses, or

viruses discovered me, because there was a growing interest in *Poliovirus* at that time--it was 1955-56--and I soon was recruited by the Department of Virology and started working on a variety of epidemiologic issues: *Coxsackie viruses, Echoviruses, Adenoviruses, Influenza* and all that, which were the fashion of science at that time in that area. And I was then put to work--

Baker: I guess on *Coxsackie* viruses you came across Huebner's work at that time?

Gori: At that time? I don't remember now. That's so far away that I don't remember details. But I remember that one of the issues in public health at the time was the polio vaccine. And I was put to work on how to regulate the polio vaccines that could be manufactured in Italy, Salk type vaccines at that time. And so I started working on tissue culture and viruses, and *Polioviruses* in particular, working with monkeys and testing this and that, as you can imagine.

One day we had a visit from Jonas Salk himself, and I was one of the few who could sputter a few English words out there and I took Salk and his wife under my tutelage and showed them around the place. I had an old battered car

at that time and took them around Rome and to some of the little villages. We had a grand time.

And two months after Salk departed I got a note from him. He had a March of Dimes Scholarship available. Would I come? And so I spent a year with Jonas Salk in Pittsburgh, and I published a paper with him in *The Annals of the New York Academy of Sciences* on the inactivation of *Poliovirus* and so on. Do I make this too long?

Baker: No. That's all right. Go ahead.

Gori: But you asked me about my background, so it's fun for me because this way I remember; otherwise, I never think about it.

I spent a year with Salk and then I joined a pharmaceutical company in Italy that was thinking of producing polio vaccines. I set up production of the vaccine in the Istituto Sclavo in Siena.

Baker: Now there is a great cathedral.

Gori: Yes, a cathedral. Fabulous. My family roots go back 700 years and come from Siena. Well, I got to know Albert Sabin because he was consulting with us on the production of the vaccine, and towards the end of that year we had a visit from Hilary Koprowski, also interested in polio vaccine. Koprowski offered me a position at the Wistar Institute. And at that time Siena was

intellectually very confining, very provincial, very small, and I did have a taste for America; so for that reason I accepted Hilary's offer. I spent more than a year in Philadelphia working on oncolytic viruses. You remember there was some thought at that time that we could have viruses that would attack the tumor cells selectively. Unfortunately it was not a good lead. I was a bit discouraged, as you could imagine, and Hilary was very sympathetic. I have a great deal of respect for Koprowski as one of the few men that have made a difference in my life.

Eventually I accepted a job offer here in Bethesda at Microbiological Associates as Director of Production and Director of Quality Control, two strange combinations as you can imagine.

I stayed with them for three years, and then I took a job with Melpar, Inc. In Falls Church and I worked with them for quite a while on chemical-biological warfare, NASA projects, and eventually on NIH projects. I remember I had a large contract with the Virus Program, with Manaker at that time as Project Officer, to produce and standardize Moloney leukemia virus.

I set up some of the first isolation systems for animals, like clean and dirty

corridors and things of this sort, which by today's standards, would look primitive.

Then, a vice president of Melpar was enticed to create Litton Laboratories here in Bethesda, and I joined the new group. I set up the laboratories and everything else and in the process we also acquired Bionetics thus becoming Litton-Bionetic s, an acquisition in which I was instrumental. r remember visiting with the Litton hierarchy out in Los Angeles. And that is when I started seeing you. I still don't remember what brought us together first, but we had some conversations, some lunches and- -

Baker: Well, first you asked me to have a look at the facilities which had just been completed at that time, and that was, I think, the way we got started.

Gori: I see, and by September 196B I was with the Ncr as your Deputy in the Etiology Division. From there it's history.

Baker: You may recall that we had a hard time finding someone to head up Etiology and finally I went to run it because we couldn't get anybody else to take it. But that was good operating experience for me because I'd been dOing mostly staff work and laboratory work, so that was good experience.

Gori: Those were seminal years because when I joined

you just had hired Saffiotti to head up the Carcinogenesis Group at that time. Kotin had just left.

Baker: Kotin had left and Hans Falk shortly thereafter, and I needed somebody to head the Carcinogenesis area and Saffiotti was with Shubik and he didn't really want to move to Nebraska from Chicago, so I recruited Saffiotti.

Gori: And then, of course, you had the dominant group of the virus people with Dick Rauscher at that time, a small but influential group in Epidemiology with Bill Haenszel, Schneiderman and a few other people at that time there.

Baker: Mantel.

Gori: Mantel. Also Bryan was still there.

Baker: Oh, yes.

Gori: Yes. Bryan was still at NCI. I was there when he retired I remember.

Baker: We had already appointed Rauscher to head up the Special Viruses Program.

Gori: Oh yes. Rauscher was there.

Baker: Bryan, of course, was a pioneer. He sort of kept the flame alive when nobody thought viruses had anything to do with cancer. And he made important contributions by showing the quantitation aspects of the Rous sarcoma virus. But he didn't take to the management side very

well. It was always very stressful for him.

Gori: I remember him as one of the old fashioned scientists. A very kind, a very good man, a very gentle man.

Baker: But he didn't take to the managerial side. Rauscher did.

Gori: Dick was shining because of his genuine capabilities as a leader of people. He was very good, very effective. And he was doing it with a flair and with an elegance that made it look effortless. He was very, very good.

Baker: I told him though he wasn't quite tough enough sometimes.

Gori: Well, he was tough in his own way. When he had tough decisions to make he'd ask somebody else to carry them through. He was very good at that.

Baker: That's one style of management. Yes.

Gori: Certainly. If you want to last in a position, that's a style that one has to consider very seriously, isn't it?

So, switching a little bit, my first operational assignment, rather than staff assignment, was the Tobacco Working Group in 1968. I first was the scribe on the first few meetings that we had with you as Chairman. Soon after you assigned me to be the Executive Secretary, and from there later I ran and

developed the program as Chairman of the Group.

At the same time I remember we were very involved in what led to the "War on Cancer" legislation that Nixon signed in 1970, was it?

Baker: December '71.

Well, let's go back to the Tobacco Working Group. You remember that first meeting was utter chaos because we got into vigorous arguments on whether cigarette smoking caused lung cancer, because we had on the committee three representatives from the tobacco industry who were outstanding chemists--they were research directors of major companies. They obviously couldn't admit that cigarette smoking caused cancer and yet we had university people and Government people and Agriculture Department people who thought we didn't need any more evidence to answer that question. The purpose of the Tobacco Working Group was to see if we could produce less hazardous cigarettes since people looked like they weren't going to stop smoking. And this was Endicott's decision to set this up and then he asked me to chair it to start it off.

Now the names of the people from industry were Helmut Wakeham from Philip Morris, Murray Senkus from R.J. Reynolds, and Alex Spears from Lorillard.

Gori: And Bill Bates from Liggett and Myers.

Baker: And the only company that didn't want to seem to have anything to do with us was American Tobacco, although when we got into the reconstituted sheets, they came to a meeting on that.

Gori: Yes. They were coming to the meetings but only as observers.

Baker: Well usually they didn't even want to have anything to do with us.

Gori: They came to the meetings several times because the meetings were open, the public meetings.

Baker: But that was more when we got into the reconstituted sheet, as I remember.

Gori: Yes. Well, they came from time to time. They were always on the sidelines. They always remained there. The other people that I remember, of course, besides the insiders like Saffiotti and Schneiderman and Kotin, at the beginning at least, were Charley Kensler from Arthur D. Little. He had always a very forceful and colorful presence. And Ernst Wynder, of course, sometimes with Detrich Hoffmann as well.

Baker: And from Agriculture was an expert on tobacco.

Gori: Dr. T.C. Tso. He's retired from USDA now.

Baker: So, Kotin made a suggestion that for the second meeting I assign different people to review information on the state-of-the-art for different

topics, which I did, and the second meeting went pretty well. And the tobacco industry people were quite helpful, I thought, on teaching us how cigarettes were made.

Gori: Surely, if we had in mind to develop less hazardous cigarettes we couldn't possibly do it without some intervention from the industry itself.

Baker: Well, we also didn't know a lot of things that they taught us.

Gori: Of course. The mechanics of cigarettes. If you remember, they all came in as private citizens. They all sent letters for the record -- unless they've been destroyed, they're still sitting somewhere -- saying that they participated as private citizens, not as representatives of their industries. It was all lawyers' work. The fact is that we avoided controversial issues like discussing whether smoking causes cancer or whatever.

Baker: Well, it was clear we had to get off that because we weren't getting anywhere.

Gori: Yes. And if the policy was to make less hazardous cigarettes, then let's go on with the job and see what we can do.

Baker: Yes. That was the way we approached it. And some rapport started to build, I thought.

Gori: Immediately.

Baker: It sounded like that they had no information on the biology side; that all they had was chemistry knowledge. But they brought a lot of information on the chemistry and the manufacturing processes, but they would never discuss the biology side of things. And then, of course, on the nicotine issue, I got the impression but couldn't really prove it that when we talked about the nicotine levels, every time we talked about getting the level below a certain amount--and I don't remember what that amount was--they became more negative. So, my conclusion was that that was an issue that, again, we weren't going to get anywhere if we went too deeply into that one.

Gori: I don't remember that at the beginning, at least for the first five years to 1975, or something of this sort, I don't remember that we had much discussion about nicotine.

Baker: No because, as I say, I got the impression that if you started talking about getting it down below a certain level they turned off.

Gori: We didn't even think about reducing nicotine until late in the game. At the beginning there was a tacit understanding, even following the 1964 Surgeon General's Report, which said "Nicotine, at the levels used by smokers, is

probably harmless." Therefore we focused on what was thought at that time to be the real problem with cigarettes, namely tar, tar and gas phase at that time, so nicotine was not even an issue at the beginning, although it became towards the end, namely in '77-'78, when we began thinking about the role of nicotine in maintaining the habit. For instance, we funded Dr. Van Vunakis in Massachusetts, and she was the first to produce a radioimmunoassay for nicotine because, up to that time, we didn't have a good assay to test how much nicotine is in the blood.

Baker: In what year was this?

Gori: This was 1976.

Baker: Much later.

Gori: Yes.

Baker: Well, the strategy I tried to develop with this group was that you look at the steps that constitute the making of cigarettes, starting with the different strains of tobacco which contain different amounts of both nicotine and tar level and how that was grown, fertilized, processed, stored, cut up into different sizes of tobacco fragments, the paper, the filters, temperature, all these variables, so that we could dissect out this process and try to lower the tar levels from each, and not only levels,

but carcinogenicity. And the test system we had wasn't ideal--it was mouse skin-painting--but we didn't have anything any better than that.

Gori: Yes. And it was at least--

Baker: And so, the idea was if you could add up these increments, the total sum might be worthwhile. And I think, to some extent, we achieved that with especially the reconstituted sheet because, for some reason tar from this material was less cancer-causing.

Gori: Well, we had not only the reconstituted sheet, as you remember, but there were also two materials that were tobacco substitutes. One was Cytrell from Celanese, and the other one was a product of ICI in England called NSM, non-smoking material, which was a partially oxidized cellulose that would be used as a filler in a cigarette thus producing very little tar. Of course, the problem was it didn't taste like much.

So we had really two developments in my mind that began to shape or to change the philosophy of--

Baker: This was after you were Chairman of the--

Gori: Oh, yes. This happened about 1975.

Baker: So you moved from the Executive Secretary function to Chairman when I became Director?

Gori: Yes. I guess something of this sort. Yes.

Baker: In '69. So I let you have that interesting job?

Gori: Yes. I got that job all together. By 1975 it was clear to me that there was an indication of a threshold in terms of adverse effects of active smokers. If you take the epidemiologic data that we had and you come up with a linear extrapolation, which is the standard procedure for finding thresholds in fields other than carcinogenesis, then you come out with a threshold for tar intake daily. And I remember I published, in 1975, a paper in *Science* that was approved by the National Cancer Institute--all our papers were looked over by Bud Morrison at that time, as you remember--and the paper essentially said, ``Look, from the epidemiology it appears that an intake of less than, I believe, 70 milligrams of tar daily, may be compatible with a safe or less hazardous cigarette.'' And this made sense from a Paracelsus approach, sort of that's the dose that makes the poison. Therefore we started heavily thinking about how to reduce the tar, instead of how to modify the tar, because all the studies that we'd done to modify the tar were marginally effective.

Baker: Not very effective.

Gori: You had some variation but I think it was really

Not a dramatic variation, and so we began

not a dramatic variation, and so we began focusing on how to reduce the tar as we came across the Celanese and NSM materials, and we started looking at various configurations of a possible cigarette.

Baker: Incidentally, I guess you remember that through grants to the Roswell Park Memorial Institute, with George Moore as Principal Investigator, they made cigarettes out of all sorts of things like lettuce, spinach, or cabbage. And I'll never forget that cabbage one. It sure did stink.

Gori: We also--this is a digression I might say--but we also tried marijuana cigarettes at that time. Do you remember?

Baker: Yes. And showed their tar was carcinogenic too.

Gori: Just as bad as tobacco. But I remember the extreme safety precautions or, if you wish, the security precautions that we had to take.

Baker: Oh yes. You had to lock up all this stuff.

Gori: The ATF people come in with guns, you know, and we made 5 million of these cigarettes with "Acapulco Gold". And after the experiment we were left with a million, or so, cigarettes and we didn't know what to do with these damned things. So the ATF says, ``Burn them in the incinerator.'' And I remember we burned them at the Melpar incinerator in Springfield, Virginia,

in their incinerator. After half an hour we had a

crowd of youngsters standing by the incinerator, having smelled the marijuana five miles downwind. When they knew what we were doing a lot of them were crying.

Anyhow, the focus of the Tobacco Working Group at the time, particularly the industry people, switched to the issue of reducing tar, and that was the main impetus behind the general policy of low-tar cigarettes, if you wish, that started in the early to mid-'70s. I would say that the Tobacco Working Group was largely responsible for this switch in policy in cigarette manufacturing and by the end of the '70s, we had some of the first really low-tar brands around, for examples, Merit and Now, and a number of others that came out at that time.

We also were focusing for the first time then on the toxicity of nicotine and carbon monoxide and we started a large study in cooperation with the Heart and Lung Institute, with Dr. Gardner McMillan as the liaison person for the NHLI. And also Claude Lenfant at that time, who was heading the Lung Section of the Heart and Lung Institute.

We had some of the best pulmonary physiologists in the country cooperating on a study where beagle dogs were fed a diet with 5

percent cholesterol to develop atherosclerosis and then they were given cigarette smoke with different amounts of nicotine, spiked with nicotine, and spiked also with carbon monoxide. The study was run for two and a half years at Hazelton Laboratories in Virginia.

We also started a number of studies with Oscar Auerbach with direct inhalation of smoke in dogs.

Baker: Well, he'd been doing that for some time and so I think we enlarged his efforts.

Gori: Well, his first experiments were criticized because he didn't have a good exposure method. He had attached a cigarette directly to a tracheostomy, and some of the dogs were choking to death right away. What we succeeded in doing was producing a machine that would puff the cigarette and therefore offer it to the dogs through the tracheostomy as smoke compatible to what smokers get and would not kill the animals by suffocation.

Actually this machine it was developed at Arthur D. Little by Dr. Kensler and Dr. Battista at that time.

Baker: Well, when I saw his tissue specimens, he still had not actually produced carcinomas, but the sequence of events made it look just exactly like

the sequence in humans. I understand that he eventually did get carcinomas produced.

Gori: No, he never did.

Baker: I thought he finally did.

Gori: No. The trouble is his dogs were dying too soon, you see, because they were inhaling too much carbon monoxide, or whatever.

With the new machine developed by Arthur D. Little, they could survive, and so we had studies that could last for 2-3 years with substantial measured amounts of smoke getting into the lungs of these animals. Unfortunately, if you gave them smoke that is compatible to what smokers get, even say 5 times stronger, they don't develop lung cancer, even after 3 or 5 years. The thing was very disappointing. As a matter of fact, the study at Hazelton was very anticlimactic because the dogs that had the highest nicotine and the highest carbon monoxide spiked into the smoke itself had the lowest arteriosclerotic lesions. It was the contrary of what you would expect.

Baker: Well, as you know, if you look at dose-response curves, they're often peaked so that you have lower production of, in this case, tumors at the low end and the very high end, and the highest instance--

Gori: In the middle. A U-curve.

Baker: Yes. A lot of drugs work that way, so perhaps that's what it was here.

Gori: This was the result of the study at Hazelton which was never published and it was quite anticlimactic.

By 1977 though we had a switch of policy within the Department, as you remember, with Califano coming on board. The old policy of trying to figure out less hazardous cigarettes was discarded in favor of a non-smoking policy, and the Tobacco Working Group slowly was disbanded. There were other forces as well that contributed.

At the beginning of the '70s, as you remember, the Institute had more money than it could possibly spend.

Baker: No. I disagree with that.

Gori: Well, NCI spent it, but a lot of that money went into construction and training.

Baker: As you know, since I am an advocate of good planning, the current plan should always be well ahead of the monies you have, so you always are ready to exploit additional opportunities for good priority work. So I never would agree that we had more money than we knew what to do with.

Gori: You're right. That is a poor characterization.

Baker: By '77 I'm not sure planning though was in the process like it was before.

Gori: What I'm saying is at the beginning we had enough money to fund some "pet" or "interesting" in-house projects--the Virus Program, the Carcinogenesis Program, Smoking and Health Program, and the Nutrition Program, which also I started, you'll remember, under pressure from the Candlelighters at that time in '74 or '73--and so we had the money--

Baker: Well, the National Cancer Act gave you a big boost in money.

Gori: Of course. We went from--

Baker: That was the main origin of the big increase in funds.

Gori: Correct. But at the beginning we had monies to fund these in-house generated programs. By the end of the '70s, the training and the new construction had created such an outside need for money that people began looking with a jaundiced eye to everything that was done in-house, and so in-house programs inevitably suffered. This was true for all the projects, and Smoking and Health probably was the best target because, again, see, the policy of Califano changed it from something that was worthwhile before to something that was not worthwhile all of a sudden. So the program

really suffered and gradually disappeared. By the end of the decade it was essentially nonexistent.

Some of the large contracts were aborted before being finished.

Baker: You left the NCI shortly after--

Gori: I left the NCI in May, 1980. I took a sabbatical in 1976, largely to escape the wrath of Califano. I took a sabbatical at Hopkins. I got a Master of Public Health and then came back.

Baker: That must have been when I was in Switzerland, because I wasn't aware of that.

Gori: No. You were already with the Ludwig Institute at that time. Rauscher had left. He probably had sensed the problems ahead and jumped at the opportunity that was offered to him at the ACS. So we had there an interregnum with Guy Newell first as an Acting Director and then Upton and then, after Upton, Vince DeVita. I left just a few months after DeVita became Director of the Institute. But of course my decision was dictated not only by the Smoking and Health controversy; I also had a problem with the Nutrition Program which I founded in 1973, and that also had become prosperous. We had terrific input from probably some of the best nutrition minds around the country at that time. We had a

~~minds around the country at that time. We had a~~ plan and everything else and it was presented to the Board. I still remember Benno Schmidt at one of the Board meetings getting up against me and saying something like ``Hey, youngster, who are you to suggest that diet has anything to do with cancer? Are you kidding?'' or something of the sort. The fact is that when the Nutrition Program needed funds, that coincided with the time when funds for internal projects were beginning to be more scarce. The Nutrition Program also continued limping over the years but never went anywhere.

My other difficulty during those years were on two other fronts. First of all, as you remember, I was Deputy Director of the Division of Cancer Cause and Prevention, and prevention initially was thought of as a money-saving device and it dawned on me that, after all, when we're talking of preventing diseases of old age -- cancer and cardiovascular diseases and all that -- we should take a look at the economic issues. And I started an econometric study, I remember, at that time which showed incontrovertibly--

Baker: What kind of study?

Gori: Econometric.

Baker: Oh, yes.

Gori: -- that if you prolong the life of people that are

already retired, in the end it's going to cost a lot of money. And I remember passing this draft of the manuscript to the NCI, that is to Bud Morrison, who then sent it also to Building 1, because it was looking at prevention in general, not only prevention of cancer. And I still have a letter in my files from Don Fredrickson forbidding me from publishing that paper.

Anyhow, the paper appeared in *Science* in 1976, and it was the first paper to show that while prevention is a laudable goal, let's not kid ourselves that we're to save money. Let's be prepared. If, in fact, we're going to have all this surge in elderly people at the end of the pipeline, let's be prepared to provide for them. That was one of my problems with the NIH at that time.

The other problem was my increasing reluctance to consider bioassays in animals as legitimate tools for determining carcinogenic risk in humans. I just could not be persuaded that animal tests could be reasonable predictors of human risk. There was no obvious link that one could make between animal responses and human responses, especially by using maximum tolerated doses which were the standard of the testing at that time and even now.

I remember having many discussions with a variety of people and many differences of opinion that really put me on the fringes of thinking at the NCI at that time. I started at that time, the so-called National Clearinghouse for Carcinogens.

Baker: National Clearinghouse?

Gori: The National Clearinghouse for Carcinogens. It was 1976 or something of the sort, with Jim Peters, at that time Director of the Division, and Gary Flamm also Assistant to the Director at that time. And I was hoping that the Clearinghouse would put some good sense into the area, but unfortunately it was taken over politically by a variety of forces that actually reinforced the idea of using bioassays for human risk assessment.

The thing though became sufficiently controversial that by the end of the decade, by '78, there was some movement to transfer the bioassays from the NCI to the newly formed National Institute of Environmental Health Sciences.

Baker: David Rall--

Gori: Yes. This transfer took place when David Rall was in-- Yes, Kotin left before, much before, that. You're right. Dr. Upton masterminded the transfer of about $200 million dollars at that

time--I don't remember exactly but it was a very large figure--of money from the NCI to the NIEHS. It was somewhat of a laborious operation, and the signing over of the program was actually done by DeVita just a few months after Arthur Upton left.

In 1980, a month after I left NCI, I published a paper in *Science* on the precariousness of using animal bioassays to determine human cancer risk -- my valedictorian, so to speak, after leaving the Institute.

Baker: So where did you go then?

Gori: After that I went to the Franklin Institute in Philadelphia with a generous grant from the tobacco industry.

Baker: To do what?

Gori: Well, they left me alone. They simply gave a million dollars to the Franklin Institute to endow my position there, and I was left to do whatever I cared to do. I didn't have to work for the industry or for anyone else.

Baker: So, what did you do?

Gori: I worked mostly on risk assessment issues and bioassay issues, and I published several papers during the first year.

Baker: Risk assessment is not a simple matter.

Gori: Well, no, it's not a simple matter, I agree, but my firm conviction is that you can do risk

assessment, or risk-benefit analysis if you wish, only if you have some objective way of determining risk. If you don't have an objective way of determining risk then you cannot do an objective risk assessment analysis nor a risk-benefit analysis for that matter.

Baker: And it's very hard to acquire this in some areas.

Gori: Sometimes it's virtually impossible to acquire it. So you have to say that under certain conditions you are going to apply your judgment, your prudence, but don't come by and tell us that you're going to apply science. We wish scientists had all answers. But here we act as if we had scientifically valid data to do this kind of analyses and, as a scientist, I must say I resented this and I still do today.

Baker: Well, I've been concerned more and more at NIH that political decision-making is being brought to bear in scientific issues. When I was there I didn't think we had that so much. And Congress is the main source of this, but that's not the only part of it.

Gori: Now you remind me of one episode, Carl, that involved you and me. This was about 1972. And that's when I became also interested in ultraviolet radiation in cancer. At that time there was a big discussion in Congress about the

~~there was a big discussion in Congress about the~~ SST, the supersonic transport, and there was all of this hullabaloo about the SST flying so high and destroying the ozone layer and bringing down all this ultraviolet radiation. And so there was an inquiry from the White House to the NCI--to the NIH actually--which came down to you, and you transferred the job to me, to assess whether, in fact, if we increased the radiation we would have more skin cancer.

I remember coming up with the conclusion: ``Yes, if you have more ultraviolet radiation, you have--'' And so I was summoned to testify before a committee led by Senator Proxmire from Wisconsin. The morning before, you came into my office in Building 31. I remember you were obviously uneasy as you said, ``Gio, are you going to be sick tomorrow morning?''

Baker: Are you going to be what?

Gori: ``Are you going to be sick tomorrow morning?'' (Laughter.)

Gori: And I didn't know what you meant, but then you explained to me, you said, ``Look, I just got a call from the White House staff and they would much prefer if you wouldn't testify tomorrow on this issue.''

Baker: And did you get sick?

Gori: Yes. I got sick that morning and, of course, the next day you have ``Government Scientist Gagged by the White House'' and my picture was in *The Washington Post*. That was my first--

Baker: Nobody gagged you.

Gori: No. Of course. That was my first, how do you say, serious encounter with the media.

Baker: But also that may have led you to be more interested in risk problems?

Gori: Well, sure. It certainly was an educational experience. But I still remember your coming in somewhat sheepishly closing the door and saying, ``Gio, aren't you going to be sick tomorrow morning?''

(Laughter.)

Baker: At least I asked you; I didn't tell you that you had to be sick.

Gori: And then I remember very well you told me, ``You can do whatever you want to'' you told me, ``but I have to tell you this.'' You know?

Baker: That was the policy of NCI. We think you shouldn't do this, but we're not--

Gori: Not proposing anything. Today--

Baker: And that's still true of Wilhelm C. Hueper, although a lot of books like *Cancer Wars* again and again go through a litany of how Hueper was gagged by NCI.

Gori: Yes.

Baker: Well, it wasn't NCI.

Gori: It was somebody else. Yes. But anyhow, that was my first political exposure at that time.

Baker: Well, it's gotten much worse, I think.

Gori: Yes. Today, if I refused an order I would probably be sacked.

Baker: Creating the Office of Alternative Medicine is a good example of stupidity.

Gori: Yes. All these things and all this talk about scientific misconduct.

Baker: Well, that's been blown way out of proportion.

Gori: I mean, what are they doing, these guys?

Baker: Scientists are no better or worse than anybody else, but the system will reveal fraud. You will get caught if you--

Gori: You will fail. If you screw up--

Baker: You can't fake data very long.

Gori: That's right. If you screw up, eventually you get caught.

Baker: Now, if you're going to make it on the basis of political decision-making, that's a different arena.

Gori: That's a different story. That's not science then.

Baker: We'd better get on to the questions, I think. Do you have those questions?

Gori: Yes, I have those questions, but I must say that

without names attached they're going to be very difficult.

Baker: Well, that's all right. We've looked up a lot of names, so that doesn't matter.

Gori: Shall I read the question?

Baker: The first question, yes, deals with five or so main--

Gori: What were the most important scientific results?

Baker: Yes, just in your view what stands out as those most important landmarks, so to speak, in Viruses and Cancer.

Gori: Now you remember that my involvement with the Virus Program was mostly as a planner, but I think that the reverse transcriptase was probably the key--one of the key things--because it opened the door to genetic engineering and to everything else that we have today. And probably it's the single most important contribution of the entire program.

Baker: Well certainly one of the most. Oncogenes might be even more significant, but they're very related, of course.

Gori: Yes. Without the--

Baker: It's all part of the same story really.

Gori: Yes. Without the transcriptase issue you wouldn't be able to do much with the oncogenes. We talked about oncogenes even before the Virus

Program was put together. Everybody was thinking that you might have some genetic loci that are responsible for cancer--

Baker: Yes, but that was in general terms and we didn't have any evidence in those days.

Gori: Correct.

Baker: So, I agree with you, this was key because it shifted our main thinking from viruses causing cancer to information of a certain genetic coding which was not only involved in cancer causation through oncogenes, but from the work of Bishop and Varmus this coded information is in our own chromosomes aside from the viruses, but the viruses allowed you to identify the cancer causing genome sequencing.

Gori: Correct. Correct.

Baker: So critically this clearly was one of the highlights. And so the actual discovery of this, of course, was credited to Temin and Baltimore, although apparently John Bader really had a publication on this earlier.

Gori: Yes. Of course, this goes, in my mind at least, beyond what the cancer virus did. You had the seminal work of Monod and Jacob, say, with the bacteria at the beginning, but then the real first breakthrough came with Marshall Nirenberg when he was first able to produce the coded

protein.

Baker: That sort of put the icing on the cake.

Gori: That's right. I remember at that time I was with Microbiological Associates, and I suggested at that time that we should start thinking about producing vaccines, acellular vaccines therefore, because the instrumentation would have been there, even at that time. The specifics might not have been there but--

So, anyhow, reverse transcriptase and the oncogene issue probably are the two stellar achievements of that program, and I think that the expenditure of a few million dollars, a few hundred million dollars, over several years amply is justified simply on these two accounts.

Baker: Well, if you go back, you see, the production of the viruses in quantity, which was part of the program, Baltimore and Varmus and Bishop all received huge amounts of this material which would not have been available had we not had the Viruses Cancer Program. Now, if you go back earlier, there are some key steps in this process because, as you know, earlier nobody thought viruses had anything to do with cancer, and Peyton Rous finally just stopped working on it because nobody thought it was important.

Gori: Nobody paid attention. Yes.

Baker: So, what was the change there? What was the key?

Gori: Well, you had Bryan.

Baker: Well, he sort of kept the flame alive.

Gori: Did we have the program at that time when Bryan was there?

Baker: No, not early.

Gori: Well, it was then Rauscher and Moloney, say, that came in with their viruses.

Baker: Well, scientifically I think there is a key step there. Well, I'll throw it out for your view. I think that the findings of Ludwik Gross and Stewart and Eddy that you could actually transmit leukemia and sarcomas with cell-free preparations changed this whole outlook about viruses and cancer, and so I think that's a very key landmark. Now you recall that nobody believed them at first, so it took two years before anybody believed them, and that was 1953. And then you had this whole spate of cancer-causing viruses and, when I left the Institute, there were over 200 viruses that had been isolated that could cause tumors in animals and yet we had hardly anything in humans. So I consider that a key one because it shifted, in modern terms, the paradigm.

Gori: No question about it.

Baker: Now, one thing that a lot of people don't realize

is that preceding the Viruses Cancer formal Program, Harvey Scudder, first as Executive Secretary of the V&R Study Section and later after he moved with Ralph Meader in the NCI Grants area, developed funding for resource production. So the outlining of what kind of resources were needed was done with Harvey Scudder getting together a lot of outstanding virologists on what they needed. And so they started all that before we had the formal Special Virus Leukemia Program.

But that allowed further expansion when the program was set up. So, there was a million dollars earmarked for the Grants Area on increased virology after Wendell Stanley's testimony. And then when the $10 million dollars was requested, in '64 I think it was, we began to pull this together in a more integrated program, multidisciplined, and this effort led to the production of quantities of resources which allowed, I think, the pace of research to speed up.

Gori: I was part of it when I was with Melpar. We were producing Moloney leukemia virus by the gram at that time.

Baker: So those, I think, are key elements.

Gori: And, of course, you had--aside now from the

scientific--you had the organizational skills and the tenacity of certain people, like Bob Huebner and his pupils like Todaro and--

Baker: Collaborators like Wally Rowe and Janet Hartley.

Gori: Correct. And these people really were the heart of the program. And I remember going to many, many meetings organized by the Program.

Baker: Now, you were there when the Zinder Committee reviewed things?

Gori: The Zinder Committee? Yes, I was there. Yes. I don't remember much of that time, but I was there.

Baker: You didn't get involved in answering them?

Gori: Now, the Zinder Committee came out in 1975 or '76?

Baker: Somewhere in there.

Gori: At that time I was embroiled with all the other issues in smoking and nutrition and everything else and I didn't have much time to--

Baker: Moloney, I guess, bore the brunt of answering the criticisms.

Gori: Yes.

Baker: So you didn't get involved in it that much?

Gori: I didn't get very much involved in that. No.

Baker: You didn't sit in on a meeting, say, with the Cancer Board when the Zinder issue was reported?

Gori: I might have. I don't remember it now.

Baker: You weren't involved in it enough to matter?

Gori: No. That's right.

Baker: Well, let's go to the next question.

Gori: The next question. "What do you think were the key administrative or management decisions affecting the Virus Cancer field from 1950 to 1980?"

Well, first of all, you said the first decision to assign one million dollars to the program in the early '60s. Well, before that there was no program. You only had these developments that we were talking about.

Baker: Just Bryan was about the only one--

Gori: That's correct, that was keeping, as you say, the flame alive. But the program started, I believe, in '64 with the first million dollars.

Baker: Well, the million dollars was before that. The $10 million was '64.

Gori: The $10 million. Yes.

Baker: And that's when we had to form a program because Endicott, when he received approval from the Congress for this, came in to Carrese and me and said, "Okay, you guys have been talking about planning, plan me a $10 million dollar program." And so we pulled Rauscher with us, spent three weeks sketching that out.

Gori: And it worked.

Baker: Well, I think it was a good thing. Then we did Chemotherapy right after that, which was easier. The Chemotherapy people used those plans; the virologists, we might as well not have done it, as far as they were concerned.

Gori: Well, if you remember, we went through several reprogrammings of the Virus Program.

Baker: Well, sure. Any planning should be updated about every year and a half anyway.

Gori: It was an ongoing process and I remember working with Lou Carrese and all the others, with Moloney and Rauscher and several of the others, of course Bob Huebner, Manaker, in planning exercises twice a year practically to refocus things and move them in a particular-- I still have some of those charts with me somewhere, I'm sure.

Baker: The justification for the $10 million, you probably didn't have much to do with that.

Gori: No. It was five years before my coming here.

Baker: But Bryan, Rauscher and I, with Zubrod commenting on it, developed a fairly long memo that went over to Shannon from Endicott asking if he could request a special appropriation from Congress. As usual, Shannon wanted more information; so there was a second memo which I signed as Acting Director providing more information. So, we had quite a bit of justification for backing up the

request for a special appropriation, which was kind of unheard of and we weren't sure that the Department would agree with this, but Shannon, in those days, was in a position to do pretty much what he would approve of.

Gori: Well, of course, he had some powerful allies that even you guys had, like Mary Lasker and the ACS at that time.

Baker: I don't think I would call Mary Lasker an ally of Jim Shannon.

Gori: All right. Was she a competitor?

Baker: No. In a different direction. Political. Shannon stuck to high quality science as his basis. They both made great contributions but in very different ways.

Gori: I still remember, Carl, just a month after you hired me, you escorted me into Jim Shannon's office, and I must have been one of the last people hired at the NIH that shook hands with Shannon as the Director because he stepped down probably the beginning of '69, or something of the sort. I remember you had the courtesy of walking me to Shannon's office where I met the man and he bid me good speed. "Do well young man," or something of the sort.

Baker: Well, good.

Gori: That was very nice of you.

Baker: So I think Endicott made a very key decision to go after the special appropriation because that was his decision based on material we put together for him that led him to that conclusion. I think a lot of us were not giving enough credit to Harvey Scudder, although I frequently mention his role, in really starting the idea that he ought to be providing resources in quantity because each individual scientist couldn't make enough.

In fact, I told some of these guys who came over from the Polio Area that it was very impressive how they exchanged samples for testing quality control, but by the time they sent samples around to all their colleagues they didn't have anything to work with and that industry is the way to do this. And they said, "Oh, well, they can't make it good enough."

"Oh? Well, we're not going to ask you to use something that you don't think is any good. You can apply the same tests as you've been applying."

"Well, they won't make it good enough."

And so, as I've mentioned before, I knew we were over that hump when Moloney came in one day, all excited, that Pfizer had just sent him a batch of Moloney virus and, "It's just as good as

anything we ever made and we've got buckets full of it." And so we demonstrated that it could be done. And I think that had a real effect on the future.

Gori: "What do you consider to be the main activities and effects of your participation in this field during this period?"

Well, I was connected with the program only since '69 essentially. Even though I was a virologist by training and a contractor to the program in the production of some of the viruses in large-scale, I really didn't have much scientific input at that time other than participating in the planning with Lou Carrese. As I said, every three or four months we would meet and bang our heads and see how it should be steered, the whole effort.

I remember I was involved with the transcriptase issue because once that came up it became an inter-institute issue at that time. There was also an effort to connect with an Italian discovery of rifampicin, if you remember, a substance that could interfere with the transcriptase. And I remember going to Italy, having those people coming over and putting some liaison with our own people.

Baker: You did speak Italian?

Gori: I did speak Italian. Yes. It was probably the only reason why I was put there at that time. But again, my involvement with the Virus Program was mostly as a friendly observer and in helping as almost a disinterested party, if you wish, in the planning process with Lou Carrese and others. And, of course, I was involved administratively as Deputy Director of the Division with many of the administrative issues--allocations, personnel problems and what not--and they had a lot of personnel problems because the program was full of prima donnas, as you would expect. You have to have personal competition, but that made for very tough personal animosities, sometimes coming down to difficult situations.

I was too involved professionally with the Smoking and Health Program and the Nutrition Program and the Carcinogenesis Program. I was attached to the Carcinogenesis Program as project officer for the research agreement with the Oak Ridge National Laboratories, for the Shubik contract out in Nebraska, for the efforts at the American Health Foundation in New York. At one time I was project officer on probably 80 percent of the contracts for the Carcinogenesis Program, so I was very busy with that particular program.

Baker: Now, one of the criticisms of the Zinder

Committee was, of course, that these projects funded with contracts were not reviewed with proper peer review. Do you have any comment on that?

Gori: As far as the contracts that I was running I don't think it was true because we had input, not only from several in-house people, but also from outside people.

Baker: But somehow the belief in the academic community is that we didn't do that.

Gori: Oh, yes, we did it extensively I would say. Now, somebody may say that we hand-picked the reviewers.

Baker: So what else is new?

Gori: Yes. We hand-picked them because they were good people scientifically.

Baker: Because they were among the most knowledgeable for the subjects at issue. And this whole conflict of interest issue gets blown out of proportion.

Gori: Oh, it's a bunch of baloney.

Baker: You cannot avoid some conflict of interest if you're picking the best people because they're the ones you want the advice from.

Gori: Yes. We made great use of people like Gerry Wogan, for instance, from MIT. Why? Because he's a good man, including his scientific

expertise. He had always very incisive comments and very good suggestions, and so why not use a guy like him?

Baker: And yet he was the recipient of funds, so obviously a conflict of interest.

Gori: That's a different story. You know? He always had very good reasons to be funded. I mean, he got funded, you see, but he also was part of the peer review process because he was a good peer reviewer. No, I must say that, at least in the contracts that I was responsible for, including the Eppley contract, we had what I considered classy peer review processes. The actual recipients of the contracts were very happy with them because they helped in many instances to shape a better program, as they should. And I don't remember any instance where suggestions made by a peer review group to modify or change a particular contract were ever objected to by the people on the receiving end.

Baker: Well, the criticisms were really the reverse; that contracts were awarded with insufficient review by outside people.

Gori: No.

Baker: Well, later on, I gather there was some change of structure of the review groups which made it stronger with in-house people and fewer outside.

Gori: I don't remember the specifics now, Carl, but we went through so many changes in policy, as you remember, during that time, that it's difficult to remember the specifics. The only thing I say is that we made our best effort to have people review those things for their scientific content and I think we have done as good a job as could have been done under the circumstances.

Now somebody could say to have 12 people review the Eppley Contract, which was one of the large contracts at that time, was not enough; we should have had 30 or something. But can you imagine what you would do with 30 people?

Baker: Well, look at your National Advisory Cancer Board. There were 200 people in the room when I went to a meeting a couple months ago. That, to me, is sort of ridiculous.

Gori: That's right. It gets to be a parliament.

Baker: Well, what happens, of course, you have executive committees that get back to a functional group.

Gori: Yes. But I think that peer review was done-- First of all, you cannot please everybody. Right?

Baker: Right.

Gori: And most of the people that were displeased at that time were displeased at a political level, rather than anything else as far as I'm

concerned, or because they didn't get all the funds that they wanted to have. But nobody did.

Baker: Well, of course, the argument mostly from the academic community is that you were taking money away from grants.

Gori: Well, perhaps, but we put eventually money more in grants, as you remember. Also, we had those Cooperative Agreements and things of that sort, or unsolicited proposals, you remember, that we had to develop because we felt that--the Institute--continued to feel that there had to be some programs that they centrally directed and, if the contracts were--

Baker: And resource production is a different thing than exploratory research.

Gori: That's correct.

Baker: And they were used to evaluating projects on the value of exploratory research, and the resources contracts were an entirely different world. Now, whether you should have research contracts, rather than resource contracts, is a debatable question.

And then Huebner lined up so many people, mostly on the West Coast, that the amount of money that the Zinder Committee figured Huebner controlled seemed unreasonable to them Well, I related Huebner to General Patton. If you really

want to tackle something, why not pull together the various capabilities under one leadership to get it done? And Huebner was very good at that.

Gori: Oh, he was excellent.

Baker: But somehow they thought he had too much money, and worse, too much control. It wasn't fair. I must say that fairness issues interfere with efficiency. The Committee seemed to ignore the fact that the results were outstanding.

Gori: Yes. First of all, Huebner didn't gain a penny out of this thing here. He still was living on a farm with cow manure all over the place. Do you remember going to parties at Huebner's?

Baker: Yes.

Gori: You had to watch where you stepped all the time. But he did it really out of a personal commitment to do something well.

Baker: Yes. So I always thought it was very unfortunate he--

Gori: People that do something are always going to be criticized in a so-called "democratic" system like we have.

Next question. "Who do you think were the main leaders that influenced the direction and course of events between 1950 and 1980?"

We spoke--we already mentioned--most of the leaders in this. But I might say even you and

Endicott and Lou Carrese, I think, had pivotal roles in shaping up the direction, and the emphasis, and the justification for this program.

Baker: I'm also after people outside. For example, Sidney Farber, although he wasn't in viruses, was supportive, and Mary Lasker, and then guys like Melnick were very helpful.

Gori: Yes.

Baker: Chuck Evans of the University of Washington was an early Committee Chairman and one of our best advisors on getting things done effectively and quietly.

Gori: Also the gentleman from McCardle, I've forgotten his name. Rusch?

Baker: Harold Rusch. Yes. These people were all, I think, very helpful. Syverton, before he died.

Gori: Yes. Gross himself, I believe.

Baker: Eventually. Once he finally got confirmed with his work.

Gori: Rubin.

Baker: Harry Rubin. An interesting fellow. Yes.

Gori: Dulbecco.

Baker: Well, Dulbecco got the Nobel Prize deservedly for quantifying viruses and tissue culture, also very important. So the methodologies, as well as the resources here, really set the stage, I think, for biotechnology and the movement of molecular

biology.

Gori: Everything that we have today.

Baker: And so I think one of the best things about the Special Virus Programs was laying these foundations.

Gori: Absolutely. Absolutely.

Baker: Even though now, on the 25th Anniversary of the signing of the National Cancer Act, everybody seems to think cancer research started with the signing of the Cancer Act.

Gori: The big money started at that time.

Baker: Even the reverse transcriptase was discovered before the signing of the National Cancer Act.

Gori: Yes. That's true.

Baker: But the way the story gets told is that it started with the National Cancer Act of 1971.

Gori: It's their perception.

Baker: Yes. They seemed to be saying that it all started with the Act and little cancer research was done prior to that. Which reminds me, did you have anything to do with the Organ Site Programs? Do you know what I'm talking about?

Gori: Yes. No, I didn't.

Baker: Because Gerry Murphy, who headed one of them, seems to think that that started with the National Cancer Act, and it didn't; I started it in '69.

~~in 1969.~~

Gori: Yes. "How aware are you of membership on key NCI/NIH advisory committees at that time and what do you think were the main contributions of these committees? What of individual consultants? What of lay individuals, including political figures? What of NCI staff?"

Well, politically you say Mary Lasker and Senator Magnuson were really pivotal, I believe, in the final act of appropriations and everything else. Don't you think that was the case at the political level?

Baker: Well certainly in getting the National Cancer Act through. But, of course, she wanted to pull NCI out of NIH.

Gori: Oh, well, that's a different story.

Baker: And when I told her I was opposed to that, she sort of cooled toward me.

Gori: That's right. She cooled real hard, didn't she? Is she still alive?

Baker: No.

Gori: She died? Yes, I remember spending many, many evenings in her suite in Sutton Place summoned there by "her majesty." She was sort of a--

Baker: An interesting woman.

Gori: --an interesting ``queen'' in her attitude.

Baker: Very effective.

Gori: Opinionated.

Baker: Well, sometimes she was a pain but, by and large, on balance, she did a lot of good.

Gori: Yes, she did. She did. Other people on NCI committees? Let's see if I can remember. I remember Harold Rusch. I remember Evans.

Baker: Names and dates are the hardest things to think of.

Gori: Yes, to come up with.

Baker: I've been reviewing committee memberships so I have lists so I can put all this together.

Gori:

``How significant is the availability of quality controlled resources to the advancement? How did such availability affect development? And compared with the period '55 to '75, what were the roles...''

Well, as we said before, without technology you cannot test hypotheses. Right? I think that Karl Popper, himself, said that hypotheses are easy; it's the testing of hypotheses that is difficult. Yes. To imagine things - its easy: its to test hypotheses that takes time, resources, machinery, and new gadgets. So, in this respect, the achievements of the Virus Program would have not been possible without the massive resource effort that the program set

afoot. So the answer to this question is, the

development of these resources was essential.

Baker: Today, a lot of stuff that is commercially available, of course, was worked out in the program because you couldn't buy a lot of these things in those days.

Gori: Of course.

Baker: So this is part of the groundwork for biotechnology, I think.

Gori: Many of the techniques developed at that time were transferred practically unchanged to a lot of things that we're doing today in biotechnology.

Baker: That's what I mean.

Gori: Yes. Including the industry that supplies biotechnology firms today with raw materials.

Baker: Yes. That's what I mean.

Gori: Most of these companies are--

Baker: Now you can buy kits to do stuff that it took you three months to set up for two experiments or something.

Gori: Yes. And most of these start-up companies have roots in people and organizations that were funded and started by the Virus Program 20 years ago. Absolutely. Even the stellar names of that time, like Bionetics, now Litton Bionetics, even Microbiological Associates, GBI and all these, they were all start-ups from the Virus Cancer

Program. They wouldn't be here if we didn't have the Virus Cancer Program. Something else might be here, but--

Baker: There are issues about whether the taxpayers setting up people to make big fortunes, you know--

Gori: Without taxpayer money we wouldn't have had the Virus Program.

Baker: No. Of course not.

Gori: And we probably wouldn't have the biotechnology industry today either.

Baker: I don't think so.

Gori: So I think that it is a proper role of spending public monies for seeding technologies that obviously are going to be useful, even if you have to seed them with a degree of serendipity, because we really didn't know whether the Virus Program would work when we started off.

Baker: Well, I must admit, I'm a believer that mankind is better than just depending upon serendipity. I think we can do better than that. I mean, I'll accept it whenever we get it.

Gori: We did. But when the program started--and I was not there, but you were there--I don't think we had any guarantee that the work--

Baker: Well, of course. You don't have any guarantee on any of this.

Gori: So you had to put some faith in it.

Baker: Of course.

Gori: That's what I'm saying, "serendipity" in this sense here.

Baker: And I was very careful not to promise--

Gori: Anything.

Baker: --because I didn't know. And I think DeVita made a mistake on saying we were going to have the certain data by the year 2000.

Gori: Yes. It's a tough--

Baker: And many scientists were very fearful of over-promise? At least I didn't fall into that trap.

Gori: "During 1950-80, are you aware of the relative funding of Virus Cancer grants and contracts and what each area contributed?"

Well, most of the contracts contributed to the logistic effort here and, of course, one thing that is not mentioned here is how much money was spent in-house. I really don't have an idea of the relative sums spent.

Baker: Most people don't. I'll work that all up.

Gori: Yes. Over the years, because it varied from year to year most likely.

Baker: We certainly increased contracts very rapidly when I was Director.

Gori: In the mid-'60s. But even in the late-'60s. Contracts, after the $10 million dollar

allocation I think that the contracts took off.

Baker: Yes. And the grants didn't go up percentagewise nearly as fast, and that led to some academics and people objecting to the whole thing.

Gori: But then you had a squeeze in the other direction probably in the mid-'70s.

Baker: Yes, because Rauscher and Benno Schmidt and particularly Upton moved back to the grants philosophy.

Gori: Well, Rauscher had to.

Baker: I don't know about that.

Gori: Oh yes. Rauscher had a good political nose.

Baker: But is that what he should be directing at?

Gori: At that time yes, at that time, because when you begin to have such large amounts of money in an operation like this--

Baker: Oh sure, politics is bound to get into it, but all the more reason to try to convince the politicians.

Gori: You tried, Carl. You tried.

Baker: All right while it lasted.

Gori: It's too bad, because you were a champion of reason and of dedication to science.

Baker: Well, I kept my eye on cancer. We were there for cancer research, and that's partly why I didn't really want Cancer Control because I thought we had our hands full with trying to do the

research, and we didn't have all that much to add into control and, with all due respect, nutrition isn't very clear yet.

Gori: I agree with you on not only that Carl, but the first hearing on nutrition was done by Senators McGovern, Humphrey and Dole, and I was the one that set it up because I was there at that time. And so we came up for the first time with the recommendations for the American diet. You remember? Less fat and things of this sort. Today we make the same recommendations. What have these guys done in 30 years, or 25 years? Nothing.

Baker: Well, the data just aren't that good.

Gori: Well, they haven't changed. They have still do the same kind of--

Baker: It's what in physics we would call "soft" data.

Gori: It is. Yes. It's hypothesis.

Baker: It's a little more than that but--

Gori: Well, I don't think so, because we have not been able to verify that they are true. All the major intervention programs, like the MRFIT, the Framingham Study and things of this sort, they are failures.

Baker: Not in heart disease.

Gori: Well, in terms of mortality, in terms of overall mortality, they're failures.

Baker: Oh no, because in the heart field now it's been going down.

Gori: Yes, but we don't know why.

Baker: Yes. I think part of it is dietary differences and some of it is new drugs, but the complexity is the problem here.

Gori: It's a terrible complexity. But if you see the MRFIT Program data published a few years back in *JAMA*, the special intervention group, namely the one that had better diet, less smoking, a better hypertension picture, has 20 percent more lung cancer than the other ones that lived up to their regular drinking and smoking and be merry, and what not. 20 percent more. It's in the *JAMA*. You don't hear these things around too much.

``If you could have changed anything in the virus cancer field as it developed, what would you like to have changed and how?''

Baker: Yes, that's an interesting question.

Gori: I would have kept Dick Rauscher there for a little longer. Dick had a managerial skill that I think was sorely missed when he left. This is not to cast aspersions on John Moloney. John Moloney is a very effective, honest, and very capable man, but I don't think he could match the managerial and personal skills that Dick had.

Baker: Well, it's hard to match Rauscher's personality.

Gori: Moloney is an introvert by nature and Rauscher also is an introvert, but he was faking it very effectively.

Baker: Well, sort of like me. I'm an introvert really, but I learned how to act like I'm not.

Gori: That's right.

Baker: But I'm more comfortably an introvert.

Gori: Yes. Of course.

Baker: But you can't just get things done that way.

Gori: Correct. And Moloney suffered by being openly an introvert. So, I think, Rauscher would have increased the effectiveness of the program. And Rauscher could have defended the program more forcefully than it was defended. Otherwise, I don't know that the program would have used more money. I think they had all the money they needed perhaps.

Baker: If that's true, your planning was insufficient then.

Gori: Our planning was always insufficient, because the money kept increasing every year.

Baker: Your planning should be ahead of your availability of money. Always have your planning ahead of the other.

Gori: We were asking for $100 million and they gave us $200 million.

Baker: So? That's all right. You should have had your

planning all ready to cover that.

Gori: Yes. We were not using--

Baker: Do you realize that Endicott and I both testified what we would do with a billion dollars a year way back in 1968 and 1969?

Gori: So, the question is, "How the political climate, public knowledge and opinion affects scientific progress and funding from 1950 to 1980 and today?"

Well, my appraisal is that the sentiment of the country has changed considerably from an attitude of anything is possible in 1950-1960, to a little bit more jaundiced outlook today, a bit more skeptical: that simply by throwing money at something you're not going to achieve whatever we want.

Today the War on Cancer legislation would not be possible. It was possible at that time because we still had this optimistic outlook and the money.

Baker: And yet the budget levels are way up.

Gori: Well, yes, they are, because traditionally they have increased and it's always difficult to go backwards. But I suspect that we are simply doing it today out of the so-called "compassionate" side of our political morals, as opposed to the entrepreneurial side of our

political morals of 20 years ago. It will keep the money there but not with the same kind of expectations and spirit that we had initially.

Today we are far less optimistic about our dreams. Let's put it this way. We are happy if we can keep the pace and do whatever we can today. At that time it was time for big dreams and for great expectations, the sky is the limit, and why not. Sometimes I look backwards with regrets that we still don't have that spirit because it looks to me that the political or, if you wish, the emotional climate of the country has aged too. It's not as young--the country--emotionally, politically, culturally, is not as young as it used to be. We are aging as a society and probably we need to have a new resurrection, so to speak, something that is difficult to say when and if it may be coming. It will come because history and culture are cyclical. We go from highs to lows. Now we are at nadir, let's say, and from there we have to go up sooner or later. My view is that we haven't reached the bottom yet, but I hope that before we say goodbye we may see the ascent toward a new zenith again.

That's about it, Carl.

Baker: Gio, I appreciate very much this--

Gori: I appreciate your coming here.

Baker: --interview. It's not only fun but important.

Gori: Important? I don't know.

Baker: I think history needs help.

Gori: It's some record. I simply want to thank you again for giving me years ago the opportunity of being part of this great story.

Baker: Yes. Well, we had a good time.

Gori: Yes, we did.

(Whereupon, the interview concluded.)