This is an interview of Dr. John Moloney, Head, Special Viruses Cancer Program of the National Cancer Institute (NCI)
February 21, 1995.
The interviewer is Dr. Carl G. Baker, former Director of the National Cancer Institute.

Moloney: Carl, I received your letter in which you have posed some 11 different questions to serve as a point of review and are really the backbone of this interview. You mentioned too that you wanted to go back in history, something well beyond the Virus Cancer Program?

Baker: Well, I think it would be interesting since you were at NCI for many years. When did you come to NCI?

Moloney: I can take you back to 1947.

Baker: Yes. Good.

Moloney: Really, it was July, 1947, when I joined Ray Bryan to work in the Cancer Institute. I actually was hired as a technician under Ray Bryan, and Ray Bryan served as my mentor over many, many years and, believe me, I have every respect in the world for him. But in 1947, viruses in cancer were not a whole here and now thing. I mean, it was hardly an accepted thing.

I'll never forget. In those days Ray was very busy working with the Rous virus, trying to quantitate responses of chickens and all that sort of thing. This is even before the days of tissue culture, rough tissue culture, so the only test system he had were chickens, New Hampshire Red Chickens. And in those days it was very difficult to get support. He hardly had support of the administration of NCI. He had just come up from Duke. He had worked with Joe Beard down at Duke, and he'd been there for a few years and now was working under--who in the heck then

was Director of NCI?

Baker: Dr. R.R. Spencer.

Moloney: It was a very small institute in those days. Everything was in Building 6 and, of course, that was the only place where any cancer research was ever done, I mean really good cancer research, only in Building 6. Well, at any rate, he had trouble getting support--money and all of that sort of thing--because, after all, it was generally believed that viruses really had nothing at all to do with cancer. And he had to fight that, and so did Joe Beard and so did Leon Dmochowski, and so did Ben Burmester and, to an extent, so did Vince Groupé, and these people became what we know today as the "founders" of viral oncology, the viral oncology that we know today. It was these men who created the field for us.

They persevered and Ray did get some support, even though at one time the Director of NCI--he developed Malloy Labs; no he developed Microbiological, what was his name--anyway he was there. He was a temporary Head of NCI, Director of NCI. He's the one that really suggested that Ray Bryan leave because he couldn't support him.

So, Ray Bryan was looking into a position at Tennessee, and I was all set to go down with him as his technician, and all that sort of thing. This was probably in '49 or '50, in those days.

Baker: This was before the paper was written with Shimkin where the good statistics on dose-response appeared?

Moloney: Oh, yes. Oh, many years before that. Ray was still struggling, and Ray used to always refer to a statement made by some of the individuals who created, who

brought together, a committee of the whole, so to speak, to develop NCI as it was created in the early '40s and so on, and in those days they said that infectious agents and all had nothing at all to do with cancer and they really shouldn't be supported. But regardless, Ray was there and he was working, and he was trying to quantitate chicken responses to the Rous virus, and I was going on working with him.

And finally then we did get some support, and Ray started pulling things together. In other words, he got away from the use of various phosphate buffers and got into the use of citrate which stabilized the agent, and, little by little, he came to the point where he could truly reproduce and quantitate--and reproduce quantitatively--responses to the Rous virus, which helped enormously. I mean, then he could do everything in terms of the effective temperature and all that sort of thing. It was one of the real first tumor viruses to be found. Of course, Joe Beard, in the meantime, was working on a myeloblastosis virus of chickens down at Duke, and Ben Burmester, because of pressure from the Department of Agriculture, was working on chicken lymphomatosis out in East Lansing. And Demi was a morphologist and good old Dmochowski, if you'd ask him for one slide, he'd create 10 or 12, just like that, all big glass ones in those days, you know, and when he'd go to meetings they'd break, he'd lose them, and all this sort of thing sort of thing. It was really hectic in those days.

But then Gross came along in 1951, and he was the first to describe a virus infection of leukemia in mammals, in C3H mice, newborn mice, which were much more responsive than other mice, and so he truly did describe the first virus

induction of leukemia in murine animals.

Then, following that, also there was a whole avalanche, really, of tumor viruses, mostly leukemia virus came down the pike, with Rauscher and Groupé and Friend, and even Upton came up with one that he got out of a mouse which was originally carrying a leukemia that had been induced by x-radiation, and so on. And so there was a whole string of these. And this carried us through then to the late '60s when, lo and behold, Jarrett, in Scotland, showed that feline leukemia could be induced by a feline leukemia virus and so on. And somewhat later then Thelen and Snider showed that feline sarcoma could be induced in cats.

Well, about this time, when we were getting all this information, there was a strong suggestion that bovine leukemia was a virus-induced disease. Several model systems were available to study the possible relationships of viruses in human leukemia. Was human leukemia a virus-induced disease and, if it was, what could be done to control it, what could be done to prevent it, what could be done to treat it, and what therapies could be developed?

And it was with this information that Ken Endicott went to the Congress--he thought that there was enough evidence now--and asked for a sum of money, and he did receive, what was it, $10 million dollars, at the time for the creation and support of what was in those days called the Special Virus Cancer Program.

Baker: It was first called the Special Virus Leukemia Program. It was changed to the Special Virus Cancer Program later.

Moloney: Yes, it was changed to the Special Virus Cancer Program, simply because we started working with cancers other than leukemia, like sarcomas and so on. And

then, a few years later, we called it the Virus Cancer Program, on the advice of R. Lee Clark, at M.D. Anderson, who was on the National Advisory Cancer Council and who said "Why are you calling it the 'Special' Virus Cancer Program?" "Well, we can call it the Virus Cancer Program. That sounds good enough." But the reason why it was called the "Special" Virus Leukemia Program was because it was initiated from the NCI Office of the Director.

Baker: It got special attention, certainly.

Moloney: Yes. And it was a specially devised and created program by Carl Baker, Lou Carrese, and Dick Rauscher. Rauscher supplied detailed virology scientific input Baker the broader aspects of cancer, and Carrese some managerial additions. The result was Special Virus Leukemia Program. And to me it was the best program, although initially I was absolutely opposed to program research in those days. I really was. I was hot for the laboratory. We had developed a leukemia virus and in 1962 we had come across a murine sarcoma virus and so on, and things were just moving ahead like lightning. And to stop, to give up a lot of that, and to get into more or less administration of program research--

Baker: I remember one time when you were still wondering about this approach we had a contract to produce sizeable quantities of one of the viruses and you were, I guess, serving as the Project Officer on this contract and you came in my office one day, eyes all aglow, a happy look on your face, and said, "Look, we checked this thing out. It's just as good as anything we've made and we've got buckets full of it."

Moloney: Yes. Well, you know, actually that's how I came across the sarcoma virus. We were having the Moloney leukemia virus produced in large quantities in mice up

at Charles Pfizer, up in Maywood, New Jersey, and we were having it produced according to our needs, wants, desires, and so on, and we would measure the concentration of it in terms of gram equivalents per milliliter. In other words in every milliliter of final material there was the equivalent to 1 gram of starting material if, indeed, it was a 1 gram equivalent concentrate.

So, anyway, I decided, gee, I'd like to go all out on this thing, so I had them produce a 100 gram equivalent concentrate of Moloney leukemia virus from the blood of mice. And they did. And they brought it down to Bethesda. And I inoculated it into my system--I had moved to the 3rd Floor of Building 6 in those days--and I inoculated into mice and a few days later, oh my God, they started developing lumps. And, of course, Sarah Stewart worked right down the hall from me at the time and she was very busy with her polyoma virus, and I was thinking, "Oh, God, I don't have leukemia; I have polyoma in the mice," and I was, "Gee, it's contaminated." Well, anyway we harvested the thing, carried it on through and as it turned out it was a sarcoma and we, of course, ran blood tests and all and it was negative for polyoma, thank goodness, and all of a sudden then we were in the sarcoma business.

I mean, we had received the virus, 100 gram equivalent, as we requested, from Pfizer, and it worked just beautifully. Of course it had been carried apparently in the Moloney strain of leukemia all along. And later, as the science developed, it turned out that Moloney leukemia virus was a helper virus to the murine sarcoma virus, which was always present but at a very low level in the leukemic extracts of Moloney.

And the same thing occurred then with Jennifer Harvey in London, who was working with Moloney virus in rats. She took it and inoculated it into animals and they came down with a multiplicity of tumors, mostly of the leukemia type, reticular cell sarcoma, the whole thing.

And then, a few years later, Kirsten, up in Chicago, developed, or came across, a virus which he called Kirsten sarcoma virus which, in turn, had been recovered from a leukemia virus concentrate that he had been working with.

But, at any rate, this was one of the nice things about having a contract. And, anyway, so we finally did-- We decided to get into the program and, of course, with you, Carl, and Lou Carrese, and we came up with an absolutely beautiful program designed to determine do viruses, or subviral information, or viral genetic information, cause human cancer, and not to get in there just studying mice, chickens and all that sort of thing for the sake of saving every chicken in the world, or saving every mouse in the world, but to develop a means for detecting, or determining, whether viruses caused human cancer and specifically, in those days, it was human leukemia, and then, if so, can we develop a suitable means for the control, prevention and treatment of such induced cancers and so on.

Anyway, the beautiful thing about the Virus Cancer Program in those days-- I suppose that you have outlined someplace how it was structured and all that sort of thing, the Virus Leukemia Program?

Baker: Oh, yes.

Moloney: In terms of the various segments of the Program including the SALES group (Special Animal Leukemia Ecology Segment)--I'll never forget one major meeting

we had in Building 31 with all the proposed segment chairmen, Lou Carrese, yourself, and Dick, and all that sort of thing, and going on, "And John, we want you to head up the 'SALES' Group." I figured, my God, I need a straw hat and all for this sort of thing. I'll never forget that. Of course, it was resolved that SALES would handle all the animal tumor virus systems, specifically the human and animal leukemia systems.

But then we had people like Tom Frei involved, who was heading up the human therapy portion, and Bob Manaker, who was into developmental research, that is the development of human tumor systems, human leukemia systems. We had Resources and Logistics, and Bob Stevenson was head of it in those days, which we really, really needed. I mean a beautiful Resources and Logistics System. Then that first special appropriation--I'll never forget that--it was $10 million dollars, and we found out about it in September of 1964. I'll never forget. We heard that it was going to be before the Congress for approval. And it went before Congress on a Friday, and I remember I was driving Tim O'Conner to Dick Rauscher's home for a poker game, and on the way we were listening to the news on the radio to find out whether we got the money or not. You know, $10 million dollars, my God that was equivalent to a billion dollars today. And just as they said yes, it was approved, but just as they were going to mention the amount, we went under an overpass and we missed it. We never did find out until hours later. Oh, my gosh. But we were all very happy. But then we had to commit it before the fiscal year ended. In other words, we would have lost it and it would have had to go back to the general Federal coffers. So, I remember we committed so many

dollars--I think it was $3 million--for the building of Building 41, a high-containment facility, and all this sort of thing. We thought we may not need it right now, but we certainly would in the future. And so we programmed it into Class 3 containment facilities, and the building was state-of-the-art in those days. It was just beautiful.

Baker : Endicott always said that you guys though thwarted his idea of making it much more flexible. When it was finished, the only flexible part was about a fifth of the building, when he planned the whole thing that way. But you guys wanted to have your offices with solid walls.

Moloney: Oh, yes. Yes, yes, that's right, instead of those partition things. Right. And we also developed a trailer--do you remember--a high-containment trailer that we moved around to various universities and all, for people who would isolate a virus and wanted to work on it under containment conditions and all that sort of thing. And we used that quite a few times.

I remember we sent it to Chicago for Fritz Deinhardt to use in the handling of the project that he had going in marmosets, and all that sort of thing. It was a DNA virus in marmosets. I forget just what it was. But anyway, that was one case where we did use the thing, and we needed it, the trailer. And we did develop all sorts of mechanisms for funding--the contract mechanism--and we did that apparently simply because it was much more rapid. I mean we could make awards much more easily.

Baker: In those days it was.

Moloney: Yes. You're right. Today, it's quite changed, I understand.

Baker: But also we used contracts because they allowed better integration of one part of the program to other parts. You had more control over the use of them than you do in grants, and it was necessary, as it was in the Chemotherapy Program, to integrate one part of the program to the other, and the use of contracts lends itself better to that.

Moloney: And also, of course, with contracts, it was the only mechanism we had available to work with the commercial types, Charles Pfizer, Bionetics, and those types of organizations. And additionally, I forget now, Carl, could the commercial types compete for contracts? I mean, when we advertised, could we award competitive contracts to commercial groups?

Baker: Yes. We did both.

Moloney: And to academia too? We could award competitive contracts to them?

Baker: Some. More often it was grants, of course, to the university people.

Moloney: No, no. I'm talking about in the Program now.

Baker: Yes. Sure.

Moloney: We had no grants at all.

Baker: Melnick had contracts for his monkey work at Baylor.

Moloney: Yes, but were they competitive?

Baker: Some were and some weren't.

Let me ask you one question before we get to the questions specifically. How about telling us a little more about Ray Bryan's personality and maybe Andervont too, as part of the old guard, and important figures at NCI?

Moloney: Well, they were just that. They were the originals at NCI. Ray Bryan had his

facility, his lab, down in the basement of Building 6. Andervont was on the First Floor, Building 6, and he was a Lab Chief of the Laboratory of Biology, in those days. Right next door to him was Walt Heston, who was the geneticist of NCI. Then upstairs was Wilton Earle, the leading tissue culturist of all the world. He was the one who was going to solve the cancer problem by growing tissues, tumor tissues, by the gram. I'll never forget that, growing by the gram, so that they'd be available for research, and that would be just wonderful.

Then we had Walt Schneider up there, and George Hogeboom--these are the biochemists and so on--of the Cancer Institute, and Jesse Greenstein, who was "the" biochemist of the National Cancer Institute, who believed that cancer was a chemically, or biochemically, induced lesion in man.

Ray Bryan struggled on. He had been doing, as I said, some work with Joe Beard down at Duke in the avian systems, and he came up to NCI, and additionally he was doing some work with Howard Andervont--Andy--on the First Floor. Andervont was working with the mammary tumor virus. So, Ray was working with him, again, trying to quantitate responses.

Of course, in those days, you know, the mammary tumor system in mice was considered nothing but a milk-factor induced tumor. It wasn't a virus. I mean they were absolutely afraid to--forbidden to--say it was a virus. At least it was not a very political thing to do or to say that.

Baker: I'm trying to get at, in this question, how their personalities influenced you. I'm sure they were inspirational to you. Can you elaborate a little bit on how they affected your pursuit of science, for example. In those days we didn't have all this

distracting talk about unethical science. Nobody would have thought of doing unethical things, I think, in those days.

Moloney: No. I came in, in July of 1947, as a technician. I was ready to lick the world though. My God, I had graduated from Tufts, and I was ready to just lick the world. And Ray Bryan was a very patient man. He had to be. I mean, I'll never forget his saying-- He would even train me in the use of the proper words or the right type of conversation in the unit. Because I had just come out of the service a couple of years before--you know, World War II, thank you--and anyway, I'd say, "You know, that gizmo," or this sort of thing. "What do you mean 'gizmo,' John? Now spell it out in so many words." And he would make me pronounce it correctly. You know, he really trained me beautifully. And he certainly trained me well and later I got my Master's under him and I got my Doctorate under Ray. And he was, again, very patient. He made available all the facilities and chickens and centrifuges and all that was necessary in order to work with the Rous virus. And he taught me how to do it quantitatively. Probits and all this sort of thing.

Baker: Quality control.

Moloney: To say the least. I mean his mind was a magnificent thing. He was really, truly, a brilliant man. Analytical? Good God, yes. He'd drive you crazy. You had to figure exactly what to say to him, you know, because otherwise he would respond in the wrong way. But he was a wonderful friend. He turned out to be a real friend. I'll never forget, after I got my degree finally--

And also Dick Rauscher, when Dick came down from Rutgers. Ray had done a lot of teaching up at Rutgers at the Microbiology Institute with Groupé, and Ray was

somewhat of a mentor of Dick's, but Groupé was more the mentor of Dick. But anyway, when Dick came down I joined Ray's group. We were in Atlantic City one time to attend the classic cancer society meeting--that of the American Association of Cancer Research--in Atlantic City, where in those days they were always held, every year, always in Atlantic City--no matter how miserable it became they were always held there until, what, 20 years ago maybe, or 15 years--well, anyway, we were sitting in the room there. "Now listen, you guys call me Ray or I will start calling you Dr. Moloney, and you Dr. Rauscher." And this is how we got on a first name basis.

I mean it was that-- Because, as a technician, and even all the way up until the point where I got my degree, he'd call me Moloney, just Moloney, and he'd call all his people by their last name. And this is the way I started calling my people by their last name. And a lot of them will say, "Why don't you call me Tom, Dick, or Harry," and all that, and no, I was trained this way and this is the way I would do it. Because, you know, you can always discipline a person that way. You know? I don't mean we beat them, but mentally discipline and teach them right and wrong that way, than if you're on a less formal basis than John-Boy, or all that. So, it worked out very well. But he's the one that truly interested me and, as I say, I did my Master's under Ray on the Rous system and later I did my Doctorate on the Rous system, under Ray, Of course, the degree was awarded from George Washington University. And we went on, and in the meantime I stumbled across the Moloney leukemia virus. I'd been working with the Sarcoma-37 in an attempt to measure the effects of lipids, and I was very interested in the effect of oxidized

lipids on tumor viruses. So I got a sample of S-37 from Morris Barrett, up on the First Floor. Do you remember him?

Baker: Oh, yes.

Moloney: He carried the Sarcoma-37 for a number of years. And I used that tumor. And I had read an article where Jack Dalton had done some work with Morris Barrett, and he described some inclusion bodies in the cells of the S-37 seen by electron microscopy. And I said, "Gee, that's interesting. Maybe that's a virus, or the effect of one?" You know? That's why I extracted it and inoculated it into two boxes of mice, newborn mice, because Ludwig Gross had inoculated newborns. And, of course, I had to keep them up on the shelf over the sink in the chicken room in a wooden box, two wooden boxes, two litters of mice up there. And I promptly forgot about them until one of the animal men came in whose name was Harvey Sims. Ray had just hired him. I later stole Harvey from Ray because he turned out to be such an excellent animal man. Anyway, he came in, "Dr. Moloney,"--yes, I guess it was Dr. Moloney, just Dr. Moloney--"these mice don't look really quite right." So I went in and I picked them up and palpated them, and I hardly knew how to handle a mouse in those days, but I palpated them and their nodes were swollen, and obviously also their spleens were enlarged. And, lo and behold, we were in the leukemia business from then on.

So, then we grew, and I developed the lab upstairs on the First Floor, right next to Andervont. And Andervont was, again, very kind, because basically, he really thought that viruses did cause cancer. I mean, it's not just a genetic lesion. I mean there are viruses, and he obviously had worked with the Bittner agent, the

mammary tumor virus. Anyway, he set it up so that I could use all these very strange mice that he was carrying for his genetic studies, and so I could inoculate them to determine whether the leukemia virus was effective in these very strange animals--and I mean he had some weird mice. For example, he had wild mice that he carried. I'll never forget that. My God, this must be back in 1960, something like that. It was probably 1959 or 1960, at least. At any rate, he had these wild mice, and he used to keep them in boxes, but in order to get them out of the box he would have to put the box into a big trash can, a garbage can, a big trash can, and open the lid, and these damned mice would hop out all over the place in the trash can. And the animal man, who was really good, because they seemingly had been with Andy for 100 years, you know, would sweep around the mice like they were stirring butter, you know, and gradually catch up on one and pull one up. And I'd inoculate it very quickly and put him into another garbage can. You had to be young in those days! It was good because it was an exciting time.

And Andervont was so supportive. You know? He was the Head of a Lab, and they backed me all the way down.

And I'll never forget too, even Heston, he was great. He was on the Editorial Board of the Journal of the National Cancer Institute (JNCI), and I remember writing my first paper, and I don't know whether it was the murine paper, a paper I had published on the Rous virus on purification when I was still directly with Ray, but anyway, I'll never forget, Heston edited the thing; he thought it was very good, very nice, and, of course, I was all up-tight, my first paper and all, but anyway, I'll never forget, one of his principal comments that's stuck with me even to this day

was the use of the word "uninoculated." "John, you can't use 'uninoculated.' That simply means that they were inoculated and then de-inoculated." So, to this day I would never use the word "uninoculated," rather than "non-inoculated." You know what I mean?

Baker: Yes.

Moloney: But you don't think of things like that, but he did, and he was a stickler for it. He was very good.

Baker: Well, those are fond memories we both have. Let me turn to the questions a little more specifically. We've covered indirectly the five or more most significant scientific results. I would put a question this way. It seems to me that what really broke open the field and made cancer virology respectable was the confirmation of Ludwig Gross' work. Nobody believed Ludwig Gross when he first published, and it took about two years --

Moloney: But the people in Viral Oncology did, the people not in other areas of cancer research.

Baker: I thought it took about two years before he was really confirmed, and then it really opened the whole field, it seems to me.

Moloney: Yes. Well, it was a question of whether he had leukemia and/or whether he had a virus to cause leukemia. But others came along, Charlotte Friend for example, in 1953 or 1954, but then she had this unusual lesion and Gross to this day will say that he's not at all convinced that she had leukemia, but she had this interesting disease in mice.

Baker: Erythroid.

Moloney: Erythropoiesis, extreme erythropoiesis, right, with huge, huge spleens in mice. But the beauty of her virus was it would go in adult animals and the latent period was extremely short, the time from inoculation until appearance of tumor was very short, and so it was a good system to use. Very rapid.

I'll never forget Thelma Dunn too, who did all the pathology for me, thank heavens, because she was a wonderful lady and extremely intelligent, well respected, and what a pathologist! And she would say, "She (Charlotte Friend) may have a virus--I'm not sure--but she sure as heck doesn't have a leukemia." And also the same way with Dick Rauscher. I mean, "He may have a virus, but --" Of course she was never hot for the viruses and cancer thing anyway. She believed in, obviously, leukemia in mice and all that. But anyway, Dick may have had a virus, but she was not at all convinced that it was a leukemia.

Baker: Were you involved in any way with Sarah Stewart's and Ludwig Gross's arguments about--

Moloney: Oh, well, it was a running thing, just like Ludwig never accepted the fact that Charlotte had a virus, or Rauscher had a virus, or Moloney had a virus. No way. I mean they were all Gross virus. I mean even today he thinks that they're Gross virus. And even when we came along with that sarcoma virus, "They're Gross viruses."

Baker: I didn't realize that.

Moloney: Oh, yes. I mean, as I told you, I'm getting some background information on retroviruses, the history of retroviruses now. I mean, he would sit back and he'd explain the sarcoma virus, and then he'd go on to say that his passage A material--

Passage A material is simply a rapid passage of the Gross virus in susceptible mice and, as you pass it rapidly, you can recover more and more potent virus. But anyway, he would do that, and then he would inoculate and, lo and behold, they'd come down with what he described as a lymphosarcoma, or something like that. So, indeed, the passage A does cause sarcoma. You know? Quite different, of course, from the MSV (Moloney Sarcoma Virus) types of Kirsten and Harvey and Moloney. But anyway, so they must be the Gross virus, and this is what he was thinking. Anyway, where the heck were we?

Baker: I think you've covered the main items here.

Moloney: Yes. There was a lot of stuff behind that.

Baker: I think we were all expecting to get more on the human side than we did.

Moloney: Yes. And, as you recall, we had that Human Task Force too created, the Human Virus Task Force, in 1963. I was just reading some stuff on that.

Baker: And, of course, we did developmental work to produce more monkeys because there weren't enough monkeys, and the ones we imported were usually full of disease.

Moloney: Just before the development of the program.

Baker: The enlargement of primate production made sense at the time. Unfortunately, we didn't get many useful results out of it with respect to human cancers.

Moloney: Right. Right. At Bionetics. And that's because, I mean, if we could get anything to go in primates, my gosh, we thought we had it in humans. There was expectation of results so that little discussion to the contrary was held at that time. We had some wonderful times with Pallota at Bionetics and Robotti. Do you

remember Robotti from Paris, who worked at the Collège de France?

Baker: Yes. We've had an interesting set of people we've worked with.

Let me turn to the next question because I'm interested in you as a scientist and how things looked from your perspective on the administrative side. So, we mentioned one key administrative or management decision, and that was the idea of asking for a special appropriation for the funds and--

Moloney: Right. Because we thought we had the background, the information, to justify it.

Baker: Shannon insisted on evidence for why we should go to the Congress before he let Endicott go and ask the Congress, so we had to put together information which Ray and Rauscher and I put together on about 10 or 15 points of why we thought this was justified. So, Shannon bought that, and then Endicott presented it to the Congress.

Moloney: He presented it to a committee, didn't he, a Congressional committee.

Baker: Yes. The Appropriations Committees.

Moloney: Yes. Who was heading it? I've been trying to think of his name.

Baker: Well, Fogarty was the Chairman in the House.

Moloney: Who?

Baker: Fogarty.

Moloney: Fogarty? Yes. Okay. He was a good man for NIH. Right.

Baker: Hill was chairman for the Senate Appropriations Committee. And in those days we didn't have to go to all these other committees like they do now.

Moloney: In the House and all. Yes. Anyway, that was the first major thing, because before those days, I mean there was essentially nothing for viral oncology. I mean viral

oncology was really in tough times. Certainly in the late '40s, early '50s, even with Gross and all, and Friend, and so on, I mean, the amount of money was really very small in those days. But, if you look at the budgets, and I have copies of these if you're interested, but only from 1963 or '64 on--are you interested in those figures?

Baker: I'll come back to that later. There was a lot of discussion on the decisions on how to structure all of this both by scientists and administrators, and you think it worked pretty well, I gather?

Moloney: Oh, yes. Obviously, you and Carrese wanted to get scientists who were active in the field to get in there to manage or to--

Baker: To guide.

Moloney: I know. --to guide programs all related to human cancer, human virus cancer studies and so on because you were in-house managers. And we, the Viral Oncology Group, which Ray Bryan had developed in those days, was probably "the" most respected in all the world, and there weren't that many in the world, mind you. There really weren't. California and Groupé at Rutgers and, so on, and a few others around the country--viral oncology groups--but Viral Oncology at NCI was "the" recognized and most respected group in all the country, known to the rest of the world, and so it was that these members that headed up the various programs, or various segments, within the Special Virus Leukemia Program with the idea, of course, that we would do some work in the lab while we administered some of this. This was in the very early days of the Virus Leukemia Program, 1964-1965.

We fast got out of the capability to do leading laboratory research and at the same time manage scientific programs well. We just didn't have time. And so we started working with, say, Assistant Section Heads, or Assistant Lab Chiefs, who handled the administrative portion of our own labs and so on. Well, we kept our foot in the door, so to speak, in the labs, but also, more particularly, we were concerned with the administration of programs.

Baker: Do you think that was bad, because, in fact, I was accused of taking Rauscher out of the laboratory and ruining him as a good lab man.

Moloney: Oh, no, I don't. I think we were probably all about ready for it by that time. The only thing is, you know, it was unfortunate that we could never go back. I mean, once you're out, you're out.

Toward the end there, 1978, which is toward my end anyway, and the thing was gone by 1978, the program had just dissolved, but anyway, about that time Manaker had quit. He was about ready to retire. He had finished, but the Program was gone. He was going back to the lab. He had been out since 1964--what's that 10-14 years--and Bob is no spring chicken either, but anyway, he went back and he no longer was interested in the lab. He couldn't keep up with the young guys because in those days--

Baker: By then molecular biology had changed the whole approach.

Moloney: Exactly, Carl. Exactly. And he just couldn't compete.

Baker: It was a different world.

Moloney: Yes. You don't even know the language. You could never go back.

Baker: I think we've covered the early part of the intramural staffing here. I want to turn

next to the committees. Who were some of the committee people who stand out in your memory as being helpful? For example, Chuck Evans chaired one of the committees--I don't remember the name of it--and I always found him very helpful. Melnick was another one who was very active.

Moloney: Yes. We had advisory committees. Let's see, the Program-- Let's put it this way. The Special Virus Leukemia and the Special Virus Cancer Programs maintained their general administrative structure throughout from its inception in '64 to its demise in '78. We had the in-house committee made up of senior scientists within the Viral Oncology Program of NCI. Okay? And we considered contract proposals which came in unsolicited and solicited, but 90 percent of the ones we handled were unsolicited. The solicited were mainly for commercial types, for the production of virus, and all that sort of thing. But we considered whether they were relative to Program objectives, and, if they were relative to obtaining the goals of the program, the Virus Cancer Program, would we give it a high priority and, if we would give it a high priority, fine.

"What would you do?" "Well, let's send it back down for scientific review." So we sent it down to the Segment Chairman for scientific review. This primary committee was made up of the Segment Chairmen, plus some additional Branch Chiefs within the Viral Oncology Program, the senior scientists. And then it would go back to the Segment Chairman, and he would get his so-called Work Group together, and the Work Group consisted of again about 90-95 percent out-house people. I love that term! I'm sorry. And they were scientists, and they were senior scientists, and they were certainly internationally recognized in their

field of competence--animal leukemia studies, human clinical research studies, and so on--so they would review a project that would be presented to the Work Group which met, maybe, 2-3 times a year, something like that, and it would be presented to these individuals and they would review it for science, scientific merit, and all this sort of thing.

And they would approve it, or disapprove it, and so on and, if they approved it would they categorize it, would they give a priority level to it. And then it finally came back for us for eventual final approval within the Virus Cancer Program itself, by the senior scientists.

For all intents and purposes really our contract proposals got a much more in-depth review than any grant proposal, when you think grant proposals came in and they were reviewed by a study section, period. Ours got a minimum of two and mostly three reviews for science, relevance to program, and all of this sort of thing. And so these were the committees that we worked with.

Then later in the program we had special committees, e.g., the Virus Cancer Program Advisory Committee. We had people like Frank Putnam and other outstanding scientists to advise us, and Charlotte Friend wa salso one of them. And what's his name, from Johns Hopkins, who got the Nobel Prize 5-10 years ago--

Baker: Daniel Nathans?

Moloney: I can't remember. But anyway, they would advise us for overall program, I mean our total overall approach to program. So, we had an awful lot of advice, an awful lot of paperwork, an awful lot of justification, and all of that sort of thing, to

justify our existence.

Baker: And yet later, I gather, the accusation was that we didn't have enough outside review and advice, the implication being that the review was not well done.

Moloney: Absolutely. Absolutely wrong. It was much better done. It was a high quality review, whether it be for relevance, priority and need for the Program objectives, or whether it be scientific review and all of this sort of thing. And, as I said, if in the judgement of the Segment Work Group the project under review is indeed relevant to the segment activities, then that activity is relevant to the total Virus Cancer Program activities. So, it was often difficult to manage these complex activities. And those who had research contracts with the Virus Cancer Program were very proud to have them. They were very proud to be associated with NCI. They were very proud to be associated with other members of the program and so on.

We used to hold meetings to update all of our researchers--our in-house people, our contract researchers in industry, universities, and institutes, etc.--in the program at Hershey, for example, an annual meeting, the so-called "Hershey Meetings," which became internationally known and recognized.

Baker: This was another function of the program, I think, that a lot of people didn't notice: the communication and transmittal aspect, because the program became a central focal point through which this information was collected and flowed and redistributed.

Moloney: That's right. I mean, from out-house coming into the house, and then presentations by our contractors and so on, for other contractors and for the

scientific community in general.

Baker: Wouldn't it be fair to say that you really didn't think of it as "in-house" and "out-house," as much as one effort?

Moloney: Yes. As a matter of fact, I don't like that choice of words either. But we did have excellent meetings and they were internationally recognized as probably some of the best viral oncology meetings in the world. Our participants, our contractors, and others were absolutely enthusiastic over those meetings.

Baker: From your perspective, you didn't have much information about political figures involved in this, other than you knew it was some committee of Congress that voted the money and listened to the presentations?

Moloney: That political? No. Not at that level. No. Not at the Congressional level.

Baker: There is another kind of political category, and that's scientists and clinicians and members of councils like Sidney Farber and Mary Lasker, who were very influential and were political figures in another sense.

Moloney: Of course I was aware of them. I wouldn't relate to them on a day-to-day basis. No. But I was very much aware of them. And people like Harold Amos and all of that sort of thing on the Board. And they were a big help to us too during our more trying times in 1977-1978, during the days of the demise of the Virus Cancer Program. I guess it was 1976.

Baker: So, you were talking about the demise probably resulting from--

Moloney: Oh yes. Benno Schmidt, under pressure from the university types and all that, because they wanted their money for grants--

Baker: This is an old battle, of course.

Moloney: Oh, through all of the years. I mean, "If you weren't giving so much money to contracts, we'd have more for grants." But that's not true. I mean, there were some programs, again, special programs, a lump of money set aside, to undertake studies which might lead to the control of a disorder, but if such a program is discontinued, the money does not go to grants.

Baker: Did you take a look at the relative amounts of funding of grants versus contracts during this time?

Moloney: Yes. And I have it here someplace and I can't find the darned thing.

Baker : What's your impression then?

Moloney: Well, of course, I mean, we had more money. But another thing which has annoyed me over the years too. We got this $10 million dollars originally in 1964 and at that time we had, in 1963, something like $4 million dollars in 1963, for all of Viral Oncology, in-house. I don't know whether that included grants or not. But these are the figures.

Baker: With that figure, it must have included the amounts for grants.

Moloney: Yes. Okay. So, the next year then it went up to $14 million in 1965. And then it grew and finally, by 1977, which was the last recorded year I have, it was $60 million.

Baker: But the grants went up at the same time.

Moloney: Much, much, much more, percentage-wise. The thing is that so many people thought that what with the National Cancer Program, the "War Against Cancer," and all that from 1972 on, that that really helped the Viral Oncology Program. It didn't. We did not progress beyond the limit. We progressed very slowly and

properly. We were at the level of approximately $60 million for several years. I have some figures here in case you're ever interested in it. And we never really did get any boost from the big National Cancer Program. I mean, we progressed in a normal way from $10 million dollars in '64 on through $60 million.

Now, that included in-house research too, support of in-house, our labs and all, in-house, as well as our contract program and resources.

Baker: Do you think, in addition to the academic philosophy here that grants, in the view of many academic people think is the only way to do it, that the shift to molecular biology made a distraction, or did the Program make its shift with molecular biology as well?

Moloney: No, no. We did make the shift. And I would say that we supported that major shift in research emphasis.

Baker: In other words, you didn't see that as a radical change, but rather a continuation and extension of ongoing work in progress before the signing of the new National Cancer Program Act.

Moloney: Yes. It was a normal happenstance. We supported the new effort it. Do you realize that we gave the Rauscher virus to Baltimore, which enabled him to come up with the polymerase? The Program supplied a lot of the virus to him. And as far as Temin, who worked with the Rous virus, is concerned I don't know really where he got the virus, but, of course, he'd been in that field for a long time.

At the Viral Oncology Meeting three years ago Carl, Baltimore mentioned in his presentation that he received the Rauscher virus from the Program through Bob Holdenreid. Remember? So there is no question. And we urged Tim O'Conner

to develop collaborative studies and all, because he was our real molecular virologist in those days; so, we started relating to the molecular types early on. And I think that we may not have been directly instrumental, but we certainly contributed to the finding and the discovery of polymerase and reverse transcriptase. Sol Spiegelman was also working vigorously in the area. You know? As a matter of fact he was--maybe I shouldn't say this--but he was a little bit green, you know, when Temin and Baltimore received the Nobel Award for it, because, Sol really had an excellent brain too, and he was working all around the area.

But indeed it was our people who then showed that all oncoviruses, all retroviruses, all RNA tumor viruses--which I prefer--have polymerase in them, and using this as a marker you could search then in a human tumor.

As a matter of fact, Sol Spiegelman came up with several human tumor tissue cultures, and others too, where they had a high concentration of polymerase and thought, "My gosh, there must be virus in there and all," but this happened several times, but they were never ever, even to this day, able to recover a whole virus particle from tissues in culture that had demonstrated the presence of RNA reverse transcriptase.

Baker: So you don't feel that the demise of the Program was due to a lack of shift by the scientific staff of the Program into molecular biology?

Moloney: Oh, no. I'll never forget one time when I was with Dick presenting before the National Cancer Advisory Board and we would justify our budgets and our programs, project what we saw in the future, what we had done, and all this sort

of thing, with Dick and Jim Peters, and they were saying, "John, what are you doing with all these polymerase studies?" You know, I had 3 or 4 contracts with the name "polymerase" in the title. Do you know what I mean? It was the way to go. It was "the" way to go, and thank goodness we did go that way, because it was just beautiful for us. I mean, we moved ahead so beautifully it was just great. And that was only one phase of the molecular work. And then later we started identifying specific genes associated with some of these viruses, the *mos* gene, for example, with the Moloney sarcoma virus, and all this sort of thing. And Bishop and Varmus, working with the Rous sarcoma virus, came up with the oncogene *src*. Oncogenes are associated with all these viruses. Emphasis and work on these helped us to understand more fully cancer induction.

As a matter of fact, investigators are using this type of approach, and this understanding and they have been able to come up with and map the progression of colon carcinoma from a simple adenoma, a benign lesion, all the way through to the highly malignant colon carcinoma, step-by-step showing that the *ras* oncogene is involved. *Ras* is the gene recovered from Kirsten sarcoma virus. A Harvey *ras* gene is recovered from the Harvey sarcoma virus. Step-by-step they have been able to show how these oncogenes are turned on in a cohort, or in collaboration with, still another effect or another virus to eventually develop colon carcinoma. It's beautiful. And I think we laid the groundwork. We contributed to that.

Baker: Tim was already into that before I left.

Moloney: Tim?

Baker: O'Conner.

Moloney: Oh, yes. We supported his lab. Tim started out with us--actually he was recruited by Ray (I guess it was Ray)--but he was started work by doing cesium chloride density gradient fractionations to make purer virus preparations. Do you remember that work?

Baker: Yes.

Moloney: That's what he started on, and he evolved into more molecular studies. Tim was trained as a chemist at Dupont, and he had a couple of patents with Dupont as a matter of fact.

Baker: Let me ask you this tricky question number 8. If you could have changed anything in the virus cancer field as it developed, what would you have liked to have changed and how?

Moloney: I think this: I would have added on some grant activities. We did do some CRAG work. Remember that assisted grant thing, CRAG, a developmental grant type of thing?

Baker: Oh, yes.

Moloney: It was around '76 or '77. We just contributed in a very minor way,in this case just a political thing to do. You know what I mean, that type of effort. But I was going to suggest--as a matter of fact I had it as part of my presentation to the National Cancer Advisory Board in 1976 or '77 when I gave my last presentation suggesting grant activities be part of the Program. Here I was trying to justify the existence of the Program. Do you remember? I used to justify the Program all the time before the Board. But this was the most crucial presentation. So I did what I

thought was required. We justified it for another year or so. But anyway, I was going to suggest that we get some grant authority. And I had a few pages on that suggesting that we get some grant authority in addition to the contract authority. I think we could have done it and I think we could have handled it. We would have needed other types of people and all of that sort of thing.

I also would have done this: I would have, as far as the actual Program and contracts were concerned, have gotten into antiviral compounds from a strictly scientific standpoint.

Baker: Well, we talked about that earlier, but there didn't seem to be even any leads.

Moloney: No. We couldn't justify it earlier.

Baker: Much less have an adequate scientific base.

Moloney: Right. And so we couldn't justify it. I agree with you, Carl. I remember. I know.

Baker: There was a little bit of screening but--

Moloney: But that, of course, would be the thinking of the infectious virus types to get into antivirals, to get into vaccines and all that sort of thing. But, it's too bad that we didn't. I mean, we could have laid the groundwork and we would have been that much further ahead in AIDS research. [Isn't that a good looking picture of you?

Baker: I was trying to figure out who that was.

Moloney: That's just because I had long hair. That was in Russia.

Baker: Well, I guess you do believe that the Virus Cancer Program laid significant foundations for molecular biology developments?

Moloney: Yes, and molecular biology contributed to us. It was a joint thing. I mean, it wasn't that we created molecular biology, per se, but we certainly did support a lot

of work in molecular virology and so on. Well, we laid the groundwork which permitted Gallo to come up with HTLV-I and II and so on, and Gallo gives the Virus Cancer Program credit for that. He came up with those viruses in 1980 and 1981, when he first reported them anyway. He'd been working on them.

We had a transfer of funds to Bob Gallo. We supported him. We transferred funds to DeVita, an in-house transfer of funds, in support of Gallo and so on, and it helped him develop that HTLV-I and II, and so on.

But the thinking, the method of research, the information, the infrastructure to permit such studies were all developed by the Virus Cancer Program.

Baker: Do you think there is sufficient appreciation of the value of the various resources that were provided by this program which now people can go and commercially order all sorts of things, but, in those days, we didn't have such resources available, and so a lot of the money in the program went into developmental research, I would call it, developing reagents, tissue cultures, antibodies, animals,etc.

Moloney: Everything. We started from a base of zero. I mean nothing. I mean, I came across a book in digging out all this debris you see in my den which originally--I really don't live like this, Carl.

Baker: Well, it doesn't look too bad. You ought to see my desk.

Moloney: But anyway, I came across an article that I used to keep when I was on the First Floor of Building 6. You remember my lab. I remember you used to come around and visit every now and then in Building 6, on the First Floor, right next to Andervont. Do you remember? Down at the end of the hall? I remember you.

Baker: Oh, yes. Unfortunately, I couldn't stay in Building 6 too long with my asthma.

Moloney: Yes. I remember that. Yes. You're right.

Baker: That's why I left the lab in the first place.

Moloney: Yes. Right. But you managed to come down. I mean, you made me feel like a "wheel." You were Head of Etiology then, weren't you? Or were you Scientific Director?

Baker: I was probably Associate Director for Program at that time.

Moloney: Yes. Anyway, that's where I used to ship out all the viruses. In those days it was Moloney leukemia virus. But before those days it was Rous sarcoma virus. And we used to count these little damned ampules because there was a lot of sweat, blood and tears in preparing what we sent out. And to have those things flung around, oh my God, it was really a struggle to do that. I mean, some of these guys, particularly abroad: "Would you send me a half a dozen half milliliter samples?" No way. That would be a year's work. Do you know what I mean? So, the development of resources and so on was just a boon to us and was a boon to science in general, everyone. I mean that would be no problem.

Even today I think Jack Gruber is doing a great job in making available-- And Jack Gruber, of course, got his start with the Virus Cancer Program.

Moloney: We were on the resources and logistics side.

Baker: Let's turn to question 10, and this may sound like a loaded question, but how do you think the political climate and public knowledge and opinion affects scientific progress and funding, including the Virus Program? Are they different now?

Moloney: I think they're much stronger. I think there is too much political effort, too much

political effect on science today. I really do. I'm a little bit sick. Obviously it's not working at all.

I mean, you talk about the influence of Congress, whether it be the House or the Senate, or what, trying to--as I used the term before--micromanage everything, even science, even down to the bench. I mean, "My wife is dying of breast cancer," or this and that and the other, and "Maybe we ought to drum up another couple of million dollars and give it to the Army," I mean, and this kind of thing, "so that they can conduct studies on breast cancer or, indeed, maybe they could check out something for the treatment of AIDS, for the control of AIDS, and all this sort of thing."

Give it to the Army! I mean, that was nothing but a boondoggle, and had been, and I understand now it's held up once again. They aren't going to go through with it. The Army has decided not to spend that because they don't have the wherewithal. But again, that's Congressional influence in an attempt to micromanage and I think it's wrong. I think it's 100 percent wrong. I mean, it is not their position to do this.

And the same thing with NASA. I mean, they have too much to say about the doings of NASA. NASA is being buffeted from pillar to post because of the unusual stress and pressure from the Congress and so on. Let them get on with the science.

As far as viruses and cancer go, the Congress has their hands in it because they don't understand it. They more or less understand AIDS because it, in itself, is a political disease. So they do have their hands into that as far as AIDS research

goes and development of control measures, even the distribution of condoms for safe sex and this sort of thing. To me it's wrong. There is no question that politics affects the goings and comings of science and certainly NIH, certainly every Federal institute.

You know, when you leave the Government and go out into a university, a young researcher will tend to lean toward an area of research where he feels rather comfortable that he will be supported, will get money and so on. And if, indeed, the support happens to be awarded to the university based on politics and politics alone, obviously he'll become politicized. And so it goes all the way down. And it's wrong. I think politics and science don't mix, period.

Baker: Well, I would put it that there are scientific matters that require scientific input to make the decisions that are being made on a political basis.

Moloney: Oh, absolutely.

Baker: But this is a complicated problem at the interface between politics and science, and, if you're going to have elected representatives, they should have some say-so.

So, my point is that the total amount to be spent, say, in cancer is a political problem primarily. But how, within that level, the funds should be allocated is primarily a scientific decision and should not be a political decision. So, where is that interface? It probably should be at the Surgeon General level. In earlier times the Surgeon General was concerned with health matters.

Moloney: But should the Congress, should the what-have-you, should the politician, make the decision as to how much should go to cancer research, or how much should go to infectious diseases, allergy and infectious diseases, or to heart, and all that sort

of thing?

Baker: I'm saying this interface is about at that level. How much we want to spend--

Moloney: Do you think that they can do a thing like that?

Baker: They have to do it all the time. For example, How much do you put for transportation?

Moloney: How much to cancer and how much to heart, and all that sort of thing?

Baker: Sure.

Moloney: No. I can see how much to go to NIH.

Baker: Well, where that interface is, is a very interesting question.

Moloney: That's my point.

Baker: What do you think about public opinion about science these days? Is that shifted from the past?

Moloney: No. I think the public are overwhelmed, certainly with viruses and cancer, which is my bailiwick. They just don't know which end is up. I think a lot of our cancerologists, or virologists, or virus cancerologists, or whatever you like, don't know what's going on in tumor virus research today simply because it's so molecular. I mean it's another world. It's in the area of genes. I mean, even immunology. You know, they're talking about molecular immunology now. I'm not talking about vaccines and all that neat stuff; we're talking about molecular immunology, the manipulation of genes, or genetic information, as expressed under the control of genes in immunology. I think that the public is very much aware and very much interested in major areas of cancer research, including AIDS. It's something they can understand get a hold on.

Baker: Do you think the support by the public for science is any different now than it was when we were active?

Moloney: What?

Baker: Do you think the public support is any different now than it was when we were at NCI?

Moloney: I don't know that it's different. I think they are more in awe. I don't know. That's a hard question to answer. Really, I don't know. In other words, do scientists and science have the "respect," is what you're saying, today?

Baker: That's another way to put it.

Moloney: Well, yes, because with respect comes money and support. I mean, if they respect you and they think that you and yours are doing a good job and will you help me in my way of life, will you extend my life and all that, will you make me better and all, then they will respect you. But if they don't understand what you're doing, and if they feel that you're so darned molecular you're at a level way, way--not below--above me and that I can see nothing that will benefit me for the next 20, or 40 or 50 years, I mean, I can't see why I should support you now.

Baker: Let me go back to the understanding on the part of the public. Have academic communities done a good job of educating their graduates about science?

Moloney: No, I don't think so.

Baker: Why not?

Moloney: Most of the education today in the sciences is in the clinical area, and it's not even in science transfer from the lab to the clinic and all that sort of thing. And for a while there they had the two degree business, the Ph.D./M.D. degree, so that the

two could meet supposedly. I don't know if they ever met, but supposedly they did. I think more emphasis today is in actual practice of science, whether it be in the clinic--

Baker: But I wasn't restricting this to biomedicine.

Moloney: --but certainly not in the lab. Huh?

Baker: I'm wasn't restricting this to biomedicine; I'm thinking of science in general. Let me throw this out for your comment. It seems to me that Science Departments in the better universities are doing a good job in educating and training those who are going into science research-wise, but they've done a terrible job in trying to educate people who are not going into science, and then they wonder why the public isn't more understanding.

Moloney: Yes. Well, of course, that's been the name of the game for all these many years. I mean, really.

Baker: But, should it be?

Moloney: In other words, because a person in science, but not going into science, would be going down the M.D. route and all, rather than the Ph.D. route.

Baker: Now again, I'm not just talking about biomedicine.

Moloney: I know. But it's hard for me to get beyond it. In other words, what you're saying is like a space scientist, or something like that? Is that what you're saying?

Baker: Or physics, or chemistry.

Moloney: Well, you know, to get back to biology, cancer research and all that sort of thing, did you read that article recently that M.Ds., the clinician, is having a tough time competing for grants and so on from NIH, the clinical types are, as opposed to the

more laboratory types, the bench scientists and so on?

Baker: No. I didn't see that.

Moloney: Apparently they are. They feel that they're not even very well trained in the writing of grant applications, much less the material that they put into a grant application. So they're talking about now creating special study sections for the review of such applications. I'm talking about research in the clinic, as opposed to research in the lab, or at the bench and so on. So, certainly they feel that they're not educated at the level where they should be, as opposed to those who are in science.

You know, when you talk about educating in science, whether it's biology, or physics, or so on, I mean, you're working under a mentor in any university atmosphere, so you learn from day one how to write a grant application.

Baker: As a graduate student you should be getting that instruction.

Moloney: Well, they should. And they should be contributing too to the application in one way or another, and so on. But that's not so at all with an M.D. or a clinical type, whether it's an M.D., or a Ph.D., or a psychologist, or surgeon, or whatever. They just are not trained to orient their thinking toward that research.

I don't know. Is there a real value of having M.Ds. in research, in biomedical research, as opposed to having Ph.Ds. in biomedical research?

Baker: I like the Shannon idea of some people getting both degrees, where you can integrate them in the clinical and life sciences.

Moloney: I think that was beautiful and I think that's what Cy Perry was trying to do there for a while; that he headed up this Office of Technology Transfer, do you

remember, between the labs and--

Baker: That's a different problem but--

Moloney: No, but it's essentially the same thing, so you could integrate the effort between the two. If you could have some understanding of what it takes to do research in the clinic, and what it takes to do research in the lab.

Baker: Apparently, the main problem of the double degree is the length of time it takes to get it accomplished, and so the number of applicants fell off.

Moloney: Do they still award it?

Baker: I think they have a small program on it still.

Moloney: At some schools? Why does it take so much more time? I mean, what is-- Like, I mean, do you get the Ph.D. first, or second, or how is it done?

Baker: They do it either way in this program that Shannon set up. I don't know. I, of course, got the M.D. first and then I took biochemistry at Berkeley.

Moloney: I always thought of you as a biochemist anyway.

Baker: Well, that's why I came to the NCI. Yes, I've got some basic research papers in my early life, and I wouldn't have left the lab that soon, I'm sure, if I hadn't gotten asthma. All those mice you had.

Moloney: Chickens.

Baker: Well, I'm sensitive to chicken feathers as well.

Moloney: Everybody is. God.

Baker: And I thought I was going to be all right in the Clinical Center when it opened, but in the wings the dander floated up and down and around the pipes from one floor to the next.

Moloney: Exactly. Yes.

Baker: Well, do you have any additional comments that you'd like to make on this whole business?

Moloney: No. But I think the Virus Cancer Program is a great loss. I really do. And I think they made tremendous strides. And I think it is being appreciated even today. I mean, what you ought to do is talk to a guy like Gallo, just to get a statement or two from him what he thinks. Now, he's a participant and he was part of it. He wasn't part of the creation, but he worked with the Virus Cancer people in the Program. He attended the Hershey meetings and everything else.

Baker: Yes. I know. In fact, I have him on my list to possibly interview. Oh, which reminds me, Huebner and Todaro and their oncogene theory, do you want to comment on that?

Moloney: Yes. It's a bust.

Baker: It's what?

Moloney: It's a bust. It didn't work. It was a good idea in the beginning, in other words, that the oncogenes are there--not oncogenes, but protooncogenes--but it didn't work. I read something about that recently. All I know is that it didn't hold up under fire because they came along and they started describing positive viral genes, viral oncogenes and all that sort of thing, protooncogenes, I mean the real McCoy. Theirs was strictly a theory that maybe this does exist.

How is he? Have you ever heard anything about him?

Baker: Yes. He's sick with Alzheimer's, so I haven't heard anything specifically lately.

Moloney: Oh, I know that. But is he still alive?

Baker: Yes.

Moloney: He's been disabled for some time.

Baker: Yes.

Moloney: But no, that didn't pan out. It was something of a bust. But that was reviewed in several papers and manuscripts and all. If you're really interested, I can dig it out for you sometime.

Baker: Well, I may get back to you on some of that.

Moloney: Well, send me a list of things that you would like that I could dig out for you.

Baker: Well, I don't want to put you to work unless I really need it. I've tried to paint some of this picture, but yours will be much better on the science side, of course. I'm not going into the science very deeply in the writing of my memoirs. But in the thing that Stevenson and I are doing, we will go into it there.

But another thing we're trying to do on this effort is to get a better appreciation of the resources and the contribution of the resources, because I don't think that's very well appreciated.

Moloney: Well, a question. I mean, the Resource Program was actually part of the infrastructure of the entire Virus Cancer Program, Specialized Leukemia and so on, because without it, it would never have moved. I think Stevenson showed a foresight in the development of that, the expansion of it, and so on. He really did. And it was carried on then by Jack Gruber and all, but not just the production of things, but of quality resources. I mean, not just that viruses are contaminated with PPLO, but quality viruses, virus preparations, were madeavailable and were something you could rely on. And also animal systems, animal test systems,

supporting the development of unusual strains of mice and all that were unusually susceptible. I don't know how much input we had into the development of nude mice, for example, but I wouldn't be surprised if we had some input into the development of nude mice that would accept human transplants and all that sort of thing.

Obviously, the primate systems, you know, was something that we really, really needed. It's too bad Art Pallota flunked out. He was good.

Baker: Well, Dr. Moloney, I thank you very much for your time and your comments and I wish you well on your present review article and I look forward to seeing that. Thank you for offering to provide some information you might have if I need it later.

Moloney: It's good seeing you again and, seriously, if you need some help, or if you need any copies, or anything, get in touch with me and I'll see what I can dig up on it.

Baker: Very good. Thanks very much.