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A CONVERSATION WITH HENRY S. KAPLAN

Interviews conducted December 7, 9 and 14, 1983 (Spyros Andreopoulos)

Q: Let's begin with the history and see where it takes us. Then I'll pose some specific questions.

A: I can't recall anymore all the dates with precision, except that I arrived at Stanford on September 1, 1948. I have been here a little over 35 years. Wally Sterling was appointed President of Stanford the following year. By 1952, if my memory is correct, Sterling had heard arguments on both sides of the issue of moving the medical school from San Francisco to the main campus. He decided to form a committee, but instead of picking its membership from among the old guard--which at the time included people like Arthur Bloomfield in medicine, Harold Faber in pediatrics and Emile Holman in surgery--he reached out for some of the younger faculty of whom I was one. Indeed, I was the youngest, because when I accepted the job at Stanford I wasn't quite 30 years old. Incidentally, Bloomfield, Faber and Holman had served on a committee which a year earlier had rejected the idea of moving the school.

We (the Sterling-appointed committee) met many times in the evening, I remember, at very attractive places like the Bohemian Club--places I had no knowledge of, and was quite

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overwrought especially when I looked at the prices on the menu. Lloyd Dinkelspiel who was then chairman of the board of trustees was a committee member.

We reviewed demographic charts with projections for Santa Clara and San Mateo Counties and it was perfectly clear that although we might initially have some paucity of patients ultimately we would have more patients than we could ever have in the city. That was, incidentally, the basis upon which the Faber committee had rejected the idea of the move--that there weren't going to be enough patients. But it was a very static view of things. The Faber committee didn't project forward into the next decade or two. In addition, the old guard in the medical school really were a bunch of pure clinicians, all very good in their fields, but with essentially zero track record in research or anything that could be remotely considered research. To them, therefore, it meant nothing that the medical school would be located on the main campus working side by side with chemistry, biology, physiology, physics etc. Moreover, the medical school was already a divided school; all the clinical departments were at the hospital in the city, but there was a small unit on campus that included anatomy, physiology, pharmacology, and a so-called division of biochemistry in the department of chemistry which was manned by two people, Hubert Loring and Murray Luck. Their general attitude was that they

condescended to teach medical students--it was a painful duty for them. This was not a very satisfactory arrangement. Our committee (which Sterling really ran, because he knew where he was going) built a very strong case for the move and one of my assignments was to build the general argument about the increasing interdependence of the sciences and the increasing scientific basis of medicine, and the fact the medical school could not flourish unless it was located in proximity to the scientific departments of the main university.

I might say parenthetically that all of this happened in a way that was most painful for me, because we had a stunning house high up on a hill in Sausalito, about a thousand feet above the water, designed by Mario Corbett. It was like a piece of sculpture. And I had labored long to develop the landscape and so on. And I absolutely loved the place. But I saw no alternative to the answer that the school must move down if it were to try to become anything but a third rate clinical medical school. It was sort of second rate clinically then and it would have slipped backward in time.

So that is one set of memories and that decision was firmly taken, and of course almost immediately there were howls of outrage from people like Holman, Bloomfield and Faber. To a degree they were all gentlemen and their howls were rather restrained, but they were forceful. They kept after Wally, through the alumni, saying this was the worst decision

imaginable, it would mean the death of the medical school, and so on. And I'd say Wally deserves a medal for the courage that he showed in sticking with that decision.

Q: Was there any indication at the time that the proposed move of the medical school might not go through?

A: There were circumstances that made the move happen, which were ironic and ludicrous. For example, Yank Chandler, who was a fine dean, could not live with this decision. So he resigned. I thought this was all right. It's an honorable way to behave. His successor, Windsor Cutting, a professor of pharmacology, was chosen. That was a very bad choice, however. Cutting wanted to build mediocrity. He wanted to build in the absence of argument and debate. Those things were anathema to him. In 1954 I went off on sabbatical and meanwhile I heard that he had concluded this crazy deal with the Veterans Administration to construct a hospital near the campus. I also heard they were going to woo the Palo Alto City Council that planned to expand the Palo Alto Hospital (Hoover Pavillion). Somebody had gotten the brilliant idea of building two hospitals belonging to two different owners under one roof! And this was Windsor, the great compromiser. And I think you know the trouble that decision has given us and continues to do so

to this present day.

When I heard about it I was in Bethesda, Maryland. That made me so upset that I sat down and wrote Windsor a long letter explaining what I thought was wrong about the two hospitals under one roof, and also about that separate deal with the Veterans Administration Hospital five miles away on campus. My tone must have been very rough. As you know I am not noted for being a diplomat. And in time I got back a letter from Windsor at the National Cancer Institute where I was doing my sabbatical, which in essence said, "Perhaps it's not possible for you to be happy at Stanford and you should think about going somewhere else." I was hurt by that but then I considered the source and I decided that this man was so stupid he couldn't understand my arguments.

There was an intervening period, which seemed to last forever, during which the medical school project was going nowhere. While Cutting was down on the campus wheeling and dealing, committing the faculty in ways that would pose great difficulty, I got a call from Wally Sterling asking if he could see me privately. Much to my amazement he asked me what I thought of Cutting as dean. I said he was an embarrassment and a catastrophe. And Wally said it had taken him much too long to come around to the same point of view. But he now felt that way and he was going to ask Windsor Cutting to step down. He asked

who did I think among the existing faculty would make a good dean. I paused briefly and I said I thought it would be very tough on him, because he didn't make decisions easily, but in terms of having integrity and the respect of the faculty and some dynamism, Robert Alway would be my choice. So Alway was appointed acting dean initially for six months, and then Wally decided he liked the way he worked with him, and made Alway permanent dean.

That was in 1957. Meanwhile, I had served as chairman of the committee to find an architect. I had visited Eero Saarinen in his offices in Bloomfield Hills and the Owens-Skidmore-Merrill architectural firm and been taken to lunch by an executive vice president and I knew immediately that if we had signed up with Owens-Skidmore-Merrill, a huge firm, there'd be some nameless, faceless person in the back room doing our work while we would be dined and wined by the brass out in front. That was not what we wanted.

We also interviewed local architects, one of whom tried to use political influence to get the job. It was a difficult business. In the end we had several groups of architects to choose from. There were local architects who had built buildings of distinction but had never done a hospital or a medical school, there were the hospital architects as such, and we ruled them out because we knew their imaginations were very

limited. They worked from old formulas. That left us with a group we called the great solo architects and that included Saarinen, Edward Stone, and a man in Boston whose name I seem to have obliterated but who was in that category.

I was sent on a trip to interview these three gentlemen and also to see the Owens-Skidmore-Merrill people, and I started with Bloomfield Hills where Saarinen lived. He was a wonderful man, very kindly, soft-spoken, and he took me on a personally escorted tour of the new General Motors technical center which at that time was considered one of the wonders of the architectural world, and yet had been designed essentially by one man's firm--not a big outfit. I was deeply impressed with Saarinen. When we got back to his office, he said, "I'm very interested in your project. I have never designed a hospital. There are of course guidebooks and brochures from the Public Health Service--every architect has access to those. It would be a great challenge to design a truly beautiful hospital and medical school. But there is a problem."

The problem, Saarinen said, was that his firm was busy designing a complete campus for some small university, renovating the harbor of New Orleans and he went on describing one project after another. The consequence of this is that he could not leave his office. "If you decide that you want me to work with you," he said, "then delegations of your faculty

would have to fly to Bloomfield Hills every two or three weeks to review progress and help refine our thinking."

My heart sank on hearing this news. I knew his proposal would be quite unacceptable to the faculty and it wouldn't work. But I must tell you, Eero Saarinen was a fantastically impressive figure. Unfortunately, he died of a heart attack sometime thereafter. After this I went on to see the architect in Boston. I wasn't impressed with him and that's maybe why his name doesn't register with me anymore. In successive days I flew to New York and visited Owens-Skidmore-Merrill, and the latter was exactly what I had expected. Some smooth-talking vice president immediately was available to take me to lunch and tell me all about their vast design, engineering resources and so on. But it was really not what we wanted because we would have gotten a carbon copy in slightly different size of some of the other hospitals they had done and they had done plenty.

Next I went to see Edward Stone. When I arrived the Chancellor of Vanderbilt University was there, finishing up a visit. Stone had been doing some new large hall for Vanderbilt and I looked at the design and it was just beautiful. When he left, Stone and I sat down. I told him about the scope of our project. I asked him whether he had ever done a hospital. He had done two, one in Lima, Peru--a huge obstetrical hospital which is still there and looks quite good-- and the University

of Arkansas Hospital and Medical School which, he said, we were welcome to visit. He had developed a very novel arrangement of beds in relation to nursing units, which worked out quite well and lent itself to a beautiful looking building. While we talked, I told him we were interested in building about a 400-bed hospital, a complete set of clinics, classrooms and research laboratories for students and faculty. He said, "If I were offered this job, it would be the biggest single design job that I have ever undertaken in my life. I will put my New York office on a skeleton force and I will move my family out with me and I will guarantee to stay in the Palo Alto area for no less than 18 months until the project is well off the ground. After that I will make arrangements for continuing supervision by one of my staff."

The difference was so night and day that when I returned I reported all this to the committee and they voted unanimously to select Edward Stone. Then Wally and the trustees said the Palo Alto City Council members needed convincing and a junket was arranged to Arkansas. I went along and saw the building. We were all most impressed and it was clear the councilmen and the university were ready to give the job to Ed Stone. It was at this point that he approached me and said there was something he had to tell me. He said he was a member of Alcoholics Anonymous, but that he had not had a drink in

several years. He said he didn't think he'd fall off the wagon on this job, but he would not accept the offer unless Wally Sterling knew this and was willing to make the offer in spite of it. Here was a man, staring at more than \$22 million worth of buildings and willing to lay it all on the line because of this question, which I thought showed great courage and integrity.

Q: What difficulties did Stone face in this project? I saw some blueprints that looked different from what we now have. Who changed his plans?

A: I'm glad you asked that. It brings up a very important and absolutely true episode. Stone's original design for the medical center included a vertical, high-rise hospital, intersecting with a clinic building of four stories, and then the medical school at the other end. It would have taken a totally different shape, saved miles of walking, and most of all it would have cut down about 10 percent in personnel costs. He took the plan to the board of trustees at the time David Packard was head of the board. And so help me God, Stone was voted down on the grounds that no building on campus could be taller than the Hoover Tower. That was it.

We went to the Stones' house that evening for dinner and he was in despair. He had fought hard with the board on that issue and he had lost.

He said, "Henry, you watch and see. In the course of time there will be thousands of people who will say this goddamned thing is so spread out, and I have to walk my leg off in it, and everyone of them will blame Ed Stone and not the board of trustees." It is also not widely known that when the hospital building committee wanted to save costs by deleting the beautiful hanging gardens, Ed Stone shamed them out of it by offering to pay for them out of his own pocket.

I'd like to have that put straight.

There's another item I must mention related to that particular era. One problem, as I mentioned, was that the Stanford trustees in their wisdom--and of course they always do things in their wisdom--and the Palo Alto city fathers had decided to build a hospital, owned by two different owners, under one roof. This led to a curious kind of struggle. It was clear that if there was to be a Palo Alto wing, there would be a Palo Alto medical staff and a Stanford medical staff. That was no problem, except when you got to Pathology and Radiology, two services seldom duplicated in hospitals. There was an agreement to unify Pathology because the doctor who ran the pathology labs for the community was then retiring and it was simple to ease him out and build a single, unified department.

But in the case of Radiology there was Howard Jones and Robert Brown as one group. They were already the established diagnostic radiology people for the Palo Alto crowd, and there were other demands beginning to surface from the Palo Alto Medical Clinic radiologists for access to machines and space and time. I was called to a hearing of the Palo Alto City Council with a couple of the university trustees of whom Packard was one.

The council asked me, "Does it make sense to have two departments of Diagnostic Radiology?" Apparently, nobody cared about radiotherapy and nuclear medicine because they lost money, at least in the way they were being practiced in Palo Alto at that time. So the only issue that was a matter of principle--money being a matter of principle--was diagnostic radiology.

They asked me, "Do you believe that there should be two departments under one roof in this hospital or do you feel they should be amalgamated into a larger, stronger department?"

I knew what they were fishing for. They wanted me to be the fall guy who would vote to throw Howard Jones and Robert Brown into the street. Then I would have inherited 30 years of hatred from the Palo Alto staff. And I said just as much.

"First," I said, "I don't believe in two departments of diagnostic radiology under one roof in one hospital. But this

isn't one hospital. This is two hospitals owned by two owners. If you can put up with anything as silly as that, then you ought to be able to put up with something as silly as two departments of diagnostic radiology."

I knew some university people were very unhappy with me for saying this, but certainly the Palo Alto medical staff were relieved although none of them came up and thanked me. I might say even to the present day, not a single one of them has thanked me.

Things became more complicated four weeks later. I got a phone call from Windsor Cutting who was still a dean, so the date of this episode must have been early 1957. He said David Packard wanted to come up to San Francisco and have dinner with the two of us. I accepted, went along to dinner and argued the same thing all over again and I thought I had made my point convincingly and clearly and the whole matter was resolved. We parted company very amicably. Then about two days later I get a call from Cutting saying, "Henry, it's been decided to unify diagnostic radiology into one department after all."

I said that was interesting and asked who decided that. "Did you decide that?"

Cutting replied the matter had been decided at the trustee level and specifically by David Packard.

I said, "Would you mind giving me his phone number, I'd like to call him up." I was in the middle of a busy clinic full

of patients when this happened.

He said, "That's not necessary because David Packard is right here in my office."

Minutes later I marched into Cutting's office, still in my white coat, and said to Packard, "Hello, David, I understand you decided to unify the department of radiology after all."

And he started out to explain and to apologize, and I cut him off saying that he didn't have to explain all that to me. I said you are the president of the board of trustees of Stanford University. It's entirely within your power to make that decision." And I added, "But there are other decisions that need making." I then took off my white coat, handed it to him and said, "For instance, there are a lot of patients down in the radiology clinic in need of care. And you've just become the chairman of the department of radiology by default. So you better get on down there and start taking care of them."

Packard was dumbstruck. He said "What is all this?"

I said, "It's very simple. In any of these situations there can be only one president of the board of trustees, there can be only one chairman of radiology. Who is it, you or me?"

He took a long look at me, I am sure he wanted to kill me, but he handed me my white coat and said, "You are the chairman and we'll do it your way."

PART II

Q: It has been my impression since I came to Stanford twenty years ago that the medical center is operating under very restrictive financial conditions. The arguments being made against building the hospital modernization program today, for example, are the same made against constructing the Stone building in the fifties. Do you share that impression?

A: I don't know if this is well-known fact. But the trustees of Stanford University had originally budgeted the total sum of \$6 million for the entire medical center project--a project that included a library, a medical school building, a hospital and clinics. (The figure excluded the budget committed by the city of Palo Alto for its hospital wing). The budget incidentally was supplied to them by Windsor Cutting and they accepted it! The final figure, of course, turned out to be \$22 million and that was accomplished by leaving out one of the important buildings for the clinical sciences which was eventually constructed four years later.

It might be of interest to point out that in contrast to these early years and based on my recent contacts, our trustees today are very much more on the ball. In those days there was a lot of nepotism, family connections and other

factors that got you on the board. So there were some people who could be tycoons of industry, but who did not have the brains to check a figure of \$6 million to build such a large enterprise. Moreover, it's clear they would not have approved a bigger budget. In a sense, therefore, this medical center and school were built by mistake or by a ludicrous misunderstanding of anticipated costs. This is an intriguing historical fact.

Q: During your years as chairman you built a department of radiology noted internationally. What was the secret behind this development?

A: When I became department chairman in 1948 there was not a single department of radiology in the world in which there was any scientific research. The only thing being called research for diagnostic radiologists was to sit on their butts in front of a viewing box and look at films and perhaps collect one or two cases of some rare malformation. That was the only notion of research.

I felt it was very important for therapeutic radiology to have an experimental as well as a clinical research base. I insisted on laboratory space from the start. We were housed in a little corner of the old Lucy Stern Building in San Francisco. We had very little space and at one time I made a desperate attempt to persuade Dean Yank Chandler to give me

additional space. I pointed out that in the small space we had there were 28,000 mice and three technicians and two physicians, which added up to 28,005 breathing, living organisms in a space of about 700 square feet. I made it clear that this was not feasible. He laughed so hard that he gave me the space I had asked for.

In those days I had a mouse colony. I had inherited it when I was at Yale from Lionel Strong. He was an anatomist and geneticist who spent his entire time in his mouse laboratory. He had hundreds of thousands of pure-bred mice that he used for various kinds of research. When I was recruited I had asked if I could have a couple of shelves where I could keep my mice. When I arrived at New Haven I was given a budget of \$300 a year to take care of my animals, which meant taking care of them personally. I cleaned the cages, replacing the dirty with clean sawdust, and put in fresh food and water. This mouse colony was the original C-57 black strain developed by a colleague of Strong's named Clarence C. Little, the founder of the Jackson Bal Harbor Laboratories. These mice turned out to be extraordinarily useful in our research and we have used them ever since.

I had taken this mouse colony to Bethesda during the year I had spent there. And then, after I had joined Stanford, they arrived one night at the San Francisco airport from

Bethesda, and my assistant Mary Brown and I were there at three o'clock in the morning to receive them and see that they got put to bed.

In a sense, I had a laboratory from the start. I felt intuitively that it was no good having a department chairman make big speeches about how important research is and himself do no research whatever. I felt on the contrary that in radiology which was then so backward with respect to research, the only way it would work was for me to see just as many films, just as many patients as anybody else, and to be chairman on top of that and do research. That would shame my faculty into feeling that they too could and should do research. Obviously, not everyone I selected turned out to be a red-hot research man. But in the course of time I weeded out most of the ones that were not productive in both areas--the laboratory and the bedside.

Simultaneously, I took steps designed to advance our field in a broader sense. In those days, Russell Morgan at Johns Hopkins was an active researcher in the field, although his specific work was in imaging and electronics. We got together and invited my former chief at Yale, Hugh Wilson, and the radiology departments from the University of Chicago, University of Rochester and University of Michigan. Our six departments founded the Association of University Radiologists. We met once a year by rotating the sessions at different

institutions for two or three days. Papers presented at these meetings had to be strictly on research and little by little that organization grew to more than 100 departments and several hundred radiologists. Russell Morgan was the first president since he was senior to me, and I was the second.

If I had to focus on some central theme in my endeavors, you might say that I created the idea of doing laboratory research in an academic department of radiology, and I insisted on doing it myself, partly because I was interested and because it closed off an escape hatch for other faculty members who could have found excuses for not doing research.

Q: What were the conditions in which you worked in those early years?

A: They were godawful. Our physical plant was indescribably bad. There had been a famous engineer at General Electric named Snook. He had designed X-ray machines in the 1920s, and Stanford had bought some of them. On the day I arrived I observed that his machines had an extraordinary feature--bare wires, carrying up to 200,000 volts, dangling above the patient. We had three non-shock proof machines for radiography and one other for fluoroscopy. I insisted that money be provided to change all that, and the dean was as good

as his word and it was changed. But it took several months to do it, and I had more nightmares than in all the rest of my life put together, and every single one of those nightmares, no matter what form they took, always ended in a brilliant blue-white flash! Until we moved these machines out of the department I just couldn't rest.

In those days Henry Jones, now professor of radiology in our department, and I were the entire faculty. He was loyal enough to be willing to stick around on an instructor's salary and work like a dog on Saturdays and Sundays to help get the job done. And I did the same. I was finally able to recruit somebody a year later and begin the process of bringing more people in and weeding out those that lacked research orientation.

Meanwhile, by sheer luck, among three young residents that I had inherited when I arrived was a young man named Herbert Abrams. The other two residents were good, hard-working but not outstanding. But Herb was clearly special. Moreover, I had the feeling that radiotherapy was such a shambles, a disgrace physically, that I had to do something about it. So when Herb finished his residency, I appointed him to an assistant professorship and gave him more responsibility over the diagnostic division which up to that time I ran also. The only specialty we didn't have was nuclear medicine. This was

later created by Robert Newell, my predecessor who had resigned the radiology chairmanship in order to do this. He was a very sweet man who never interfered in what I was doing.

When I think of the young faculty we recruit today--for instance, those we have attracted to staff the Structural Biology program in the Fairchild Building--they were given everything on a silver platter. We had to struggle for years with an inadequate physical plant, even after those old machines were replaced.

Meanwhile, in the early fifties another development became of overriding importance. I began to hear cocktail party conversations about an interesting new atom smasher being developed on the campus by Bill Hansen and Edward Ginzton and their colleagues. I became increasingly interested because of the properties of this machine. In the fall of 1951 I asked Yank Chandler to introduce me at a luncheon to Fred Terman who was then head of the School of Electrical Engineering, Leonard Schiff who was chairman of physics, and Edward Ginzton who in the meantime had replaced Hansen who unfortunately had died of pneumonia at an early age. I met Ed for the first time at that luncheon and, as you know, we have become lifelong friends.

He and I spent our time at that luncheon talking about the properties of the linear accelerator, and what we could do. One of the nicest things about it was that you could get very high energies out of it for a very low energy input. You

accelerated electrons with these high energies, and you could hit a heavy metal target and make them into X rays. I explained to Ed that's what radiotherapists were for years dreaming about. We needed much higher energies than we could possibly get from the crude devices then available.

Here was a microwave device which by the clever positioning of the electron pulse--just ahead of each microwave peak--could force that pulse to ride along the microwave at almost the speed of light. The farther it went the greater energy it got. This sounded miraculous and I became convinced that this was to become the radiotherapy machine of the future. Not only had I convinced myself, but by the end of that luncheon I had convinced all of them. And so a committee of Ed Ginzton and myself was formed to look into the possible applications of the linear accelerator in radiotherapy. But then sadly we ran into problems.

There was an MIT professor whose name is no longer important but who was a stockholder in the company that made Van de Graaff machines. Every time we submitted a grant application to the government he knocked it down. It took us more than two years in the richest country on earth to raise \$150,000. Hard to believe. Of course, in those days I had no wealthy contacts. It was strictly dealing with the American Cancer Society and the National Institutes of Health. But with

some additional help from the Office of Naval Research, we got started. Ginzton was in charge of the design and construction of the machine. I used to come to the campus from San Francisco as often as I could, to meet with him and members of his staff to see where things were. I participated, of course, most actively in the clinical design because I was interested in having a machine that was functional therapeutically, not just a gadget.

By 1955 we had our first linear accelerator for radiation treatment finished. We were about six months behind the British who had built a machine totally different from ours. The delay was, of course, because of the time it took us to raise the money. We began steps to have our machine assembled in the department in San Francisco. But there was a new problem. Before we could assemble the six-million volt machine, it was clear we had to have protection from its beam.

Wally Sterling again came to our aid. He went to the Irvine Foundation and they met with us. He made an impassioned speech and I made an ordinary technical speech. At that time it was publicly known that the medical school was preparing to move to Palo Alto within four years. But I said to the Irvine Foundation people that if they helped us, we could learn a great deal in four years of work in San Francisco, because the techniques of treatment with this machine were different. "This is like a rifle," I said. "Whereas the past machines had been

like shotguns. And we're going to have to learn our profession all over again. We're going to have to work out technical procedures that have never been done before. The opportunity to have four years to do that before moving the machine was to us crucial."

The Irvine Foundation made a one-time gift of \$75,000. We built a tiny concrete pillbox into the hillside. It was well-protected and had a hatch on top because we knew that someday we would have to bring a crane along to pick that machine right up through the ceiling and move it to Palo Alto.

In January 1956 the installation of the machine was completed and a team of physicists calibrated it. I should mention here that all this time, if I hadn't given free rein to Herb Abrams and Henry Jones to run diagnostic radiology, I would not have been able to do all this. I was devoting my efforts to patients, to the work on the linear accelerator, and to keeping my lab running.

The great day finally came and we were ready to turn on the machine for a patient. The true story of what happened is somewhat stranger than anything I could have made up. It was quite unbelievable. We had had many bull sessions of what kind of cancer patient would be our first referral. The very first patient was a seven-month-old baby with retinoblastoma of the only remaining eye, because surgeons had already removed the

tumor in the other eye. This was one of those genetically determined tumors since his father had had it. I don't think I will ever forget the puzzled look on the face of the garage owner down on Fillmore Street when I asked him to borrow a heavy duty automobile jack, and then explained that it was to carry a huge block of lead with a pinhole in it, to enable us to position that pinhole day-after-day for six weeks directly opposite the tumor in the baby's eye while missing the lens and the cornea. That boy is now in his twenties and doing very well, with his vision in the treated eye intact.

Q: Wasn't there some controversy generated by some San Francisco doctors who had questioned the use of the machine in patients?

A: That came mostly from a dirty bastard named Henry Garland. He didn't believe in high energy X_g rays. He was so cynical that he was once heard to say that the only reason he would like to have a cobalt machine was not to use it in order to prove to everybody that it wasn't essential. He was just a private entrepreneur but he hated me because he had the feeling that if I had not accepted the job at Stanford, he might have been selected instead. He had demanded at one time that he not only be the chairman of radiology at Stanford, but be permitted

to continue with his private practice at 450 Sutter Street. And the university would not agree to that. And he felt that I had pre-empted that possibility by accepting the chairmanship. So he hated me with a passion. And he had a scurrilous tongue which he used very freely. I dealt with him mostly by ignoring him. It worked.

Q: What do you think were the major milestones in your work, not only in radiotherapy but also the biology of cancer?

A: The fact that I was carrying a major laboratory program made matters difficult, because it did not directly parallel and support the clinical program. There was no easy way to do that anyhow at the time. But I did begin to build up a group of radiobiologists who could try to understand the fundamental mechanisms of radiation on patients. And we did develop an unusually strong program of radiobiology at Stanford.

 However, my own research started off with those mice. I could induce lymphomas of the thymus in them with radiation. At that time the absolute gospel was that this happened because of mutation. We proved little by little that the mechanism couldn't be mutation. And we succeeded in isolating a virus from these mouse tumors which would induce exactly the same lymphoma in unirradiated baby mice of the same strain. I called that the radiation leukemia virus. We spent probably the better

part of the last 30 years trying to understand how that initial exposure to radiation results in activation of such a virus. Only just now do we have some extremely exciting leads. It's curious that this work has gone from one end of my professional life to the other, and is still very active and in an extremely fascinating state.

Without going into major technicalities, there are two broad classes of these so-called retroviruses. There are the ecotropic viruses which grow in the species of origin. If you get a virus out of a mouse it will grow in mouse cells. If you get a virus from human cells it will grow in human cells. Then, some years ago, it was discovered that there is a very strange class of a virus known as xenotropic, which comes out of a mouse or other species, but the only strain on which it will not grow is its own. Yet it is clearly present in the genome of the cells--you can prove that beyond any question.

We have recently generated some strong evidence showing that in all of the virus-induced lymphomas a complete infectious virus is present from day one when you first harvest the tumor. That virus is ecotropic. However, when you take successive radiation-induced lymphomas, you'll find some of them will also produce virus which is ecotropic, will grow in mouse cells and cause lymphomas in mice. But the great majority of the radiation-induced thymic lymphomas seem to be devoid of

any sign of virus even though we know a virus is present. They present no viral antigens, nothing that grows when there's passage from cell to cell, and here is the great mystery.

I should explain that when xenotropic viruses were discovered, an all out effort was made to find what they do. People thought maybe they induce certain types of tumors, but nobody has ever induced a tumor with a complete infectious xenotropic virus.

Q: Did you find this discouraging?

A: Marty Scott, one of Kirk Fry's students, has now shown in about 15 consecutive radiation-induced lymphomas that a radioactive probe, prepared from a specific region of the envelope gene of a xenotropic virus, will detect in every single one of them, without exception, an altered size of messenger RNA. So far we have not found any tumor that lacks this messenger RNA. Currently, he is studying those messages through cloning techniques. Since we have a tumor already in existence by the time the message is detected, we suspect the xenotropic message for just this short region of the viral genome, the envelope gene, to be sufficient by itself to switch on or induce the tumor. Then, by happenstance, there may be a secondary event in which an ecotropic virus comes along in the same mouse or the same tumor cells grown in culture, undergoes

recombination with this little bit of xenotropic virus and rescues it, so what comes out is a recombinant virus with a small patch of xenotropic virus envelope gene in it, which carries the message for tumor induction.

If that secondary event happens, then you see virus. If it doesn't happen, then you're looking at apparently virus-free tumors. So we are dealing with a remarkable phenomenon which appears to be an alternative to mutation. We know that it applies not only to radiation but to certain chemical carcinogens or leukemogens as well. I think our lab has been the only one in the world to have been stubborn or foolish enough to have stuck with this. I don't know how globally significant it's going to turn out to be, but I think we really are now on the threshold of breaking open this very long-standing mystery.

Q: Some years ago when we talked about this, you mentioned how difficult it was to actually prove the existence of a human cancer virus. But you said your gut feeling was that in fact many lymphomas were induced by viruses. Can you comment?

A: The implications here are very straight forward. You don't have to have a virus in order to have a virus-induced lymphoma. If you have that little bit of genetic information of

the specific type, that may be all that's needed. Humans have few or no ecotropic viruses that could possibly come along and rescue that bit so that you could fish it out and detect it. Our discovery will lend force to that notion and open ways of looking now for such subviral entities. Until recently I think we've been looking at the wrong level. People were looking for typical mouselike viruses and that just didn't work. It's much more subtle than that.

Q: Besides developing successful treatments for Hodgkin's disease and the cancer virus work, what other milestones would you include in your career?

A: We made important contributions in several areas. One point perhaps I should make in connection with your earlier question of what makes for a good or great department is a point I frequently stressed to our faculty. It was that no matter what they were doing in their research, no matter what else was going on, it was a given that the quality of patient care in the department had to be absolutely first class. We would not undercut the quality of patient care just to find more time for our research. You may remember I had some memorable fights with Halsted Holman on issues relating to that. That's a point worth stressing.

In 1956, shortly after we treated the infant with the retinoblastoma, a urologist on our staff named James Ownby came by. He was like a little boy with an electric train. He saw all the red and green lights on our control panel and he was fascinated. And he became eager to have one of his patients treated on our machine.

One day he asked if I'd be willing to treat a patient with a large cancer of the prostate but with no known metastasis. The man was unable to urinate and Ownby was managing him with estrogens, an acceptable treatment at the time.

I told Ownby that I really didn't know whether we could safely irradiate the prostatic tumor in such close proximity to the urinary bladder and rectum. Nevertheless, he pleaded and I started this patient on treatment. Within a couple of weeks he no longer needed his estrogen. He could urinate all right, his prostate was down and was softer. We went on treating him for several weeks and that patient lived for years. So Jim Ownby began going up and down the area attending urological meetings, and drumming up business for us and we treated large numbers of patients with locally inoperable cancer of the prostate. Malcolm Bagshaw arrived shortly after I treated the first patient. As I got busier with Hodgkin's disease patients I asked him to take over the prostate work which was growing in volume.

We also evolved new techniques for treating cancer of the nasopharynx, many of them with lymph node involvement. These have resulted in survival data reported by Rich Hoppe several years ago which, as far as I know, are still unparalleled. We have, of course, done a great deal with cancer of the larynx of different stages, and the overall result has been very successful. In cancer of the ovary our former colleagues, Zvi Fuks and Eli Glatstein, had devised a completely new, rather aggressive treatment that covers essentially the entire abdominal surface. Their work has resulted in improved survival in ovarian cancer. Similar data were developed by John Earle for testicular tumors, with cure rates for some of these of 100 percent.

I would have to say in fairness that the recent introduction of chemotherapy regimens, including cis platinum, have made an even greater impact on advanced testicular cancers. Those are a few examples.

PART III

Q: How do you wish your research to evolve by those who succeed you in the years ahead?

A: That would depend on who is actually recruited, and

what his interests are. But there is a case to be made for the generation of human monoclonal antibodies to cancer-related antigens. If I were well, our program would be oriented in a strong way toward trying to develop such antibodies. I know this is exceedingly difficult. But antigens capable of stimulating human lymphocytes do exist in many cancers, and that would be the direction I would like to see followed. I know some of my young people want to continue with this research as long as they can, and certainly for the next six months or so. I see no reason why anything should interrupt them. But after that a lot would depend on the status of our laboratory funding and on the recruitment of a new endowed professor who will presumably take over the lab. His interests, I would presume, might be different. I would expect a lot of new, quite unrelated kinds of things would come to the lab over which I have no control. That's all I can say about the future of our program.

Q: Where do you see radiotherapy going as a specialty?

A: It's a tough situation. Many of the newer radiobiological approaches have been tried. It's true some have not been tried well and our own Norman Coleman and Martin Brown want to do them really carefully. Nonetheless, there is a good chance that the so-called radiosensitizers may not pan out as

an effective treatment. If they do their work well, Coleman and Brown will prove this point more conclusively than other people have done already. There is a second area of hyperthermia interacting with radiation, and Malcolm Bagshaw is very interested. The underlying mechanisms of hyperthermia are not well understood. They are being worked on carefully by people like George Hahn and others. They are making progress but they do not know why hyperthermia works when it does and why it fails. Nonetheless, Bagshaw and others have acquired machines that permit the introduction of heat deep within the body in a more directed way. To the extent they can do that now will take away one of the arguments that one couldn't focus these beams and get the heat directed to the tumor. It could be an enormous success if this were true, but it could also fail. It's a gamble.

Another area in which people are putting a lot of effort now concerns radiosensitizers. I had been pushing this idea for years, but no one has been listening.

Let me explain. There are three classes of radiosensitizers. One uses the oxygen effect, but I don't think it will be very important. A second category uses bromo-deoxyuridine, a drug that has to be introduced into the DNA of tumor cells in advance of radiation and must get in quite uniformly. The radiosensitization is very powerful. But

the usefulness of this approach is seriously limited by the consideration that not every tumor cell makes DNA at the same time.

There's a third approach that doesn't depend on either of these. That's the one that shows promise. It can best be called radiosensitization through inhibition of enzymatic repair. Radiation produces injury to tissues. A lot of that injury is repaired almost instantaneously by enzymes. People now finally are just beginning to look at the metabolic pathways of the repair system and to consider the use of drugs which can be introduced through a catheter during and after radiotherapy. The drugs would block the repair in irradiated tissues, but would have no effect at all on unirradiated tissues. I see that as being a valid area for exploration in radiotherapy.

There's also renewed interest in the combined use of external beam radiation and interstitial radiation. This approach does less overall damage to the tissues while still affording the same probability of eradicating the tumor. The technique is used with the greatest effect in early breast cancer. In that I think there will be a big development. We and Harvard have been using it. The method makes it unnecessary to remove all the breast. All one has to do is remove the small lump. It's most appropriate for patients with very small lumps.

If one removes the lump surgically and then does external beam radiotherapy and performs an interstitial implant, the long-term cosmetic result on the breast is nearly indistinguishable from that of the opposite unirradiated breast.

A more important consideration is that today many women are so fearful of a lump when they discover it in their breasts that they refuse to go to their doctors until the lump has grown much bigger and their chances of a cure have been seriously reduced. If they knew they did not have to lose a breast, that merely removal of this little lump followed by the radiation procedure would offer them exactly the same long-term survival, many more of them would come forward early. That would change survival. I see this development gradually pushing radical surgery for breast cancer into a much more restricted frame. And that's good.

Beyond that there are people talking about new machines and types of beams--neutron beam machines and negative pi mesons--but frankly at the moment I don't see any good arguments in their favor. There is a trial going on in Switzerland using a Stanford design for a negative pi meson machine that we would have liked to have here. If there is anything to it, they will come up with the hard data because they are doing the work carefully and objectively.

I don't appreciate any other major new thrusts in

radiotherapy that I can see, just as I see none in chemotherapy except with the advent of new drugs that are really effective and not too toxic. The last really good drug was cis platinum, introduced 12 or 15 years ago. That says to me that it isn't so easy to find new drugs that really have a different mode of action and are free of toxic effects.

Vince de Vita, the director of the National Cancer Institute, is and has to be, because of the nature of his job, very optimistic that we will continue to push the frontiers of cancer prognosis forward. I looked at this as dispassionately as I could before I got sick, not just now when I could take an unduly pessimistic view. But even before that time I felt I could really not see any major directions from which big increments in the results in cancer therapy were going to come.

Today there is a lot of excitement in oncogenes. It may be that some brilliant molecular biologist will be smart enough to figure out a way, not to put them back where they belong, because I don't think that would be feasible, but maybe to neutralize the effects of their abnormal expression and "put the cancer to sleep" so to speak. That I believe is a possibility but it's obviously a very speculative one. And it's going to take a long time.

My overall perspective on the cancer problem, however, is not very favorable except for those tumors that are due to

viruses where I think we can have a handle on the disease. It could be possible to work through the virus to get at the viral oncogene and hopefully to eliminate the virus from the tissue. That would be a way of achieving cure. But we are only beginning. We know very little so far about viruses in human cancers. As I mentioned Bob Gallo has succeeded in identifying a so-called HTLV human T-cell leukemia-lymphoma virus. This virus occurs in a selected group of T-cell leukemias and lymphomas in Southern Japan, the Caribbean basin, and adjacent shores of North and South America. That's fascinating and important, but it accounts for a very tiny amount of the total problem. It certainly has nothing to do with cancer of the lung or of the pancreas, for example.

In the last 40 years the cure rate of cancer of the pancreas has remained at 1 percent. It has not even increased to 2 percent. There, we see a measure of the challenge that still exists. I, for one, am not smart enough to see where to go to conquer cancer of the lung or pancreas or the esophagus, some of the major killers. Overall, I come down on a somewhat pessimistic side.

Q: Why is it so difficult to develop effective treatments against these particular cancers?

A: The main answer is early metastasis. We know very

little about how metastasis really occurs within the human body, except for general patterns of distribution. There are researchers doing all sorts of fancy and elaborate research on metastasis with mouse and rat tumors. They can isolate in substrains of tumor cells certain genetic markers or other traits and they know those cells will not metastasize at all. In other tumors they find different genetic markers or traits and they know they will metastasize very quickly. Every tumor, however, is a mixture of all of these things, and so it's likely to contain some cells that will metastasize.

The rate of metastasis in a cancer of the skin is known to be extremely slow. It's for that reason that we achieve very high cure rates. We remove the tumor and it doesn't come back anywhere else. In lung cancer, instead of something close to a 100 percent cure rate, the best that de Vita can report in his newest data is 12 percent survival, and the best he can report for pancreatic cancer is 1 percent. For cancer of the esophagus the survival rate is somewhere between 5 and 10 percent. And the answer is metastasis. These are silent areas, the tumors tend to get rather large before they can be detected, and the chance of metastasis is linked to the size and type of the tumor and to its location. One reason that lung cancer is so bad is that we are breathing 20 times a minute, and so we are massaging the tumor as often.

Q: Are you concerned about the level of cancer research funding? Are there areas of cancer research for which you'd like to see more money spent?

A: For some years viral oncology was underfunded after a brief period of generous support. But then the environmental carcinogenesis people jumped into the act and made big noises. These are people like Bruce Ames of the University of California, who argued that we must identify every mutagen in the environment. Today we know that most mutagens are not cancer inducing so that a lot of that thrust was false and unwarranted. Agents that can break chromosomes or alter chromosomal structure are much more dangerous. I suppose you can refer to them as mutagens, but that definition to me is outside the general meaning of the term, mutation. So structural rearrangements of chromosomes are meaningful in cancer and what brings them about is an important area of future research.

As to funding, the biggest problem I see is the politicization of the boards that are called upon to make final decisions, at least at the federal level. Today, the National Cancer Advisory Board is a heavily politicized body with relatively little scientific competence. That was not true 15

or 20 years ago. I think each president in recent years has carried this one step further, and Mr. Reagan has really done it much more than before. Now, we have Republican businessmen and Republican writers and conservative right-wing thinkers, all of them pronouncing judgments on how funds for cancer research should be allocated, while a tiny handful of very frustrated, competent scientists on the board cannot get their opinions through. I think that's the most dangerous thing. It has not yet happened at the lower echelons of the National Cancer Institute. The boards of scientific counselors of the different branches of the Institute are still made up entirely of scientists and physicians, and they really function very well. How long this will last I don't know. But I know that de Vita is much aware and concerned about this problem.

Q: You mentioned recently that you have some yet to be published important research. Can you tell me about it?

A: We have developed two new sets of human monoclonal antibodies. One of them protects against endotoxin, and thus should contribute to a decreased mortality in gram-negative sepsis, which is a major problem throughout the world. It's estimated that in this country alone there are about 100,000 cases a year, with about 35,000 deaths. In many instances antibiotics not only don't help, but their use makes things

worse by causing the release of endotoxin from the bacteria as they are killed. That in turn causes circulatory collapse and shock.

We have developed a highly potent broad spectrum anti-endotoxin. Stanford has applied for patents, and we are currently negotiating with some companies to select one or more. We want to have this scaled up first and carry out clinical trials, then hopefully market it with the usual royalty agreement with Stanford. So that development will generate royalty income to support research in the lab in the future.

The other set of antibodies we have developed are directed against the blood group antigen Rh Type D, the major Rh antigen. About 15 percent of people are Rh negative and 85 percent are Rh positive. If an Rh negative woman is married to an Rh positive man, there is a very strong chance that the first progeny will be Rh positive. Some of the blood of that baby during birth, through the force of the delivery, somehow finds its way into the maternal circulation so that now Rh positive cells, which are foreign to the mother, are residing in her circulation. And obviously it doesn't take long until her immune system will make antibody to this antigen.

During subsequent pregnancies with Rh-positive children, the process is repeated, stimulating an increasingly

strong response, with more antibodies formed. These antibodies move across the placenta into the fetal circulation now destroying the red cells of the baby. The baby will develop jaundice and be mentally retarded. Often the baby is born dead. If the baby is born alive a technique of exchange transfusion has been developed to save his life. Another way to prevent this from happening though is to give the Rh-negative mother an injection of serum containing powerful anti-Rh antibodies, which sweeps the baby's red cells out of her system.

Today in some countries there are extreme shortages of immune serum. In this country there are about two of three companies supplying immune serum, but their supplies are also dwindling. These companies often have to procure some of their blood products from people who may have hepatitis or AIDS, whereas a monoclonal antibody is a pure substance.

We are sending manuscripts of this work to the Proceedings of the National Academy of Sciences very shortly. Dr. Abe Braude at UC, San Diego, the chief of infectious diseases, has worked with me on the anti-endotoxin project.

Q: Let's go back to the question we left unanswered last time. What role did you play in the recruitment of faculty before the medical school moved to the campus?

A: My role had to do mainly with two faculty

members--Arthur Kornberg and Joshua Lederberg.

With Arthur, his entire team of what was then the department of medical microbiology at Washington University in St. Louis came right along with him. I suppose I can take some of the credit for that. It happened this way. I had been invited to Harvard to become the Cook Professor and Chairman of Radiology of all the Harvard-affiliated hospitals. It was a tempting offer and it came at a time when I was losing my conviction that the new school when it moved down really would become a first-class scientific school. I felt that way because I saw no action on the part of anybody in the recruitment of a new chairman for the department of biochemistry, which did not exist except as a small division in the department of chemistry, with a very limited research base.

Fred Terman, who was then university provost, became chairman of a search committee to recruit the biochemistry chairman. I was beginning to lean more toward accepting the position at Harvard. Leonard Schiff, who was a very dear personal friend, was then on sabbatical leave in Paris. He had sent a very anguished telegram to Wally Sterling saying, "For Christ's sake, do something, don't let this happen." In time I was asked to come to the campus and see Terman. As we talked, I was amazed by his negative attitude, because it was clear he didn't think he could compete with Harvard. But I made it clear

I really didn't want anything for myself. I was already well on the way to building a fine department. I didn't need anything that I couldn't get in the normal course of events.

I told him what I wanted--literally the word was "playmates," intellectual playmates. I said the medical school had been so strongly clinically oriented--and I have no quarrel with strong clinical care because I believe in it and I practice it--but I also believed in fundamental basic research in order to make future care better than what we can do today. I said, for example, there's biochemistry. We've talked about it now for two years, but there has been no action that I know of.

On the contrary, Terman said, I am the chairman of the search committee. He reached in the drawer and he fished out a list, and there was Arthur Kornberg's name at the top of the list.

I said that's wonderful, because Arthur is a dear friend and one of the greatest biochemists in the world. But I added that having him on the top of the list and getting him to come to Stanford are two different things. I said I hope you'll pardon my being cynical, but I've watched how Stanford does some of its recruiting. Typically, from what I've been able to observe in some other recent episodes, you wait until you hear that the guy has been invited out to give a seminar at Berkeley

so that you won't have to pay his plane fare, and then you'll invite him across the bay to the Farm and you'll talk to him about the sunshine, the climate, and the bay and you won't offer him any budget or any space, and then you won't understand why he won't come.

Terman turned kind of green, because this was a rough way to talk to a provost and especially one as tight-laced as Fred Terman. But I didn't give a damn, I had nothing to lose. He said, "What would you like us to do?"

I said, "I'd like you to invite Mrs. Kornberg with Arthur on the first visit. I'd like them to come first class. I'd like to have a car waiting for them at the airport, preferably a convertible. I'd like them to have a suite at Rickey's. All these are just creature comforts, but they make a difference because they indicate the level of your interest. Beyond that, I want you to promise him every square inch of space and every dollar of budget that he asks for, because I know Arthur well enough to know that he won't ask for more than he can use."

As I got up to leave I said to Terman, "I'll make it very simple for you. If you recruit Kornberg successfully, I'll stay. If you fail, I'm going to Harvard." It was just as simple as that.

Terman's jaw, of course, by this time was hanging kind of slack. But he was as good as his word. A few days later

Arthur and Sylvie came out, and before he returned he had accepted the Stanford job.

A few months after that I was returning to Stanford from Bethesda by way of St. Louis to offer whatever help I could to Arthur in getting his department ready for the move to Stanford. There was a party that evening at Paul Berg's to celebrate this great new venture. Just as we walked in the door, the phone rang. It was Lederberg wanting to speak to Arthur.

Lederberg simply wanted to know what was going on at Stanford. "I was out there last year looking at a job in biological sciences," he said. "It seemed like the same old sleepy place as ever, and I turned them down. But now I hear that Henry is staying and you are going. I want to find out what's happening to inject this degree of excitement."

We took turns on the phone and talked about the new and improved curriculum, outstanding students and so on, and Josh said, "Gee, that sounds wonderful! I'd be very interested."

I flew back to Stanford that night and the next morning I went in to see Bob Alway. This time I didn't request, I demanded that he forthwith create a new department of genetics, which didn't exist in the medical school, with Josh as chairman. Alway had to dig down into some temporary dean's funds in order to get it off the ground. Alway, to his

ever-lasting credit, was never afraid of people who were smarter than he was. And he was not afraid to do battle with the university administration if the issue seemed important enough. That's how it happened. And, of course, the two Nobel prizes came a year apart at just about that time. Because of that, suddenly Stanford was catapulted literally from a second or third rate clinical school into some kind of mysterious but very exciting place that students thought of in the context of Harvard.

Subsequently, more faculty were recruited, some good, some not so good. We had, of course, Avram Goldstein who was as good as gold, a very strong effective leader. Alway was dean and was doing a good job. But one of our most terrible mistakes that started us on the road to losing our momentum was the selection of David Hamburg as professor and chairman of psychiatry. I take personal responsibility, because I was chairman of the selection committee. It took me three more years to realize that he was the original three dollar bill. The second bad mistake was the selection of Halsted Holman as chairman of medicine.

Q: Do you think the school today offers the same excitement as it did when it was first started--especially in its ability to attract topnotch faculty, or has it lost its momentum?

A: Losing momentum is a very real threat. Another threat is that over the years quite a number of people have been brought in for not absolutely first rate work. They are better than mediocre certainly but not great. Our ratio of Paul Bergs to other types of people is gradually changing. Another aspect that's important is perhaps inevitable. When we were a little group, even though there were 16 departments, only seven departments were calling the shots on everything. We would meet evenings and weekends to try to keep up with our own work as well as the planning for the school. We'd meet with Alway and try to function as a very active and dynamic kind of sub-executive committee. There was tremendous idealism and a willingness among us to give up things for the greater good of the school. The school improved dramatically because of that. I don't see that anymore. Perhaps this is a function of size, but it seems there are an awful lot of people more concerned about maintaining their own territory and don't want anything that would expose them to possible discomfort or loss of power or anything that might help the school, but not directly help them. The dean has a tremendous problem in that whole area of motivation and I don't know how he's going to get around it.

Q: The same question comes to mind when you consider what

happened with the faculty practice plan. Why after 20 years of debate about it, we still have no plan that satisfies everyone?

A: It's too painful for me to talk about it. The major reason is simple. We have never been a true faculty practice except for a brief period before Bob Glaser's arrival as dean. We used to have a faculty practice plan run by the chairmen of the clinical departments. They arrived at 7:15 a.m. every Wednesday morning and met with the lady who was doing all the accounting and bookkeeping for the practice. After a year or so we got her an assistant. But we were making money. We were willing to distribute some of that money to the dean as long as the earning faculty got their share. When Glaser came he made it an absolute issue that he take over that program. That very nearly destroyed the school, and it almost led to my departure. What we have had since then has not been a faculty practice plan--it has been a dean-run practice supposed to be carried out by the faculty as hired hands. It's not our own practice plan. And even now, in my view, we have to ask only one question. It's the acid test. That is, who appointed and who can fire Don Tower. You know damn well it's not the faculty. With each of the respective deans, every single one of them after Alway, the faculty was given no incentive to give a damn. They were helpless. They had no control over how these pieces

of paper were shuffled. And this has led to losses of millions of dollars of faculty hard-earned income.

Q: This is my final question. It's a difficult one to ask, but it would be important to do so. The question is how would Dr. Kaplan like to be remembered?

A: That's a tough one. Well, I guess I'd say, on the one hand, I'd like to be remembered for those of my accomplishments that stand the test of time such as Hodgkin's disease and malignant lymphomas. That is an area where there will be continued further improvement, but I think we contributed a foundation stone which today is leading to the cure worldwide of hundreds of thousands of patients.

It's hard for me to know whether my work on mouse virus or radiation-induced lymphomas will be remembered or not. I happen to think it's very interesting, but I have the feeling people tend to gloss over it.

I'd like to be remembered as the co-developer of the medical linear accelerator for cancer treatment, which today is a standard of excellence throughout the world, and has dramatically improved cure rates for many types of cancer despite of what I said about cancer of the lung and pancreas which are among the areas of failure. There are many other cancers in which radiotherapy is extremely effective and has

changed prognosis significantly. That's another area I'd like to be remembered for. And not just for developing the machine, but the standards for its use. We had to learn to think differently since we were working with a rifle and not with a shotgun. We had to develop techniques of precision that were utterly new. Those are part and parcel of that development.

I'd like to be remembered for my service on the National Cancer Advisory Council in 1960 at a time when the total number of radiotherapists in the United States was about 120 and the number of physicians in radiotherapy training was 18, of whom 6 were from other countries and scheduled to go back to their native lands. In other words we were not training enough people to take care of our own natural attrition. I was asked to give a lecture on radiation therapy to the Cancer Council, one of several such invited lectures at a time when that body wanted to educate itself. I decided not to fear washing dirty linen in public. So I brought out these numbers. I pointed out that radiotherapy was dying of attrition. The presentation must have been compelling, because the council voted to have the National Cancer Institute director, Kenneth Endicott, create a committee to look into this. Six weeks later Endicott came into my office at Stanford, sat down, and in his typical, and distinguished gravelly voice, he announced that he had been asked to form the committee, and he added, "Henry, you

and I both know that the optimal size of any committee is one. I'd like you to be the committee. You're free to consult anyone you want to, but you call the shots on what you recommend."

I talked to other leaders in the field extensively. I finally put in a program both for research training and clinical training with decent stipends, and it was accepted with enthusiasm. Today there are close to 2,000 board-certified radiation therapists.

I was also instrumental in fighting a battle with the American Board of Radiology to certify specialists in radiotherapy alone. At one time you were forced to do both diagnostic radiology and therapy, and I won that battle too. Today, there are departments of radiotherapy of good quality in a high proportion in all the medical schools in the country, and some of the major community hospitals. That went hand-in-hand with the linear accelerator. There was no point having an expensive gadget, which was superior to all other known gadgets, without enough people around who knew how to use it. I believe I'm generally credited as the person who created that.

Those are just accomplishments. As for the rest, I guess I'd like to be remembered as somebody who has been basically kind and deeply concerned about his patients, and very humane in dealing with them. At the same time I want to be remembered as somebody who was tough enough to be willing to

fight the battles with a number of deans--battles that were needed to create and maintain high standards not just for our department but for the school. I fought more battles that had nothing to do with radiology than almost anybody in the school except Avram Goldstein. And he'll probably back that up.

So viewed from one side I think I was a kindly physician, a role model. Viewed from another side I was a malignant son-of-a-bitch that drove deans to despair and was known as a dean killer. These two kinds of things were sort of hard to reconcile. I must say I never enjoyed fighting with deans, I never enjoyed it for one minute. It took an awful lot out of me. But if I believed in what I was fighting for, I didn't care how hard I fought.

I guess I'd also like to be remembered as somebody with a reasonably good sense of humor, with a love of art and music and literature, and hopefully as a good husband and a good father and a loyal friend. That's about all I can say.

end