

John Kersey, M.D.

Narrator

Dominique A. Tobbell, Ph.D.

Interviewer

**ACADEMIC HEALTH CENTER
ORAL HISTORY PROJECT**

UNIVERSITY OF MINNESOTA

ACADEMIC HEALTH CENTER ORAL HISTORY PROJECT

In 1970, the University of Minnesota's previously autonomous College of Pharmacy and School of Dentistry were reorganized, together with the Schools of Nursing, Medicine, and Public Health, and the University Hospitals, into a centrally organized and administered Academic Health Center (AHC). The university's College of Veterinary Medicine was also closely aligned with the AHC at this time, becoming formally incorporated into the AHC in 1985.

The development of the AHC made possible the coordination and integration of the education and training of the health care professions and was part of a national trend which saw academic health centers emerge as the dominant institution in American health care in the last third of the 20th century. AHCs became not only the primary sites of health care education, but also critical sites of health sciences research and health care delivery.

The University of Minnesota's Academic Health Center Oral History Project preserves the personal stories of key individuals who were involved with the formation of the university's Academic Health Center, served in leadership roles, or have specific insights into the institution's history. By bringing together a representative group of figures in the history of the University of Minnesota's AHC, this project provides compelling documentation of recent developments in the history of American health care education, practice, and policy.

Biographical Sketch

John Kersey was born in Minneapolis, MN, in 1938. He received his BA from Dartmouth College in 1959. He began medical school at Dartmouth Medical School and finished at the University of Minnesota Medical School, receiving his MD in 1964. He interned at Ancker Hospital in St. Paul. From 1967-68, he served in the Army Medical Corps. He then returned to the UMN, completing residencies in pathology and pediatrics. Following his residencies, he was appointed to the faculty, serving as Assistant Professor in the Departments of Laboratory Medicine, Pathology, and Pediatrics (1971-74) and Associate Professor (1974-77) and Professor (1977-the present) in the Departments of Laboratory Medicine and Pathology, Pediatrics, and Therapeutic Radiology. From 1974-95, he was the director of the Bone Marrow Transplantation Center. He was acting director (1991-95) and director of the UMN Cancer Center (1995-2007). He is currently Founding Director Emeritus of the Cancer Center and the Children's Cancer Research Fund Land Grant Chair in Pediatric Oncology.

Interview Abstract

John Kersey begins by describing his background, including his education and why he went into medicine. He describes his experiences during his residencies, being appointed a Medical School faculty member, and as a faculty member. He discusses faculty and research at the UMN Medical School while he was a student, the reorganization of the Health Sciences in 1970, the effort to establish a children's hospital in Minneapolis in the 1960s, relations between UMN faculty pediatricians and community pediatricians, teaching, Homecare for the Dying Child Program, and hospitalists.

He talks extensively about cancer research and treatment work, touching on topics including his own research and other work, funding and the NIH, clinical research versus laboratory research, informed consent and medical ethics, cancer research in the 1970s, the development of medical and pediatric oncology and chemotherapy, bone marrow transplantation, cancer research funding, the bone marrow transplantation program, nurses who worked on cancer treatment, the Masonic Center in the 1970s, the Cancer Coordinating Committee, the development of organ transplantation treatments, the Comprehensive Cancer Center in the 1980s, the Cancer Detection Center, ALG, experimental treatments, and clinical research. He talks about James Dawson, Mead Cavert, and Robert Good.

Interview with Doctor John Kersey

Interviewed by Dominique Tobbell, Oral Historian

**Interviewed for the Academic Health Center, University of Minnesota
Oral History Project**

**Interviewed at 554-E Masonic Cancer Research Building
University of Minnesota Campus**

Interviewed on May 9, 2011

John Kersey - JK
Dominique Tobbell - DT
Emily Hagens - EH

DT: This is Dominique Tobbell and I'm here with Doctor John Kersey and Emily Hagens. It's May 9, 2011, and we're in Doctor Kersey's Office which is 554-E Masonic Cancer Research Building.

Doctor Kersey, thank you for agreeing to be interviewed.

JK: Thank you.

DT: To get us started, can you just tell me a little bit about where you were born and raised and, generally, your educational background?

JK: Okay. I was born in Fairview Hospital in Minneapolis in 1938, raised in South Minneapolis, attended Washburn High School in Minneapolis, graduated in 1956, attended Dartmouth College, followed by Dartmouth Medical School, followed by the University of Minnesota Medical School because Dartmouth Medical School, at that time, was a two-year school, so I had to transfer somewhere after two years.

DT: What led you to Dartmouth in the first place?

JK: [chuckles] I went to Dartmouth because a friend of mine of had gone there the year before.

DT: [chuckles]

JK: And I didn't know very much about colleges. It was the only college I applied to. I don't know if I had not been accepted where I would have gone to college. [laughter] That's basically the story.

DT: Your decision to come to finish your last two years at the Medical School at the University...what was your rationale for that?

JK: Actually, that's kind of an interesting story. After my first year at Dartmouth Medical School, I wasn't sure that I liked medical school, so I took a year's leave of absence and came back to Minneapolis, came back to the University of Minnesota to do something different. I obtained a job that summer in one of the research labs, and enjoyed the research so much that I continued that whole year there. Then, I went back to Dartmouth College for my second year of medical school and, then, back here for my last two years. So my career started with my not being very happy with medical school, because I thought it was too much memorization and not very interesting, but then I got excited about the research.

DT: Whose lab were you working in?

JK: I was working in the laboratories of two people: [Doctor] Carlos Martinez in the Department of Physiology and Doctor Robert Good, who subsequently became my mentor. I was doing research with newborn mice, about an inch and a half long, and under a microscope taking out their thymic organ, the thymus and, then, looking at the effect of that on their immunity.

DT: What led you then, once you did complete medical school, to pursue residency in pediatrics, or was it pediatrics and laboratory medicine pathology?

JK: After I finished medical school, I did one year of internship at the old Ancker Hospital [Saint Paul, Minnesota]. That was followed by a two-year stint in the Army Medical Corps in 1967 and 1968. Then, I returned to the University, first to do a residency in pathology, and a residency in pediatrics after that. I was interested in those two areas because those were the areas that my primary mentor, Robert Good, was involved in. He was doing research in both of those areas, experimental pathology and pediatrics.

DT: So you knew from that first experience working in his and Carlos Martinez' lab that you wanted to pursue research?

JK: Well, I thought I did. I was interested. I found it very exciting. Yes, I thought it would be really interesting to pursue research.

DT: At that time, the Medical School...was it standard practice to still get Ph.D.s or, at that point, was it still the M.D. degree and if you wanted to do a master's or Ph.D. that you could do that?

JK: I would say at the time, it was unusual to obtain a Ph.D. and an M.D. degree. It was more likely that you would just have an M.D. degree, which is what I did. There were occasional people that did also obtain a Ph.D. degree, but not nearly the number as do it now.

DT: I just remember seeing that Owen Wangenstein kind of instituted that within the Department of Surgery, that there would be more of an emphasis...

JK: Yes, some departments had it, but I would say for most people who were doing research work, M.D.s, were not obtaining Ph.D. degrees. That's not to say that we didn't have to have a lot of research training, because, subsequently as the years went by, I had more and more research training, following and during my residencies in pathology and pediatrics.

DT: What are your experiences or memories about Medical School here? Were there any kind of notable stories, notable faculty members?

JK: Yes, it was an exciting place here when I was a student. It was a time which was very exciting because there was so much going on in the field of immunology with Robert Good in Pediatrics. There was so much going on in cardiovascular surgery. I remember C. Walton Lillehei in open heart surgery and watching a couple of those cases. But, also, it was a time when there were some other real giants who were here. C.J. Watson was head of the Department of Medicine. He and other people who influenced me. It was an exciting time.

DT: I forgot to ask earlier or maybe you said... What led you to go medical school in the first place?

JK: Oh! What led me to go to medical school in the first place? Well, I had an ophthalmologist doctor who was looking after my eyes at that time, Malcolm McCannel, who was a well-known ophthalmologist, who said I should go to medical school. No one in my family had been in medicine, but it seemed interesting to do, so I did it. It's been an interesting trip.

[chuckles]

DT: What was it like doing your residency, a dual residency essentially, one in pathology and one in pediatrics?

JK: It was interesting because when I was in pathology, I learned how to use a microscope, and I learned how to look at cells, and I learned how to look at tissues and look at disease from that process. Then, of course, in pediatrics, it was different because there we were working with children who had diseases. At that time, of course, the Department of Pediatrics was a very strong department...John Anderson, the chairman, and Paul Quie, and Bill [William] Krivit, and many others were interested in serious

childhood diseases. Pediatrics is interesting because you learn that children either have a common problem which they recover from no matter what you did with them or they have, unfortunately, uncommon, serious disease, which is very, very difficult to treat. It was the latter, the serious diseases that were very difficult to treat that were of interest to my teachers here and really the focus of the research. It was really an interest in serious, potentially fatal, childhood diseases that made this interesting and exciting to me.

DT: When you were dealing with sick children that, obviously, must be particularly challenging. Was that the case? How did you deal with that and with the parents?

JK: It was difficult. It was very difficult, because, as you say, the children were very ill. Many times, they had either heart diseases that they were going to die from or they had leukemia which they were going to die from or they had some kind of severe immune disease that they were going to die from, so it was difficult for the parents and for the physicians and for the nurses. It was difficult for everybody.

We didn't have a lot of things that we take for granted now. For example, nowadays, you assume that if you're going to have some blood drawn, doctors will have sharp needles, and you have somebody who is an expert especially in drawing blood from small children or babies. At that time, you didn't have people that were so expert, and we didn't have very sharp needles. So just the business of taking blood from children at that time was difficult. Now, there are catheters that are put in that make it very easy and we have experts who draw the blood.

DT: Given the nature of the children's sickness, did you have a lot of interaction with other departments in their care?

JK: Yes, I would say so. I would say there was really good interaction, particularly between the Pathology Department and the Pediatrics Department. There was also a Department of Laboratory Medicine. Now Laboratory Medicine and Pathology are joined as one department. But at that point, they were two.

DT: Yes, I'd forgotten that they were separate at that point. Who was the chair of Pathology?

JK: James Dawson. Dawson was the chair, Jim Dawson.

DT: What was he like to work for?

JK: He was really interesting, because he was, like most pathologists, interested in looking into the microscope and trying to figure out from the microscope the nature of a patient's disease. So he was a very good diagnostician and was able to diagnose diseases very well.

DT: I would imagine that in Pathology you have a lot more interaction with other departments?

JK: Yes, that's true. One of the areas where there was a lot of interaction was at the time of autopsy. Autopsies used to be very common. Unlike today where we take a lot of tissue biopsies, we didn't do that then. Often the cause of death wasn't known until people would gather around the autopsy table when somebody died—dissection would be done by pathologists—people from Pediatrics, if it was a child, or Medicine or Surgery, if was an adult. Yes, there was lots of interaction. Of course, the Department of Laboratory Medicine was involved in all the laboratory tests from the bloods that were drawn.

DT: When did biopsy tissues become more common, say, with oncology? At what point did the shift from autopsy to more biopsy take place?

JK: As there began to be more treatments available for diseases, it became apparent that in order to properly treat scientifically, you had to know what the diagnosis was. So people said, "Well, let's do biopsies." So during the 1960s, 1970s, 1980s more and more biopsies were done on all kinds of organs looking for disease. That was a huge change in terms of the practice of medicine, because, then, you had tissue to make a diagnosis and you perform the treatment before the child or the adult died of their disease. So there was a whole mindset stating that we can actually treat, potentially, so let's find out what the cause is before we treat.

DT: When you were doing your residency there were a number of diseases that you could diagnose by pathology?

JK: Oh, yes. For example, with leukemia, which is a disease that I became really interested in, it was very clear that you could make the diagnosis best by doing a biopsy of the bone marrow, because that's where the disease originates. So, yes, the biopsy of bone marrow became the standardized test for looking at leukemia. Now, in the very early days, a hundred years ago when people were just beginning to discover leukemia, they looked only at blood. When the disease becomes more advanced, then you can find it in the blood, but in earlier stages, you can't. So, yes, that's been a revolution in the last thirty years, largely with the increased use of biopsies for making diagnoses.

DT: Were you relatively rare among residents to do this combination of residencies?

JK: Yes, I was pretty unique, I would say. Yes.

[chuckles]

JK: At that time, it was interesting that people were less concerned about having exactly so much time in your residency and doing exactly this, that, or the other in your residency. Things were much more flexible. So it was easier, I think, for residents to try different areas, different departments, than it is now—although it's not impossible now. I think it was a little bit easier partly because things were less complicated. There were fewer departments and there were fewer people in the department. I was not a faculty

member at that time, but it seemed that all the faculty members knew each other well and, often, that doesn't occur today.

DT: When you finished your residencies, did you then get appointed on the faculty?

JK: Yes, I did. I was appointed on the faculty. I had sort of a shortened residency program and I was appointed on the faculty while I was pretty young, probably earlier than I should have been.

[laughter]

DT: What was the culture of the Medical School like from the perspective of a faculty member then? That would have been late 1960s?

JK: Yes. I was first appointed on the faculty in 1971.

DT: Okay.

JK: The culture was one of, I think, a lot of collegiality, not that there weren't plenty of fairly ambitious, and I could say fairly aggressive, people who were on the faculty. They were bright, and they worked hard, and they made a big name for themselves and for the University of Minnesota Medical School, at that time, so it was a pretty exciting time.

DT: You were doing your residency while Bob [Robert] Howard was dean of the College of Medical Sciences?

JK: Yes, part of it, yes.

DT: What was your experience of him as dean, if you had any?

JK: I must say I didn't have really much contact. I did not really. [laughter] In subsequent years, I had lots of contact with his son [Greg] and his daughter-in-law [Joanne Howard]. His son is a cartoonist. I don't know if you know that.

DT: Yes.

JK: His daughter-in-law was a nurse who worked with us in the BMT [Bone Marrow Transplant] program.

DT: Oh, I see.

JK: Joanne Howard was probably ten years or so with the BMT program. She was one of our leading nurse coordinators.

So, no, I didn't have very much to do with him, so I can't really say very much about him. I was really dealing with the faculty and the department chairs rather than the dean.

DT: During that transition from when you were a resident to a faculty member, the health sciences were being reorganized and the College of Medical Sciences was disbanded. Did you have any perspective on that from your position as resident and then faculty member?

JK: I'm not sure what you mean by the college being disbanded.

DT: The College of Medical Sciences, which had included Public Health, Nursing, Medicine, and the hospital, was disbanded, and each of the schools or colleges was appointed each its own dean.

JK: I see what you're saying. No, I didn't really have anything to do with that. I was not involved in that in any way.

There was no question that there was lots of reorganization that went on over the years, especially in terms of definition of roles of department chairs versus deans. This was significant for cancer, as we got into the where and how there was a lot of tension about who was in charge of things. Were department chairs in charge or were deans in charge of things?

DT: That's interesting. You reflected on that after the fact, but not while you were a resident or young faculty member.

JK: Yes. I was involved, as I think one should be at that stage of your career, in doing real important stuff like taking care of patients and trying to understand diseases. I wasn't very involved or very interested in those administrative issues, I guess. And no one asked me my opinion, so I didn't give it.

[laughter]

DT: It's actually really interesting because a lot of the first people that I interviewed from the Medical School had spent time in the dean's office during the 1960s and 1970s, so had a very different perspective in that they were very much engaged and embroiled in some of those conflicts and tensions. Now, I've spoken to people who were primarily clinicians and faculty members who... Yes, you don't see it, so it's a really valuable perspective.

JK: So my major involvement with the Medical School administration was when I was a medical student. I had made plans to be in Sweden to do a brief stint student as a fourth year medical student. I wasn't a very savvy young guy, so when I bought a ticket from the Minnesota Student Association to go to Paris by way of Pakistan International Airlines...

DT: [chuckles]

JK: Being so naïve as I was, my plan was that I was going to split a ticket one way and another person was going to split a ticket the other way. I got as far as New York City on my way to Paris and when I was to get on Pakistan International Airlines, the ticket didn't have my name on it. It had the name of the person who was going to return. They said, "Well, you can't fly. This is not your ticket." [laughter] "I paid for it." Well, it turns out that I lost. They said, "We think you probably stole the ticket." I was desperate. I didn't have any money. So I called up the Medical School associate dean, who then was Mead Cavert and I said, "Mead, here I am in New York and I'm on my way to Paris to do my stint in Sweden, and I don't have a ticket, and I don't have any money." He was very generous to me. He found out how I could get a loan from the Minnesota Medical Foundation, so I could buy another ticket. That was my major action with the dean's office.

[laughter]

DT: From what I've heard, Mead Cavert was very accessible.

JK: He was. He's a wonderful person. In fact, I see him a lot, because we both belong to the Minnesota Academy of Medicine and we go there together. Was he there the night you were there?

DT: Yes.

JK: He's a good leader. He was really accessible for students. We were lucky to be able to have him. Yes.

DT: It sounds like it.

[chuckles]

JK: In fact, the story is even funnier. He helped me get some money so that was fine. But, then, I still didn't have a ticket, so how was I going to get the ticket? I started shopping around and talking to friends of mine in New York, "How can I possibly buy a ticket?" There was a fellow I reached on the phone in New York who said, yes, he could get me a ticket to Paris. I had to appear at a certain street corner in Brooklyn that next day at ten o'clock in the morning with a hundred and fifty dollars and he'd give me a ticket. So I arrived there and I gave him my hundred and fifty for the ticket and he said, "Well, we don't actually have tickets on our airline, but I can assure you you're going to get on this airline." The thing was actually, it turned out, an illegal, unapproved airline called Trans International Airlines, a propeller plane. [chuckles] It had four stewardesses, all of whom had different outfits on and served cold lunches. It had a plane breakdown in Gander, Newfoundland, on the way to Europe. Then, after I got back home after this was all over, months later, the FAA [Federal Aviation Administration] came to me and said, "We're investigating this airline. Tell us about this."

DT: [laughter]

JK: The whole story is kind of a strange sidelight.

DT: You must have been brave to get on that airline.

JK: Either brave or stupid.

[laughter]

JK: When the plane broke down in Gander, Newfoundland, since this was a non scheduled airline, they had no maintenance plans, so the pilot got out his hammer and was hammering the engine. It took us twenty-four hours to get from New York to Paris.

DT: That's a little scary.

JK: That's a little scary, yes. Anyway, that's my connection to the associate dean.

DT: The random stories that you get from interview questions.

JK: Right. These things never appear in print—fortunately.

DT: It seems like there were a number of developments within pediatrics in the Twin Cities during the 1960s and the first was the establishment of the Children's Rehabilitation Center in 1964. Was that when you were a medical student?

JK: That was while I was a medical student, so I was not really involved in that. I just knew it was happening.

DT: Then, later in the 1960s, there was an effort by physicians to establish the children's hospital in Minneapolis.

JK: Yes.

DT: Can you talk about that?

JK: That was a matter of a lot of controversy. I was young and pretty naïve, but I couldn't see why there was a need for a children's hospital when we had such a fantastic facility here. By hearsay, I learned that there was some conflict between the surgeons about who was going to do pediatrics surgery. Tague Chisholm, who was a very good pediatric surgeon, I understand, had some conflict with Owen Wangenstein about the specialty of pediatric surgery. This was a problem in these days, where some surgeons thought they could do every kind of surgery, and Chisholm said, "No. We need to have specialists." That was one of the reasons, as I understand it.

Then, another person who was very actively involved in this was Arnie [Arnold S.] Anderson, who was a pediatrician who was in practice. He was somewhat interested in having a separate children's hospital. As I said, I wasn't directly involved.

DT: I interviewed Arnie Anderson last year sometime, and it had sounded quite contentious. He had mentioned that John Anderson was particularly reluctant to support the children's hospital.

JK: Yes. As far as I know, that's true. The sense was, as far as I can tell, the pediatricians couldn't see any need for it. I can't tell you what Owen Wangenstein's view of it was except that there seemed to be this conflict between him and Tague Chisholm. It's one of those things that if you're not directly involved, it's hard to know where the truth lies.

DT: Did you get the sense that there was any kind of tension between the pediatricians who were on the faculty and those who were working in the community, like Arnie Anderson, that there was some kind of town/gown tension?

JK: Amongst pediatricians?

DT: Yes.

JK: Oh, I think there probably was, but I wasn't really directly involved enough to be able to comment on that.

One thing that struck me goes back to something I said earlier and that is in caring for children, you either tended to take care of healthy children who have fevers and colds and well baby care, or else you took care of sick children. The interest at the university was more on the sick children and in the community, as you might expect, it was more in children who were not so sick. In fact, when I was a medical student, I took one week just to see whether I would like it in the community as a community pediatrician taking care of mostly well children. I decided for me, it wasn't right, but I would say most people who are in pediatrics have the opposite view. They'd rather take care of people who are not going to have a terrible disease, but have a disease that's going to get better.

DT: Can you say a bit more about what led you to continue working in cancer research, to take that on as your...?

JK: Yes. Okay. I think what happened was that as my medical school and my residency evolved, I came to realize that I was really interested in research. I was interested in being in a place where people were asking questions about difficult diseases and people were doing new and innovative things, new and exciting things to try to treat these horrible diseases of children. It was just the atmosphere, having people who were very committed to answering scientific questions and medical questions and doing research on those bad diseases. It was kind of an evolutionary process of mine. I think like everyone, I had to learn how to be successful in that kind of environment. It was one thing to go to

really interesting conferences and hear people talk about interesting diseases, but what are you going to do about it? What can you personally contribute to this became a question for me, as it, I think, does for every young person who is getting involved in research.

DT: How did you start developing your research program? Was it based on what you had done for Bob Good?

JK: Yes, to some extent it was. Yes, because what I had done with Robert Good and Carlos Martinez was to look at the role of the thymus and the immune system and, particularly lymphocytes as the key cells that were important in the thymus and the immune system. So I became very interested in those white blood cells called lymphocytes, and, then, it was sort of an extension to become interested in the diseases of those white blood cells. While Robert Good was here, I was interested more in immune deficiency diseases, but as I began my own kind of more independent career, I evolved more into the study of abnormal white blood cells that produce leukemia. Does that make sense?

DT: Yes.

JK: That's kind of the evolution of things. The leukemias in children were more common, and still are more common, than immune deficiency diseases, which he was very interested in and which are really quite rare; although, very interesting.

DT: I interviewed Paul Quie I think last month. He was interested in neutrophils.

JK: That's right. Paul's an interesting kind of parallel to me. Of course, he's older and more seasoned, more experienced. He became interested in the neutrophils and the neutrophil diseases. I, along with a couple of my colleagues... Bill Krivit and Mark Nesbitt were interested in leukemia, childhood leukemia, so that was the evolution of my interests.

DT: In the early years of your career, how did you divide your time between research, clinical practice, and, say, teaching?

JK: I would say there was not very much of formal teaching. It was really more research, both laboratory research and clinical research, taking care of patients and clinical. Those two really took the vast majority of my time, trying to understand some of these diseases and how to treat them. The teaching that we did, at that time, was more informal. When we made rounds, we'd see patients and, then, talk about patients. The teaching was of that sort more than medical student teaching; although, I did a little bit of that with medical students.

DT: What kind of staff did you have or post docs [doctorates] helping you with the research or were you really...?

JK: Well, that's an interesting question, too. At the beginning, I was really kind of a post doc. Towards the end of my residency, I was in this kind of transition to becoming faculty. I really functioned as a post doc; although, it wasn't as formalized as it is now. If you wanted to do something in the laboratory, you kind of went and did it yourself. I remember very early in my career—it was in 1971—I went to the NIH [National Institutes of Health] on a project that Robert Good and I had planned. I went to work at NIH on a project where I did everything myself for three months. Then, when I was here in those early years, I essentially did most of it myself and occasionally would have the help of somebody in the laboratory. One person that was very helpful in the laboratory when I was getting started in the laboratory side of things was Edmund Yunis, who was the head of the Histocompatibility Lab. That was important because as we got started in bone marrow transplantation, histocompatibility of tissue was absolutely critical. So he was really helpful in this. Then, on the clinical research side, I got a lot of help from Mark Nesbit and Bill Krivit, because that was their strength. When I was a young faculty person, I guess I was also kind of a post doc.

[chuckles]

JK: Now students do this laboratory work and go on for years and years in their training. I just went and jumped right in and took a faculty job. Somebody would pay me, so I took the job.

DT: How was your research funded? Were you part of the National Institutes of Health?

JK: Well, that was interesting, too. Getting started, it was not easy to get money. Bob Good and I had some connections at NIH. I told you I went to NIH. I went to work in the laboratory of George Todaro, who was one of the people who was the discoverer of the oncogene. Subsequently, he's retired from science. He was part of a program called the Special Virus Program that was committed to finding the viruses that cause human cancer. We were all pretty naïve back then, and everyone thought that as in mice most of cancers were going to be caused by a virus. Anyway, we were able through my connection, my being at the NIH, to get a contract with the NCI [National Cancer Institute] in the Special Virus Program. My first laboratory research support was through them. It was a contract. That's what got the laboratory going.

Then, after a few years, I got my first real NIH, National Cancer Institute, grant for laboratory studies. By that point, by 1972, we were doing some pretty exciting things with defining the kinds of lymphocytes that become leukemia. We published a paper in the journal *Science* in 1973 describing that a fair percentage of childhood leukemias have origin in thymus-derived lymphocytes that I studied back when I was a student, and we were able to get funding for those studies.

DT: I think this will be tricky to answer. I'm curious if you can explain a little bit what that specific relationship was between kind of laboratory research and clinical research, like when you were moving between the two if they were...

JK: Yes, that's a good question. In those days, the boundaries between clinical research and laboratory research were not that well defined. In particular, laboratory techniques were relatively simple. For example, this first paper we published in *Science* just showed that these particular leukemias when you stained them with antibodies and look at them in the microscope were derived from the thymus. By today's standard, that's pretty simple stuff. The clinical research was also relatively straightforward. For example, the first clinical research that I was involved in was working with Good and some of these immune-deficient patients and children with leukemia with Bill Krivit and Mark Nesbit. We would take a single child and try to treat him with whatever we had available and, if they responded, we'd get really excited about that and publish a paper on that. Well, nowadays, of course, things are much more sophisticated, much more complicated. In order to do good clinical research nowadays, you have to be pretty sophisticated about statistics and trial design and all this kind of thing, and you have to have dozens or often hundreds or, sometimes, thousands of patients in studies. So it was just simpler in those days that you could theoretically do both clinical and laboratory research, and a lot of us did. I think Paul Quie was another good example of somebody who did that. Now, it's much harder and young people who are getting started kind of have to make a decision...are they're going to do laboratory research or clinical research?

DT: Those lab studies were always done using mice?

JK: No, not always using mice. Some were done using mice. But some were just done taking the cells from a patient who had either immune deficiency or leukemia and studying them by various means in the laboratory.

DT: It's interesting that you talk about the way in which clinical research changed later on in your career.

JK: Yes.

DT: In the 1960s and 1970s, there's a lot of change within clinical research even then, especially in the kind of parameters and structures for clinical research and government regulations.

JK: Yes.

DT: I guess Informed Consent Regulations were instituted or changed in 1967 and, then again, in the early 1970s with the requirements to go through institutional review boards. I wonder if you could talk a bit about...

JK: Well, in the early days, we were not very sophisticated about this.

I'll give you a good example of something that happened. That would have been in 1973. We had a patient who had aplastic anemia, which is a bone marrow failure disease. It was actually interesting. Bill Krivit and I took care of this patient. This was a boy who had aplastic anemia. His sister was the donor. That was the first bone marrow transplant

that we did for that disease. The sister donated the bone marrow, and, then, the bone marrow failed. The donor was then nine years old. We wanted to do another transplant using her as a donor, because she was the only one that was a match. She said, “No, I don’t want to do that, because I’ve done it once. It hurts. I’m not going to do it again.” Her parents said, “You’re going to do it.” We had a *tremendous* problem in terms of getting experts to help us decide: does she have the right to refuse at that age or not or does she have to do what her parents tell her? It brought to light the difficulty of obtaining informed consent properly. We really struggled with that. The sad outcome of it was that she donated for the transplant again and the boy died in spite of it.

But we didn’t have mechanisms for informed consent. We struggled with mechanisms for whom to talk with to deal with these kinds of informed consent issues. It became very apparent that we needed to have processes for informed consent where people could really approve, understand what they’re going to go through and approve or disapprove.

DK: It would seem to be particularly complicated when you’re dealing with pediatrics.

JK: Yes. It’s far more complicated with pediatrics. But the issue with informed consent now is... We did a lot of things without having today’s standards of informed consent. We didn’t deal very well with any privacy issues at all. We seldom talked about privacy. Everything was public about what we did. There wasn’t much confidentiality about anything.

DT: It sounds like from what you say about that example is that at some level that need for federal informed consent provisions came directly from the work that you were doing in the clinic as opposed to being imposed by government necessarily.

JK: Yes, the government being involved in informed consent really came about from some pretty egregious abuses of patients in trials. For example, there’s the Tuskegee thing [Syphilis Study]. There are a lot of examples of people being used for research without ever being told what they were doing. It was probably necessary. These kinds of regulations tend to develop because of abuses on the part of people.

DT: Yes.

JK: That was to be expected, I think.

DT: But even without those abuses or anything, just the complexity and the ethical dilemmas raised by clinical research....

JK: Yes, I think that’s the other thing. The other word I was going to use was ethics. We began to understand more about the ethics of clinical research.

DT: My sense from other people I’ve spoken to who went to medical school the same time that you did is that there really wasn’t much or any teaching about ethics within the curriculum.

JK: No.

[chuckles]

DT: It was just something that you were supposed to develop as you...

JK: The best thing that can be said is that some of the time, we had good common sense.

[laughter]

JK: I don't think the word ethics was used very much.

DT: Now when you might think of cancer research, it seems like it would be very multidisciplinary. Was that case in the 1970s also?

JK: That's a good point, too. It was less multidisciplinary then, because we didn't really know very much about what to do. In the early days of human cancer research, the only thing that was really available was surgery. So, in the early days of the twentieth century, surgery was all that there was. Then, as Aimee [Slaughter, Doctor Kersey's research assistant] and I worked through a project that we did about the 1920s and 1930s, it became evident to us—no surprise—that radiation therapy became available, the development of radium, the development of X-ray as a new therapy. Then, all of sudden, people had to begin to think about being multidisciplinary in our treatment.

Then, the next really big thing that came in cancer treatment was the development of chemotherapy. That was interestingly initiated primarily in Boston by Sidney Farber and colleagues about sixty years ago with chemotherapy of childhood leukemia. So chemotherapy was the next thing that came along. That did not involve surgeons, nor radiation therapists. So that really started a whole new era in terms of treatment...chemotherapy.

Then parallel with that was the development of more basic research into etiology. This is where the virus etiology comes back that I mentioned to you. In the 1970s, we and other people were interested in virus etiology. It goes back earlier; the first basic cancer researcher who was hired here at the University was a fellow by the name of John Bittner who was hired in the 1940s to develop the breast cancer research program, which was based in mice. He had discovered that the milk of mouse mothers was able to transmit breast cancer. It was the discovery of the so-called mammary tumor virus and he was one of major discoverers of this. But at that point, had no real clinical implication. In fact, it turned out that although that was true in mice, it wasn't true in humans. But that was kind of the beginning of basic cancer research.

DT: You talked about, obviously, the interactions or collaboration or competition between surgeons, radiologists, and I guess what became oncologists.

JK: Yes, right, exactly.

DT: Then, also, I wondered what the state was between organ specific specialists. Maybe this is less so for peds, but OB-GYNs [obstetrics and gynecology] and, say, ENT [ear, nose, and throat] specialists...

JK: Yes, the idea that certain of the specialists became more and more interested in their particular kind of cancer was evolving. GYN oncology was a good example, where the gynecologic cancer, uterine cancer, ovarian cancer, and so on became of great interest to the people in the Obstetrics and Gynecology Department. So they developed a strong program where they felt that they could do the surgery, because they were primarily surgeons, but, they would also do the chemotherapy. That happened a couple other places, but I would say most of the chemotherapy in the 1960s and 1970s evolved more under the umbrella of medical oncology for adults or pediatric oncology for children and was not organ specific. In other words, there was some of both.

DT: The development of medical and pediatric oncology... Did that emerge or develop because of the developments in chemotherapy?

JK: Yes. Yes, basically it was the developments in chemotherapy with these new drugs becoming available that made it possible to develop that new field. People like B.J. Kennedy here became very interested in medical oncology and people like Bill Krivit who was really the first one in pediatric oncology, became interested in chemotherapy of childhood cancers. Yes, it was really that the field of oncology as a specialty evolved parallel with the development of chemotherapy. Of course now, chemotherapy has become so widespread that it involves every organ site.

DT: Can we talk about the bone marrow transplantation program?

JK: Yes. I was just going to say that. We'll transition from what we said into what happened next. Bone marrow transplantation was really a great example of combining multiple forms of therapy. Setting aside immune deficiency diseases that Robert Good studied where one need only replace some cells that are defective and aren't present in those babies... Setting that aside for a minute, the major area was bone marrow transplantation for malignant diseases, particularly leukemia. By the early 1970s had available radiation therapy and we, for example here, pioneered the use of the linear accelerator, which was a new kind of development in terms of how you could develop total body radiation which was combined with multi-agent chemotherapy, and cells from a donor that could have an immune response against the cancer that made this whole thing come together. Now you had all these modalities that we hadn't in the past. Surgery played almost no role, but certainly, chemotherapy and radiation were absolutely necessary as well as getting the donor of bone marrow. The thing that was unique about the donor of bone marrow was that it produced what we now call graft-versus-tumor or graft-versus-leukemia reaction where there's an immune reaction of the donor cells against the residual cells in the patient that has leukemia. This was really in many ways a paradigm for a multidisciplinary approach to dealing with cancer.

DT: Bone marrow transplantation was pioneered here?

JK: Basically, the story goes back to animal studies that were done back in the 1940s—at a time of concern about nuclear weapons and nuclear accidents—and the idea that if you irradiated an animal, they would die of a radiation disease, but you could rescue them from radiation sickness by getting bone marrow. People said, “Since that could happen in animals, could it happen in humans?” In the mid 1960s, French researchers treated people who had been involved in radiation accidents while working in the nuclear industry. They gave bone marrow to the people that had received radiation from a radiation accident. It turned out those patients all died because they didn’t know how to match the bone marrow and they died of complications. What was evolving at the same time was the idea that you could match the donor and recipient and that that was really critical. The donor and recipient had to be matched in the system called the HLA [human leukocyte antigen] system, the histocompatibility system.

It was really bringing knowledge of the HLA system that made the first successful bone marrow transplant possible here in the child that had genetically defined immune deficiency. That didn’t involve any radiation with that patient, because of his genetic disease. He basically had a bone marrow failure. So there it was relatively simple. You didn’t have to worry about all of the outside influences of radiation either from a radiation accident or because of some other reason. Basically, one way to look at it is that these infants just had an empty slate, basically an empty bone marrow, so the idea was just to put bone marrow in from a matched sibling. That made this transplant of this patient, David Camp, by Robert Good in 1968, a relatively straightforward issue. It was the first successful transplant. It was done under better conditions than those previous radiation accident cases where the marrow wasn’t matched and the patients had radiation induced disease from nuclear accidents.

Here in Minnesota and the group in Seattle [Fred Hutchinson Cancer Research Center, Seattle, Washington] were also very interested in the transplantation of leukemia and lymphoma. Now, we thought we could give the radiation under controlled circumstances combining it with the matched bone marrow, because we then had the matching available. Then in 1975, we did this first successful bone marrow transplant for lymphoma. The patient is now the world’s longest lymphoma transplant survivor. Our colleagues in Seattle were doing similar kinds of things with leukemia patients and having some long term survivors.

It was a very exciting time. The development of this bone marrow transplant approach, particularly for malignant disease, of necessity required multidisciplinary approaches, because you had to have experts in radiation, chemotherapy, and immunology. You had to have radiation therapists who were really expert. Doctors Seymour Levitt and [Tae Kim—Seymour Levitt was head of the radiation therapy—were developing the linear accelerator approach for delivering total body radiation. That was completely novel. Then, those of us who were more on the chemotherapy or immune therapy side were working on the matching of the donor recipient and, then, finding the right combination

of chemotherapy. So, it required immunologists, radiation therapists, and oncologists to make it all work. It became kind of a paradigm for multidisciplinary research, which evolved into the [Masonic] Cancer Center and for the Cancer Center to be successful. Other people, other places, had found that multidisciplinary people, multidisciplinary approaches were necessary in bone marrow transplants. That's kind of how it happened here—and elsewhere.

DT: This might seem like an obvious question, but why the focus on the pediatric population for bone marrow transplantation?

JK: Oh, yes, that's a good question, too. It turned out that at Minnesota, the people that were interested in transplants, Robert Good, myself, and Bill Krivit, were pediatricians. That was one thing. The first transplant, David Camp, was, obviously, a pediatric patient. The other thing which was beginning to be understood at that time, which is even more evident today, is that children are much better at tolerating total body radiation and the chemotherapy and some of the complications of bone marrow transplant than adults. We now know, that from the body's point of view—I hate to say this—by the time you're eighteen, your body begins to deteriorate...

[chuckles]

JK: ...in terms of its ability to tolerate these treatments. It turned out—although, we didn't really know that at the time—children are just better able to tolerate treatments. Of course, the first successful chemotherapy, which was in leukemia, from the 1960s, the Sidney Farber thing, those were children with leukemia, too. The interest nationally and locally was primarily in children. In contrast, the group in Seattle, Don [E. Donnall] Thomas and his colleagues, were mostly focused in adults. It was just that he was an internist and we were pediatricians here. [chuckles] It worked out well in retrospect, because the children had better outcomes.

DT: Obviously, the National Cancer Institute [NCI] was a funder of this research. Were there other, say, children's oriented foundations like the March of Dimes who were interested in this?

JK: That's interesting, too. It turned out that the original work that Bob Good did with immune deficiency was funded by the March of Dimes. The March of Dimes didn't have any interest in cancer, so we began applying for grants from the National Cancer Institute. That was another thing about this that was multidisciplinary. Bill Krivit and I were principal investigators on a grant—transplant oriented—which was funded by the National Cancer Institute. Yes, the NCI became the major funder and we had grants that were several million dollars a year, which, at that time, was almost unheard of. We were ahead of the time and people were very interested in what we were doing. So the NCI became the major funder. The other group that was important locally here and still is—this came much later in the 1980s—a Children's Cancer Research Fund [CCRF] was developed here, and they became a local sponsor. Thus the National Cancer Institute at

the national level and the CCRF locally who have been the two most important funders of the research.

DT: When you first successfully treated David Stahl, the first lymphoma patient, was it right afterwards that program in bone marrow transplantation was established or did it come...

JK: It evolved slowly.

[chuckles]

JK: Of course, one of the interesting things, which Emily [Hagens] and I have been talking about recently, is the fact that we always called ourselves a bone marrow transplant [BMT] program but other people weren't sure what we should be called.

[chuckles]

JK: The BMT program certainly never reached the point of ever being a department or had any administrative recognition by the Medical School or the departments as being an administrative unit. That was a major challenge in the 1970s and 1980s. We always said that we—by that time, I'm talking mostly about myself, and Norma Ramsey, who became the lead pediatric clinical faculty, and Phil [Philip B.] McGlave in Medicine, the three of us—would go around to the departments with our tin cup and to the dean asking for money...

DT: [chuckles]

JK: ...to do what we wanted to do in terms of our program development. It probably never should have been a department but we needed administrative organization and support. But I also said, "Why don't we do this in a way that is more accepted nationally, and, that is have this part of the foundation of the developing of a cancer center?" Cancer centers, by the 1970s, were becoming recognized as administrative units by hospitals or and medical schools, so that kind of made sense. By then, the program and the research had grown to the point where the deans and department chairs recognized that it was profitable and, more importantly, scientifically successful. In the early stages, yes, we called it a program. [chuckles] Of course, some said these are a bunch of people who are trying to do their research and maybe it will work; maybe it won't.

DT: As director, who did you report to then?

JK: As director of what?

DT: The BMT program.

JK: We reported to everybody. Whoever. We reported to the department chairs. Norma Ramsey, Phil McGlave and I would meet with the hospital director and meet with the

department chairs and meet with anybody who would listen to us trying to get money. What we wanted to do was to hire more people and continue to develop this and that takes money. It takes investment. As I say, it was kind of the tin cup approach that evolved. It was interesting. Finally, I think it was about 1995—by then I had been spending a lot of time thinking about the BMT program in the context of cancer center development. There was some recognition of the need for BMT program support. By then, the head of the Academic Health Center, Bill [William] Brody, thought maybe the BMT program should have some kind of administrative recognition. But it never came about, except at the hospital level—and never will. It's always... I don't think it's ever going to happen.

DT: You talked about the multidisciplinary of BMT: I'm wondering: what about the nurses who were involved?

JK: Oh, they were absolutely essential. Yes, that's really a critical part of this. These patients that underwent BMT, particularly the patients that had malignant disease, those that had leukemia, lymphoma had a bad disease to start with. Then, we would give them total body radiation. We'd give them chemotherapy, really intense, which would completely knock out their bone marrow. They'd have no white blood cells. So they were *extremely* susceptible to infection. We had, in the early days in the 1970s, special sterile units where the patients would receive antibiotics on their skin and their body. Nurses and everybody who saw them would have to wear sterile gowns, gloves, and masks. It was *really* intensive nursing. We had just a fantastic group of nurses. In fact, two of the nurses that were absolutely essential to the program's success...one was Joanne Howard whom I mentioned, who was Dean Howard's daughter-in-law, and the other was Sharon Roell. They, really, from an organizational point of view, were *tremendously* important in terms of this. And it wouldn't have been possible if we didn't have really good strong support from nursing, because these patients were so sick. These nurses worked so hard getting these patients through this really critical period when their blood counts were so low and such. We never would have been able to do it; so, as nurses, they were great and as organizers, they were really great. Then, we actually developed a whole team of nurse coordinators who were key to this from the very beginning.

It would have been easy for people to say, "Well, those poor kids. They're sick kids and you're making them even sicker. Why are you doing this? You're just torturing them," and so on. It required people who had some foresight in saying, "We're really going to find out whether this works or not." Now, BMT has grown to the point where after the children, we expanded into adults in the late 1970s, as I mentioned. Adults were a little harder, and there was less interest amongst the adult physicians. Phil McGlave was really the first one who was interested, so he came on board in 1979 and led adult transplants. I think having proved that BMT transplants worked in kids was really critical to this whole program.

DT: The nurses who worked with you, did they develop specific techniques or nursing protocols?

JK: Oh, yes. Yes, yes, they did. They developed nursing protocols. Can you imagine what it would be like if every time you go in to see a patient you had to put on sterile boots, sterile gown, sterile mask, sterile head cover? The patient was sterile because they'd received skin antibiotics and IV [intravenous] antibiotics. Then, just being with kids and...uhhh, their feces, all...just...I'm just surprised they didn't all quit and go find easier work. [chuckles] It was really tough. After a couple of years, we realized that we could get by without so much intensive care, so it became less complicated.

We started out here in the Heart Hospital. The first transplants were done on what we used to call Station 301 in the heart hospital. It was pretty tough. I remember one day, three patients died. Three kids died in one day of complications of the transplant while they were still in the hospital.

The other problem in transplant was that when you do these bone marrow transplants or any stem cell transplant, not only do you have this beneficial graft-versus-tumor or graft-versus-leukemia effects, but you'd have the potential of the donor cells actually reacting as normal cells in the body, the so-called graft-versus-host disease [GVHD]. GVHD is still a big problem in transplants. We still have people who die from GVHD, but not nearly as many. We have much better drugs to treat it. That was another thing. It caused horrible skin rashes and diarrhea and liver disease, and a lot of morbidity and mortality. So just caring for those patients was a big problem.

DT: That's even a problem when you got matched...?

JK: Even when you have HLA matched bone marrow. It's definitely related to degree of matching, I mentioned. It's a little bit less now. Currently we use umbilical blood for transplants to reduce GVHD.

DT: You mentioned there was reluctance on the part of some adult and pediatric oncologists or physicians to do the BMT. Why were they so reluctant?

JK: Because they had other treatments that they thought were better... In the Department of Medicine, there was a Division of Hematology and a Division of Oncology. B.J. Kennedy was the head of Medical Oncology and Harry Jacob was head of Medical Hematology. Leukemias and lymphomas fit in either one of those groups. But neither Harry Jacob nor B.J. Kennedy had much interest in BMT. They said, "That's not going to help. There's got to be a better way." It was almost like... "Why could you torture these people when they're going to die anyway?" That was of course part of medicine till the mid twentieth century. The idea was that people who had a bad disease would die. All of this was evolving in terms of what we are going to do and not do for people with bad diseases. There was also a lot of reluctance amongst many pediatricians. Some of our colleagues were pretty skeptical about what we were doing by making those kids so sick. One of my very good friends, who took care of adults—his name is Carl Kjellstrand—who treated kidney failure. He used to be so proud of taking people who were going to die from kidney failure and using dialysis and keeping them alive. He said

he couldn't believe that anybody could make people so sick as we did with our treatment. Others had the same view. We definitely made them sicker to get them better and some people didn't believe in it.

DT: How did parents respond? Did you get that same kind of mixed feelings from the parents?

JK: Well, yes, to some extent, but not nearly as much from the parents. Parents, basically, would do anything to keep their children healthy and alive. So parents were really quite good about this, really very, very good. I would say that the kids were even better. Sometimes, the parents would get down, and say, "Oh, Johnny, you're so sick. You're throwing up all the time. You have a fever and you're in pain." The kids would cheer up their parents. The kids were, in many ways, the heroes.

DT: I heard something similar from Ida Martinson when I interviewed her in the summer.

JK: Yes.

DT: She talked about your help setting up the Homecare for the Dying Child Program.

JK: Right.

DT: Given that we're talking about the kids right now, could you say a little bit about that program from your perspective?

JK: That was interesting. Ida Martinson, who was a nurse on the faculty, was a neighbor of mine. When we had children with cancer who we decided we couldn't do any more for, we said, "Typically, they tend to die in the hospital. Could we do it in the home?" Of course, from the patient point of view, whether you're a child or an adult, the worst place you ever want to be is in the hospital and the best place to be is at home so you have your familiar surroundings. From a patient point of view, it's the best place to be. It requires a huge amount of organization to make it really work. Ida and I worked on this and it worked well for a lot of people. To do successful homecare for dying children... the parents have to be really committed to it, because it's a huge, huge burden on them. I still think it's a great idea when the parents want it. The children always want it. They always want it. They don't really want to be in the terrible hospital. But you have to be available night and day to provide. They get fevers and what do you do if they need blood transfusions and all these kinds of things? Some people are very uncomfortable with being with somebody who they know is going to die and some people aren't, so it's quite variable. From a patient point of view, it's clearly the best whether you're a child or adult. I'm glad we did it. I don't know... I didn't talk to Ida recently about it in terms of how she thinks it has gone nationally or internationally. Yes, it's something that has tremendous appeal for obvious reasons, but how many children and how many adults are really getting homecare when they're dying, I don't know.

DT: It sounded from her discussion about it and from your comments earlier that it was really contingent on there being nurses out in the community willing also to be available.

JK: Yes, that's true, nurses also. But it's hard for nurses, too, because they get called in the middle of the night with a really sick child. For them to be alone with the child or the family was tough, too, so the nurses didn't overwhelmingly go for this. It requires people who have a real commitment to the idea and feel very self confident and aren't afraid to see somebody die in front of them, to see their beloved patient or their beloved son or daughter die in front of them, they weren't afraid of that. There are no legal barriers to it. It's just a matter of having people who are willing to provide that kind of care.

DT: Changing directions a little bit... You mentioned a little while ago about kind of your first ideas to get a cancer center established.

JK: Yes.

DT: I saw from some of the archival material that there were efforts to establish Unit D in 1976, the Masonic Cancer Center.

JK: Unit D... [pause] You're talking just about the BMT patients?

DT: No. It wasn't clear. It looked like it was beyond BMT.

JK: You're talking about 1970 in the hospital or in the Masonic Hospital Building? Which?

DT: I think it was in the Masonic Center. I remember Charles McKhann was involved.

JK: Ohhh, okay. When Charles McKhann was here, yes, there was an attempt to build, to do something with cancer patients and, yes, we had some patients in the Masonic Hospital Building. I would say it was never an astounding success. When we moved into the new hospital in 1984 then we developed some well organized units, both pediatric and adult oncologists developed very nice units, and we developed a pediatric BMT unit and an adult BMT unit. Those have I think, done quite well.

The 1970s were kind of a turbulent time in terms of organization of oncology. There was a fair amount of squabbling, primarily between the surgeons and the medical oncologists. Charlie McKhann was a good guy, who was heading surgical oncology. B.J. Kennedy was a good guy, who was heading medical oncology. There was a lot of confusion as to who was in charge. There was a cancer committee that was appointed by the dean and led by Charles McKhann. You've looked at some of that?

DT: The Cancer Coordinating Committee?

JK: The Cancer Coordinating Committee stuff. [chuckles] When [President Richard] Nixon decided that we were to have the war on cancer, then the NCI, the National Cancer

Institute, decided there was going to be a program for cancer centers, and everyone got pretty excited about the possibility of doing it here. But in order to make this happen, it was clear that the institution had to be organized. This is when the cancer coordinating committee was active. There are the minutes of one infamous meeting, which I think Emily has seen, where the one thing they decided was that whoever was going to be the cancer center director had to be a “weak cancer center director.” [chuckles] They did not want anybody who was going to be a strong director. That kind of doomed any cancer center planning, at that point, for ten year or so, fifteen years. Charles McKhann ended up leaving here about 1979, I think. He went to Yale [University]. There were many growing pains. The 1970s were growing years and people were trying to establish their turf.

DT: So you think that was largely kind of issues of personality and kind of staking turf?

JK: Do you mean the failure to develop the cancer center program?

DT: Yes, and wanting to have a weak director.

JK: Yes, yes. I think that was the general consensus that there was not going to be any agreement as to how to make a cancer center really work—which was, I think, too bad.

DT: It seems to be if you’re multidisciplinary, like cancer treatment is and cancer research, that it raises that problem more so than something that’s much more focused within a single discipline.

JK: Oh, yes. Also there was and there still is and always will be some tension in terms of how a particular cancer is best treated. Just take leukemia for example... The fact that some of us thought that for really bad leukemias, a bone marrow transplant was a good treatment and some of our colleagues, really respected colleagues, nationally and locally, said, “No.” They didn’t think that was a good treatment. They had a potentially better treatment. So I think these discussions will always go on. That’s where it’s *really* critical to develop multidisciplinary clinical research programs. The institution must be really well organized, and there should not be the possibility that Doctor X will recommend this and Doctor Y will recommend that. We also need the ability to ideally study A versus B treatments.

[break in the interview]

JK: So we sometimes have issues where we have different treatment options for a patient with a particular cancer and depending on who the doctor is that patient sees, he’ll recommend one or the other. Often there’s no reason to think that X versus Y is better. That’s just an unfortunate reality. Prostate cancer is probably the *worst* example of this is not just a local problem but a national problem. If the person they go to see is a surgeon, the surgeon is likely to recommend surgery. If they go to a radiation therapist, the radiation therapist will recommend radiation therapy. What’s really needed are studies to find out which is better, but it hasn’t happened locally; it hasn’t happened nationally.

Actually, it's done better in Europe. That's an unfortunate reality that existed in earlier days and still exists today to some extent. But I think we're getting better. [chuckles] We're getting better.

DT: It sounds like there is no physician who has kind of over all authority of the patient, that it's always going to be hard. I know the position of hospitalist has emerged in recent years.

JK: Yes.

DT: Is that the kind of scenario where a hospitalist would be coordinating the patient's care?

JK: Well, maybe, but I don't think so. What one needs is a team at the administrative level where all the doctors who are interested in, let's say, prostate cancer at the University of Minnesota, where all sit down in a room and say, "Okay, we don't know whether X or Y is the better treatment. We're going to study it." Hospitalists, unfortunately, just work in a hospital and outpatient doctors work in the out patient, but the reality is cancer treatment is, probably, nowadays, more outpatient than hospital based. So I don't think hospitalists are the answer to this. It's really the expert physicians. We now have really expert physicians in each of the major cancers. In breast cancer...fantastic people and G.I. [gastrointestinal] cancer. In every cancer there is, we have people who are experts. Those are the physicians and staff who need to make these plans and work together. Then we can be better.

DT: What happened to the BMT program then through the 1970s and 1980s?

JK: The BMT program began development and it still continues to grow. It just expanded and expanded and more and more people became involved. In Internal Medicine, for example, Phil McGlave was the first one who became interested and, now, Dan [Daniel] Weisdorf is the one who was working with Phil, but he's now the one who is primarily responsible for it. In Pediatrics, there was me and Bill Krivit, and then, Norma Ramsey. Subsequently she has retired and now John Wagner, Paul Orchard, and Mike [Michael] Verneris. There's now a very large group of BMT doctors in both Medicine and Pediatrics. The number of patients continues to grow. There are now 5,000 patients who have had transplants here. It's continuing to grow and get better, doing more and better organized treatments, and continuing to study the role of transplantation.

I'll give you an example going back to my own interest, which is childhood leukemia. We started transplants for childhood acute leukemia back in the 1970s. There were probably only ten percent of the patients that were cured with chemotherapy. Now, locally and nationally, over eighty percent of the children with cancer are cured and the vast majority of them are cured by chemotherapy without ever having a transplant. So non-transplant things are continuing to get better and that's good, because the morbidity and mortality from transplants is still too high.

Another form of leukemia that occurs mostly in adults called chronic myelogenous leukemia or CML. When we started in the late 1970s, the only curative treatment was a transplant and many patients died from the complications of the treatment. First, there was one drug called Gleevec, which you probably know about, which was a pill that was discovered by Brian Druker with the understanding of the cells had produced this leukemia. So people were able to take a pill without even going in the hospital. That's now been around for a dozen years and patients are not cured of their disease, but they're leading normal lives. They're not having all the complications of a transplant. So now the first line of treatment for CML is not a transplant, but it's a drug, Gleevec and, now, there are a couple of new derivatives of that. That's where we need to move. A transplant is not the end-all-be-all for these diseases.

That's why in my own research work, I'm working in the laboratory now. We're working with new compounds as new potential drugs.

DT: Do you think BMT will become obsolete ever?

JK: No, I don't think it will become obsolete, but it already is becoming more selective. My hope is that what will happen is that it will become safer and for genetic diseases to become more effective. Obviously, genetic disease you can't treat by chemotherapy. Like the first child that had a transplant for the immune deficiency disease, those diseases are genetic diseases and there's not going to be any simple drug for this. People have thought that we're just going to put genes into cells for gene therapy, but that's turned out to be *extraordinarily* complicated and still isn't very effective. Cellular replacement therapy for diseases where there's a deficiency of normal cells probably has the longest chance of being effective. My hope is that in leukemias, we'll find more of the Gleevecs because that would be a lot better—but we're not there yet. We're a long ways from that. The number of patients just keeps growing and growing and more and more of the patients who come for bone marrow transplants are ones that first get chemotherapy and when that fails and they come here to get a transplant. We used to get patients that would come here very early on because there wasn't anything else available.

DT: Have you seen among the BMT patients here...? I know that for sickle cell anemia and thalassemia patients that BMT was something being tried.

JK: Being tried, yes.

DT: That was in the 1980s?

JK: Yes. That's a good example. Thalassemia and sickle cell disease are genetic diseases that you know in theory you can correct by providing new stem cells, because those are diseases of the red blood cells. The red blood cells, like all the other cells in the blood cell system come from bone marrow stem cells. So, in theory, everybody with sickle cell disease or thalassemia or any other severe disease can be cured. You just put in new stem cells, right? Simple. Well, but the problem is that there still are too many

complications of the transplant, like graft-versus-host disease. Sickle cell disease, unfortunately, still continues to be a problem and a transplant has never become the standard of care just because the morbidity is too high. The side effects are too high with the transplant. Too bad, especially since you know that putting in normal stem cells would do the trick. It's various other cells that form out of the stem cells, those lymphocytes, that cause graft versus host disease.

DT: You say that there's been a lot of progress in the field...

JK: Yes.

DT: ...but given that there are still so many intractable diseases, how does that affect you as a researcher and physician?

JK: Intractable diseases?

DT: I suppose the fact that there are things like sickle cell disease and thalassemia, there are still limits to what you can do for some of the leukemias.

JK: Right. Is it frustrating? Is that what you're saying?

[laughter]

DT: Yes. Is it frustrating?

JK: It is frustrating. In fact, it even goes beyond those blood cell diseases. There are a lot of diseases, heart diseases, and there are muscle diseases, and there are liver diseases that in theory can be corrected by stem cells that could come from various sources. There's a whole evolving field called regenerative medicine but there's the problem of the graft-versus-host disease, which is a major problem and, then, there's the problem of, sometimes, the cells being rejected if they come from somebody else.

Let me give you an example. I don't know if you've talked to the surgeons very much about the problems in organ transplantation. One of the things with solid organ transplantation, liver transplantation, heart transplantation, pancreas transplantation, all the solid organ transplants, all of them have multiple potential problems. But one of the potential problems is that the organ is rejected by the body, right? So, it's been very clear from experimental animals for a long time that if you take bone marrow from the same donor, you can change the whole immune system of the patient to that of the donor, so then, the organ won't be rejected and you won't have to worry about organ rejection.

Back in the 1970s, it was already an idea, and John Najarian and David Sutherland and I said, "Well, let's do a transplantation of bone marrow and kidney from the same donor." All right? It made sense. Other people were thinking... It wasn't a brilliant new idea. This is kind of a shaggy dog story. We set up a protocol where there was going to be a kidney transplant and bone marrow from the same donor, but, at that point, everyone felt

it was too risky to use a living donor, so the kidney was going to come from a cadaver. In one particular case, the donor bone marrow and kidney were going to be given to the same recipient. It turned out that the person who was to be the donor, the cadaver, was in Duluth [Minnesota]. So I was going to be the bone marrow guy. We were going to collect bone marrow and a kidney from the same cadaver donor. I flew in this little University airplane on the world's stormiest night. There were thunderstorms and lightning in all directions and flew on this little tiny airplane to Duluth, got to Duluth. There was a surgeon there who was going to take the kidney out of the cadaver and I was going to get the bone marrow. So, of course, the surgeon had no idea what was going on. Surgeons are, sometimes, really very nice and neat, but with a cadaver, they don't really care. There was blood everywhere. [chuckles] The surgeon removed this kidney. Meanwhile, I was trying to remove bone marrow. I, finally, did, but what I was getting, because all the blood had been lost from this cadaver, mostly, and the bone marrow was also pretty empty, so what I was getting was not very much bone marrow. I, finally, got enough of something to bring back to the University.

I flew back in the storm again and gave the kidney and the bone marrow to this recipient. The kidney did fine, but the bone marrow never took, probably because there weren't enough bone marrow cells, because of all the bleeding. To make a long story short, we did a couple more of those and never got evidence that the bone marrow ever took, because we hadn't given enough bone marrow.

Also, there was a problem that unless the patient has received chemotherapy and radiation, they don't have the space in the bone marrow for the new bone marrow to go. Nobody wanted to give that intensive radiation and chemotherapy to somebody who was going to get a kidney transplant. In fact, this idea, a great idea, still hasn't materialized all these years later. In theory, it's a wonderful idea for a way to get organs into people without having to provide any immunosuppression afterward.

DT: Those are really great examples of the practice not matching the theory.

JK: Yes, right.

[laughter]

JK: There are just a lot of tremendous practical problems in terms of doing that kind of clinical research.

DT: My understanding is there were renewed efforts to establish the cancer center in the 1980s.

JK: Basically, the renewed effort began in the late 1980s when Seymour Levitt, who was the head of Radiation Therapy—I mentioned his name before—and I and the dean, who was then David Brown, met. It was at my suggestion and Seymour Levitt's suggestion. We had this long tradition in cancer, with B.J. Kennedy and Charles McKhann, and others. They were really doing good things, and they were nationally recognized for

what they were doing, but they weren't organized. The Surgery Department was very strong. Anyway, Seymour Levitt and I met with the dean and said, "We should have a comprehensive cancer center, because all the top institutions are doing that, and it's a good way to get ourselves better organized and bring people together. So we began to talk about developing a cancer center. David Brown was the dean and he was a good friend of mine. He was very receptive to the idea. Frankly, I had not been involved in any discussion with deans prior to that, so that was really my first involvement. He was organized and was willing to take it on as the dean. We began discussions about 1987, I guess, about using a cancer center as a way to bring people together, get some resources from the NCI (the National Cancer Institute), help bring our BMT program under one roof, and make things more organized. That was really the beginning of the cancer center.

There was a need for a commitment from the institution. At that point, Nils Hasselmo was the president, so we had discussions with Nils Hasselmo. David Brown had even more discussions with Nils Hasselmo about this. Nils Hasselmo said, "Having a cancer center is a great idea, but I don't have any money." [laughter] So, we never gave up. We just pursued this and said, "Okay. Help us raise money."

We were able to work with the Minnesota Medical Foundation. At that point, the president of the Minnesota Medical Foundation was Jim Spicola. He was very interested and the Minnesota Medical Foundation was very interested. It turned out shortly thereafter that Jim Spicola developed pancreatic cancer and shortly thereafter died. Pancreatic cancer has a very bad prognosis, and he died. His friend and colleague was Win Wallin.

Win Wallin, who was, then, just recently retired from Medtronic, was asked by us and Nils Hasselmo to help us raise money. We decided that since the University didn't have any money, and the Legislature wasn't going to give us any money, we would raise \$30 million in philanthropy. It turned out that Wallin was a fantastic person to chair our fundraising committee. We knew that if we were going to have a successful cancer center, we had to have that kind of money, because we needed research space. Out of that—it was \$32 million—we built the Masonic Cancer Research Building. Then, we had some money to hire faculty. No one was really opposed but it was just a matter of finding people that were willing to help. Fortunately, the Masons were a contributor. They contributed \$5½ million for the building. Out of the \$32 million, \$5½ [million] was theirs, and the rest came from a variety of other sources. We were able to raise a lot of money, thanks to Wallin. He was wonderful. We raised a lot of money from industry, from major corporations, as well as some private individuals. That's how it got started.

DT: It sounds like this was something of interest to you and some of your colleagues consistently through the 1970s and 1980s.

JK: Right. Yes.

DT: What about 1987 had changed to kind of make it more compelling?

JK: I think what made it more compelling was the fact that we had more strengths, and, frankly, some frustration that the BMT program wasn't getting any administrative support. There was no University support, so we said, "Let's try another tack." It was pretty clear that when we compared ourselves as an institution in terms of the grant support coming from the National Cancer Institute, compared to some of our peers nationally, comprehensive cancer centers that were being developed and funded by the NCI, they had less funding than we did. It sort of was logical. It was one of those things where the time had arrived for doing something different. As I say, the dean was very receptive. I would say that David Brown was the first dean who really took a major interest in doing something to help coordinate cancer programs in a significant way and was willing to take some risks. As I said, the president said he didn't have any money. We said, "Why don't we go get a legislative special?" The president said, "No, we can't do that." On the other hand, Nils Hasselmo was willing to help us in terms of approving it and helping us get leadership in terms of fundraising. I think the time was just right for it.

DT: I don't know if this was in Emily's notes or if it was from the archives, but there was a letter from Don Peterson to Nils Hasselmo saying that whatever you do, don't ask the State Legislature for money, because the legislators think the Medical School has enough money already and they don't need anymore.

JK: Right. Yes, that's right. Yes, that's true.

[chuckles]

JK: There's another twist to this, too. It turned out that the Department of Surgery's role in all of this has always been kind of interesting. In the 1970s and into the 1980s, the Surgery Department had something they called the Cancer Detection Center. That was a clinic that, basically, was set up so that anybody in Minnesota could come to this clinic once a year and be part of a program to detect cancer. The Surgery Department had a fair amount of money and he had the ability to run this clinic. But the Cancer Detection people also thought that they should try to get money from the Legislature. [chuckles] So, we had some discussions about whether the Cancer Detection Center or our new emerging cancer center was going to get money from the Legislature. Well, it turned out that neither one of us got any money.

[laughter]

JK: The Cancer Detection Center has now disappeared. It's been gone for, probably, ten years. That was competition for the new emerging cancer center.

DT: Was there a sound clinical rationale for having a Cancer Detection Center at that time?

JK: There was. There was. It's interesting. In many ways, it was ahead of its time; in other ways, it wasn't. Everyone has had the idea that the best thing for cancer is to prevent it or diagnose it early. That makes a lot of sense. So the idea of early detection was a good idea as an idea. It's turned out from what was learned from that Cancer Detection Center, as well as some really major national studies since then, is that early cancer detection is really difficult. The major cancers cannot be detected just by doing annual exams. One exception seems to be colon cancer, and for breast cancer, just doing an annual physical exam. It turned out that we are still not a very good at early detection. So it really fell on its own merits. That's kind of what happened. I know they also ran out of money, but that wasn't really the major thing. It was kind of a status thing for people to come and be part of that clinic. People would come away with a good feeling that they didn't have cancer, because they passed the exam, but it wasn't good enough. .

DT: This reminds me, I didn't ask... How was the BMT program funded throughout the 1970s and 1980s?

JK: Oh, it was funded mostly by patient revenues and research grants. It was from the very beginning, from the hospital point of view, quite profitable. So there was always some money in the hospital.

[chuckles]

JK: The BMT program has always been relatively profitable, and still is. The laboratory research was supported by, as I mentioned, the program project grants from the National Cancer Institute. But, we never had any money from the University or from the Legislature or any local source. That's why Norma Ramsey, Phil McGlave and myself went with our tin cups to the hospital and departments.

[chuckles]

DT: The plans to develop the Comprehensive Cancer Center... Were you modeling that on other existing comprehensive cancer centers?

JK: Yes. In fact—we kind of skipped over this quickly—when Nixon pushed the Cancer Act, one of the things that was in that act was the establishment of comprehensive cancer centers and recognition and support for comprehensive cancer centers. So there was already a really well defined model for how you should develop a cancer center. You had to show that you had institutional support. You had to have authority of director. You had to have adequate facilities. You needed to have an adequate research base. So we needed to show in the 1990s then when we first put in our grant to the National Cancer Institute that we had those. But it took all those years from the late 1980s to the early 1990s to get institutional support, as I told you, to raise the \$32 million, and, then, to get the authority of the director.

As far as the director... Originally, my plan was not to be the director of this. I just wanted to get it going. They decided to do a national search, and they asked me if I'd be

a candidate, and they had some candidates come in, and they ended up with me. But, they did, I think, a fair national search for a director. But the director had to have the authority to have space, to assign space, be able to do recruitments and so on. That really required the support of the dean. The model that we were using was a model that was that of a so-called matrix cancer center where the director would not have absolute authority over personnel, but would have shared authority with departments. So that meant that whenever we hired anybody, it had to be jointly with Department X, which meant that the people in Department X would have to agree that they wanted to hire jointly with us. Some of them had varying degrees of interest in that. But, once we had some resources, they became more interested.

[chuckles]

JK: The department chairs became more interested, particularly when we had some space, because we were building the Masonic Cancer Research Building, which was finished in 1996. Once we had some space, then they got more interested, because they could then put there people into this space.

That was all based on a well established national model. So we could say, “This isn’t anything that we invented. This is a national model the NCI established.” The NCI did us a *huge* favor by establishing these cancer center programs in the late 1970s. It’s really almost unique in terms of cancer center relationships to medical schools to have a strong cancer center existing, along with strong departments where, basically, there has to be a shared authority. It can’t just be a cancer center by itself, and, hopefully, Department X is not going to do it by themselves either, because the dean will say, “You need to work with the Cancer Center.” So that’s the model. That national model had been accepted by a number of places.

Now, having said that there were, even back in the 1970s, a couple of cancer centers that were really quite independent of the medical schools and departments: for example, Memorial-Sloan Kettering [Cancer Center] in New York, M.D. Anderson [Cancer Center] in Texas, and Roswell Park [Cancer Institute] in New York. Those are three examples of centers that were really quite independent of a medical school. By the time we applied, there were twenty-some comprehensive cancer centers. Wisconsin had one [University of Wisconsin Carbone Cancer Center], the University of Pennsylvania [Abramson Cancer Center]. So there were plenty of role models for us.

Whenever we were challenged by the dean or challenged by the department chairs, we’d bring in somebody from one of those places to help us. We had a really great scientific advisory committee made up of people from outside. Ron [Ronald] Herberman, who was the director of the cancer center in Pittsburgh [University of Pittsburgh, Hillman Cancer Center], would come and tell the dean and tell the department chairs, “This is what works.” So it worked. There’s nothing like peer pressure from the outside to make things happen.

[chuckles]

DT: Did the Mayo Clinic [Rochester, Minnesota] have a comprehensive cancer center?

JK: Mayo Clinic, also, yes. Yes, also Iowa, Michigan, and Wisconsin have comprehensive cancer centers. There are no cancer centers in the Dakotas or Montana. The University of Washington has a strong center. These examples were really helpful.

DT: With that matrix administrative structure, say the Surgery Department hires someone who works in cancer, oncology, do they automatically approach the Cancer Center to see if there's a joint appointment?

JK: Usually that's the case. Occasionally, there are people who are hired by a department where the physicians are strictly doing patient care, not doing any research, where they will hire somebody. Yes, we make them members of the Cancer Center. The basic scientists, the people that are doing the laboratory science, the people that are doing population studies, epidemiology, anybody that's doing serious research has been jointly appointed.

We have the ability in the Cancer Center to home grants here. We probably have about 650 members of the Cancer Center right now. Of those, there are about 150 who are what we call research members, meaning that they have research grants, many of who, but not all, are homed in the Cancer Center. So there are a lot of people that are Cancer Center members who are social workers or nurses or statisticians. They are members, but they're not research members. We've had resources available to hire people and research space... One of the big pluses of the current arrangement is that we have space. In fact, we're going to have a whole new space in another new cancer and cardiovascular building that's being built next to the Wallin Building. It's going to have a lot of Cancer Center space. Having resources available makes it much more attractive to the departments.

DT: In terms of the physical space then, what's the priority of the Cancer Center? Is it to be a center of clinical research? Does that take place within the Center or are the patients located in the hospital?

JK: The major goals of the Cancer Center are in research, both laboratory research and clinical research, but, as I mentioned, there's a lot of clinical care of patients that doesn't involve research. The care of patients with cancer is primarily the responsibility of the University of Minnesota physicians and the hospital and Fairview [Health Systems]. So the Cancer Center does not obtain patient care revenues from the hospital or by the UM [University of Minnesota] Physicians. That money does not come to the Cancer Center. Now, in a freestanding cancer center, like Memorial-Sloan Kettering, it would, but not in a matrix cancer center like ours. It's really the hospital and the UM Physicians that are responsible for the direct patient care.

What we do support in the Cancer Center is the research protocols that patients are treated with. We help organize them. We help the physicians and the staff organize

these protocols and we provide some financial support for doing clinical research—and that can be quite expensive. Actually, by now, the biggest financial challenge of the Cancer Center is to find enough money to pay for clinical research. [chuckles] So we have to battle a bit with the hospital and the UM Physicians to say, “What we’re doing is helping you, so you help us.” But we don’t have primary control over those dollars.

DT: What are the attitudes of community physicians, private practitioners?

JK: Uhhh...mixed, I would say. To the extent that we’re doing research that doesn’t threaten their bottom line, they love it. [chuckles] To the extent that we’re doing treatments that competes with them there are problems. I guess that’s probably not surprising. Or the extent to which we do treatments that they are not so interested in, they refer to us.

A good example is the BMT program. I think that’s one of the reasons why we’ve been so successful is that most of the physicians in the community think that bone marrow transplant is too complicated for them to do. So they are happy to refer us patients, the sick patients that have leukemia or lymphoma for a transplant. They don’t want to deal with those patients. But in ordinary early stage breast cancer, yes, there’s competition, as well as early stage lung cancer or prostate cancer. The other thing that we offer, which is unique is that physicians most often accept the idea that if the patient has breast cancer and they’ve failed all of the standard therapies, then if we have something to offer, an experimental therapy—experimental therapy is a big part of what we do—then, often, they will refer patients. Often they won’t; often they will. That’s where BMT is a win/win for us and for them, because they don’t have to deal with these people who are quite so sick.

DT: Does the University Cancer Center find itself in competition with the Mayo Clinic Center?

JK: Yes, to some extent, the same issue with the Mayo Clinic, but, of course, the Mayo Clinic doesn’t refer many patients here, except for a transplant, actually. We get quite a few for transplants. Similarly, we don’t refer that many patients to the Mayo Clinic for cancer care. However, there are a lot of people in the community who go for cancer care to the Mayo Clinic. I would say there’s a pretty healthy relationship with the Mayo Clinic. I would say it’s a relatively healthy relationship with the community docs here. I don’t think it’s any worse in cancer than it is any other field. They have to make their living by seeing patients, so they do.

DT: When you were appointed director of the Cancer Center, was your appointment received supportively by the faculty and administration generally?

JK: [laughter] You’ll have to ask them.

[laughter]

JK: You'll have to ask them. I don't know. I always felt that I was getting good support from people, but I'm sure they didn't always support the decision. I always felt good about it, but you'd have to ask them. I think the major challenge I perceived is there will be questions when you hire somebody new. Is their primary loyalty going to be to the Cancer Center or to the Department of Surgery or the Department of Pediatrics or whatever, but that's inevitable. I think that's an inevitable tension. I think the dean did the right thing in terms of the appointment, of going through a national search, doing that so no one would say that he was appointed just because he was my friend. I think that was important. No, I felt the departments and the dean, everybody, were really very accepting of the idea. Of course, it brought in more money, because we obtained the designation by the NCI and that resulted in an additional \$3½ million a year to the institution for the Medical School, some of which went to the departments. Overall, I didn't feel any major opposition, and felt that I was well received by all.

DT: In terms of your administrative authority, you shared, essentially, authority with department heads?

JK: The academic appointments are in departments.

DT: Are they?

JK: So the promotion and tenure is in departments.

DT: Okay.

JK: That's something that won't change, and probably shouldn't change. That was not something that we could really offer people, and the department chairs are pretty possessive, as they should be of that responsibility. Promotion and tenure is complicated, because there are associated financial responsibilities.

Resources that we had to offer included space—that was really attractive to people, a place where people could be—and, then, money and faculty salary support. We had money that we'd give to faculty members, particularly young faculty members, for new ideas, which they can't get from their departments so easily.

[telephone rings – break in the interview]

DT: Within the Medical School then, would you say you were equivalent to a department chair and that you would report to the dean directly?

JK: Uhhh, yes. They might say I wasn't equivalent, of course.

[laughter]

JK: It was really interesting, too, because what was happening at the same time as we were developing the Cancer Center was the development of the Academic Health Center.

DT: Yes.

JK: Remember I said that David Brown was the dean and he was dean at a time when the Academic Health Center was not really very organized. Cherie Perlmutter was a wonderful person, but she didn't have very much authority as a vice president. What happened was when Bill [William] Brody came here and he started the Academic Health Center... Well, I had been the interim chair, or the acting director is the right word, for the Cancer Center up until Brody came. He was the first one to appoint me permanently to be the Cancer Center director. So that was concurrent with the beginning of the Academic Health Center. At the same time, it didn't want to lose ties with the Medical School, so, actually, the way I kind of looked at it was as a dual appointment with the Medical School dean. Then I had a reporting relationship, which was not as formal, to the Medical School dean. So I would meet every month with both separately, with the vice president and with the dean. It worked out fine. I didn't think that was a problem. I often met with other deans as well. The official reporting relationship was to the Academic Health Center.

DT: Okay.

JK: Actually, that brings up something that I think is important. One of the best aspects of the development of the Cancer Center for me personally, and I think this is true of other people as well, is we began to learn what faculty were doing in cancer around the university. For example, we had this wonderful, wonderful College of Pharmacy with medicinal chemists who were developing new drugs. I had not even known who they were. There were people in Public Health, in Epidemiology who were doing *really* outstanding cancer research. I hardly knew who they were. What I would say more than anything else this Cancer Center has done is to bring people together, so you get to know who is doing what, develop new collaborations, new relationships with people you didn't know even existed, even relationships with the Institute of Technology. The Cancer Center has *really* encouraged and fostered those ties with those other schools in a way that was just fantastic. Nanotechnology...I had no idea what nanotechnology was. This is what happens when you get bright minds coming and meeting together and talking about things soon you have collaborations and new programs. That's really what a cancer center is all about more than anything else.

DT: It seems that that fits well within the vision of why the Academic Health Center was established in the first place.

JK: Yes, exactly. That's exactly right. That was why the Academic Health Center was formed and that's what has worked. I don't know what Frank Cerra would say to *you*, but what he said to me was that the Cancer Center was sort of the flagship of the Academic Health Center in terms of this kind of interdisciplinary interactions. It's different than the College of Pharmacy, the Medical School and the School of Public Health. They all have their major, major roles, and they'll always be the ones that will be the most important; but, as far as interaction, I think having this Cancer Center has really

been great for bringing people together from all the different disciplines. So we have research these programs that are just *fantastic* that are made up of people from various departments and various schools. That's been a big plus.

DT: Do you have much interaction with the State Department of Health in terms of cancer epidemiology?

JK: A little bit...a little bit. There's DeAnn Lazovich who is an epidemiologist. She is one who works really closely with the Department of Health and, also, Julie Ross, another epidemiologist who works closely with the Department of Health. We don't have any formal ties, but we have a lot of interactive relationships with them. Of course, they keep statewide records on cancer so that's important.

DT: I'm curious, because right during those years when the Cancer Center was being developed in the early 1990s, there was the controversy around Najarian's ALG [Antilymphocyte Globulin] program.

JK: Yes.

DT: Was ALG used to combat graft versus host disease?

JK: That's a good question. It turned out that the ALG was really designed primarily as an immunosuppressive agent, to primarily destroy so-called B-lymphocytes; although, it was not entirely specific. With graft versus host disease, it's mostly a result of the other major class of lymphocytes called T-lymphocytes, which are the ones that I've been interested in going back to my days in the thymus. So we actually had been working with the ALG lab, Dick [Richard] Condie and John Najarian, making ATG [Antithymocyte Globulin] rather than ALG. We were actually using ATG, which they made. We were involved not in the ALG side of it but the ATG side of it. ATG was never something that was a big deal for the surgeons and was kind of a side project for them. It never became a big thing for us. ATG was also commercially available, so we didn't have any problem making a transition when they stopped making ALG and ATG. We were never really much involved with any of that controversy, because we weren't using ALG in our patients.

DT: I recall that there were sanctions by the FDA [Food and Drug Administration] and the NIH on some of the research being done at the University.

JK: Yes.

DT: Did that impact the Cancer Center?

JK: No, it didn't, not really. No, it really didn't. We were able to switch to using, as I said, commercially available ATG. It was a lot cheaper to get it from them than it was to buy it commercially; but no, we managed to stay clear of that. [chuckles]

DT: What is your sense of the reactions among the faculty to the controversy as it developed?

JK: Well, I'll tell you my frank view of this. Having been involved with Dick Condie, who was the one who was running the ALG operation, and working with them as they were making the ATG, it was kind of a loose operation. It wasn't very well managed. I think shortcuts were taken. There were not the kinds of clinical trials...real clinical trials. Those of us in cancer believed that if you really want to study something, you need to do a real randomized trial where you'd compare ALG with X in order to find out which is better, and they weren't really doing that. The assumption was it was good and so we're going to keep using it. So I think it was a little bit loose. And they were selling it at pretty high prices so there was a lot of money involved. I don't think there was anything dishonest, but it wasn't up to the FDA standard and the FDA said, "It's not up to our standard."

DT: Were there other versions of ALG being made elsewhere in the country?

JK: Yes, there were. There were other people making ALG. As I said, we didn't really use ALG so much, so I can't give you details as to who was actually making it. But I know there was...

There's always been concern about any product that's given to patients. It really needs to be well tested in terms of safety and in terms of efficacy. The standards probably weren't very well kept.

DT: That raises another question I have just in general. It kind of returns to our question earlier when we were talking about some of the ethical difficulties around informed consent. Within research cancer treatment, because there are often situations where there isn't necessarily a really effective treatment, it seems you have the potential where you have a lot of patients taking experimental treatments. I wonder as a researcher and as a physician, what challenges do you confront when you are, on the one hand, trying to give the best treatment you can to patients, but, at the same time also, having obligations to conduct clinical trials to proper parameters.

JK: That's a good question. Let me tell you about something that happened, although not with us. Seattle was the other center that had done lots and lots of transplants, although, in fact, Don Thomas in Seattle got the Nobel Prize for the bone marrow transplant field. We and the Seattle group were very interested in the idea that we could use this method to get rid of the T-lymphocytes that caused graft versus host disease. That was based on some studies that we had done in mice that showed you could get rid of the T-lymphocytes with antibodies. Bone marrow contains T-lymphocytes and contains stem cells and with antibody, you can kill the T-lymphocytes. Then, you can give the bone marrow to the patient—or to the mice. And you can prevent graft versus host disease. It works beautifully. We were engaged in some studies of this and our colleagues in Seattle were also, now on a patient level, engaged in doing these studies. They had a protocol and I think it was twenty patients on the protocol and most of the patients died. Most of them died because the bone marrow didn't work because it turns

out the T-lymphocytes are required to get the bone marrow to work. So it's tricky business.

Anyway, there was a major lawsuit against the Seattle group by families of patients who had died who said that the patients had not had adequate informed consent for this method of treating bone marrow. I spent quite a bit of time as an expert witness trying to work with the Seattle people in terms of this major lawsuit. The lawsuit was a huge deal and it was in the Seattle newspapers. This well recognized, well established bone marrow transplant center was accused of doing unethical things without proper informed consent. It turned out that after I spent a huge amount of time as an expert witness at the trial in Seattle, the lawsuit was, in the end, dropped, because the jury was convinced that it wasn't really known at that time that the graft failure was a big risk, and so they were not negligent, which I think was the right decision.

However, I learned in that their informed consent procedures, as ours were at that time—this was in the early 1980s—were really not optimal. They were not doing an adequate job of informing people of what all the risks were, even though some weren't known. It illustrates how careful you have to be about informed consent and getting proper informed consent. The kind of forms that we used back then, and they used back then, wouldn't fly today, because they weren't nearly as complete. The risks weren't as well spelled out as they should have been and the uncertainties weren't spelled out. I think it's always a problem to make sure that people have all the informed consent that they need for a particular experimental procedure. It takes a *lot* of time to explain to people what the risks and potential benefits area of a particular new treatment. People are busy and take shortcuts. But it's something we need to be more and more vigilant about. Now we're much better. Again, we can't do things the way we used to do them.

DT: It's the physician who is responsible rather than, say, the nurse for communicating these things?

JK: The physician is responsible. Absolutely. The piece of paper is signed by the patient and then signed by the physician that says, "I explained all of this..." blah, blah, blah.

DT: Can it be a resident or does it have to be ...?

JK: There's no law that says who it has to be, but here and, I think, everywhere, it is a staff physician who gets that informed consent. It's an area where things can slip real easily if you're not really vigilant about it and really spend time explaining. Of course, it's sometimes difficult because some people don't want to hear. At the time, they don't want to really hear at the time what the downside of it is or, occasionally, they don't really understand. But a lot of people just don't really want to hear it. They say, "Whatever you want to do." Well, quickly, people can change their mind if things go sour and say, "Nobody ever told me. Yes, that's true. Nobody ever told me."

DT: Something that's implied within informed consent is that the patient has heard and received the information.

JK: Yes.

DT: And there's doesn't seem any good way of measuring that receptivity?

JK: That's right. Absolutely. You can imagine if you're a patient that has a serious illness and somebody comes at you with this. They're telling you all these things going on with your son or your daughter or your mother or your father. Then, they come at you with some new something or other, and, then, they say, "Well, these are the hundred things that can happen to you." Patients often don't want to hear all that.

DT: Do you have a situation where you have patients or their families demanding access to experimental treatment as opposed to you presenting it to them, because they've heard about some treatment?

JK: Yes, people will ask. Seldom to they demand. A more common situation is you don't have anything to offer. That's a sad reality.

The other sad reality is that there's a lot of misinformation in the community, that some unproven therapy is good, that prune juice can cure cancer, and people are absolutely convinced of that.

Are you thinking of some particular example?

DT: No. Well, not specifically to cancer. I was just thinking in the early history of HIV-AIDS [Human Immunodeficiency Virus- Acquired Immune Deficiency Syndrome], that patients were very vocal about demanding getting access to experimental treatments before they had been FDA approved.

JK: Oh, I see. The point at which you say that something is proven therapy versus experimental is sometimes hard to define.

DT: Yes.

JK: That's a real challenge, because one of the things in clinical research that happens all the time is if you offer patients an experimental treatment, let's say, standard therapy versus standard therapy plus X, they'll say, "Why can't I have standard therapy plus X?" Superficially, that sounds like a great thing. If you've got something standard and you have something that's going to be better, but what people need to be told is that adding plus X might it worse. You might have a worse outcome. People will say, "Can't I just have that new whatever it is?" Most of the plus Xs are things that are not available anywhere else, so we don't really allow that to happen, because it might put the person at risk. It's not a given that adding X to standard therapy is going to make it better. Sometimes, it makes it worse. Or some people will say to you, "You know, if you were

me which would you choose?” My answer usually is, “Well, I wouldn’t know how to choose. That’s why we’re doing the study.” I don’t know. I don’t know which is better. Doctors have a tendency to say either, “I think actually Plus X is better,” or “It’s not going to make any difference.” Particularly if you have to refer a patient to another center, they might say, “Oh, plus X isn’t going to be any good.”

DT: [chuckles]

JK: It’s human nature.

[chuckles]

JK: That’s the way we are. We look out for our own interests. Doctors look out for their own interests. That’s an unfortunate reality.

DT: Maybe it’s hard with pediatric patients and maybe it’s more about their families, but I’m wondering if you’ve noticed any changes in the patients’ and their families’ attitudes towards their treating physicians and whether you have seen family members being more active about getting opinions and stating their preferences.

JK: Oh, I think so. I think people are clearly more knowledgeable and with knowledge comes progress. Patients often come now with some a paper that they got off the Internet that says, “This is really good,” whatever this is. So, then, you end up having to say, “Yes, this is fine. This is proven therapy and a good idea and I’m glad you read about it. The more you know about it, the better for you.” But it could be that that piece of paper is just junk. The Internet is just filled with testimonials of people that are cured with prune juice or carrot...whatever. That’s what an open system allows. It’s sometimes damaging. If prune juice is what they want, that’s what they’re going to get if they absolutely insist. You have to be very careful. So I think over all, it’s better. The first thing of good news is that people nowadays, as far as cancer, are much more open to talking about cancer. Nobody comes now and says, “I’ve got the Big C [cancer]. I know I’ve got the Big C [whispered].” Cancer always has been the dread disease and always will be because there are so many bad things about cancer, but there’s *much* more openness about talking about it and more knowledge about it and more to know. But with more to know also comes more misinformation. It’s also a challenge because to explain all of this to patients takes a lot of time.

One thing that’s kind of interesting that I’ve observed over the years—I don’t know whether you’ve ever thought about this—is there is a tendency for adult patients to sort of say, “Well, I want my treatment to be this.” “I want to have this kind of treatment.” “I want to have that kind of treatment.” But if you say, “Well, we have a research study where we’re doing this versus that,” people will say, “No, I’d rather have this.” And you say, “We don’t know whether that’s better.” “No, I want to have this.” In contrast, parents dealing with their own children who are, in many ways, the most important people in their lives, are much more likely to accept the research study.

In fact, it is interesting that over seventy percent of children with cancer in the United States are on some kind of clinical research protocol. What would you guess the percentage of adults with cancer are in clinical research protocols?

DT: It's going to be shocking, right? Thirty percent?

JK: No. Three percent.

DT: Wow!

JK: Three percent. That's just shocking. It's absolutely shocking. So there is this huge problem with people having preconceived ideas about things that are unknown, just having decided that they want this because they think they know best.

DT: You think then that in the pediatric population parents are more willing to concede authority and trust the physician than are adult patients?

JK: Yes. More parents are more willing to accept a clinical research study. Well, I think pediatricians are more honest than adult physicians are in saying, "I don't know. I don't know whether A or B is better." I think adult physicians are more likely to say, "I'd go with this one if I were you." The attitudinal difference is partially financially based. It's how you present clinical research to somebody.

DT: Do you ever have problems recruiting patients to into clinical research?

JK: Oh, sure! Oh, yes, we do for the reasons I just mentioned, plus there are lots of competing clinical protocols and lots of different options for people beyond. Also, for a patient to get on a research study does take a little more time. They have to follow their appointments and things are a little more rigid—and, yet, I think it results in better care.

DT: In cancer research, clinical research, is it more or less the case that protocols will compare standard therapy to some new experimental therapy. You never get the case where it's placebo?

JK: No, never with cancer. There are some prevention trials that are with a placebo, but they're trials with compounds that are likely to be marginal. Studies of green tea, for example. That's a good example.

DT: [chuckles]

JK: Green tea might prevent cancer, but the evidence isn't convincing yet. So it's very ethical to have a patient in a green tea study where there's a placebo. Obviously, the patient has to be told that. You know, you don't do that without the patient consent.

DT: I think I'll buy some green tea.

[chuckles]

JK: Yes, right.

DT: Given Minnesota is the “Medical Device Alley” that is very prominent in this part of Minnesota, has there been much of a relationship between the Cancer Center and also pre-Cancer Center the research that has been done here with medical device companies?

JK: I would say yes and no. As I told you, we were really fortunate to have as our leader of our fundraising campaign Win Wallin, who had been the CEO of Medtronic. He was able to entice some people from Medtronic to help us with fundraising for the Cancer Center. From that point of view, it’s been really, really good.

On the other hand, devices have not had very much of an impact on cancer treatment, so far. There are a couple of devices for pain control that device companies have had, but the impact of the device companies on cancer has been relatively little compared to cardiac. Maybe it will be, but so far, it hasn’t.

DT: I was thinking really about drug delivery systems.

JK: Yes, there are some potential drug delivery systems, but, of course, everyone’s dream is to have a pill. It doesn’t require any drug delivery. Most of the drugs now that are given intravenously are given through catheters that are pretty well established. They’re indwelling catheters. They don’t require a lot of electronics, so it’s pretty simple.

DT: Have you had much experience working with pharmaceutical companies?

JK: That’s an area where I think Minnesota is behind. We don’t have enough pharmaceutical companies in Minnesota in contrast to some other states. I wish we had more.

DT: You mentioned a little while ago that Don Thomas at Seattle got the Nobel Prize for his work with BMT.

JK: Yes.

DT: Given that here at Minnesota you were pioneering BMT was there any kind of...?

JK: Why didn’t we get it here?

DT: Yes. [laughter]

JK: Well, I would say that Thomas is very bright, a very good clinical scientist, very thoughtful. He’s now in his mid nineties. He was really a pioneer. He was the first one to use radiation, total body radiation, for treating leukemia patients prior to transplants.

They all died in the early days, because they didn't have matching... Seattle had and they still have the largest program in the country. I don't think there's any animosity. Bob Good might have thought that he should get the Nobel Prize, but didn't. Bob Good was a pioneer also. Interestingly, it's *extremely* unusual to give a Nobel Prize for clinical research. It turned out that year—I don't know what year it was exactly [1990]—his was shared with [Joseph E.] Murray for kidney transplants. In the same year, Thomas got it for bone marrow transplants.

EH: Wasn't he here for a while?

JK: Thomas?

EH: Yes.

JK: No. He often came to see us. He came to visit us here. What you might have seen is that he came here to visit during the 1960s, when Bob Good did the immune deficiency transplant. I think there's something in the notes about that. But he never did any work here.

DT: I don't know how they make those decisions and, obviously...

JK: [laughter]

DT: I'm sure there's more to the story.

JK: Oh, yes, oh, yes.

DT: It seems like an obvious question. [chuckles]

Is there anything else that I've missed that you would like...?

JK: You're pretty well covered everything.

[laughter]

DT: Emily, do you have...?

JK: Emily might have some things that weren't covered.

EH: I don't think so. You've covered a lot.

JK: You asked really good questions. You're a great interviewer.

DT: It definitely helps having some of the research that Emily has done that she shared with me. It was incredibly helpful.

JK: You got all the key points, so you two are a good working team.

DT: And we've got some names now. You mentioned some of the individuals before that we should think about talking to you. I wonder if Joanne Howard might be someone who would...

JK: She would be a good one to talk to.

DT: It would be good to get the nursing perspective.

JK: I suggested—maybe I didn't suggest it—that talking with Norma Ramsey and Phil McGlave would be useful. And I think talking to David Brown...you haven't talked to him yet?

DT: No, but I definitely plan to.

JK: I'd definitely talk to him, plus those other people, and Joanne Howard would be great. She's lively.

DT: It would be nice to have interviewed a family connection.

JK: Yes, a family connection.

DT: Great. Thank you so much, Doctor Kersey.

JK: Thank you.

DT: This has been very informative.

JK: Thank you for asking such good questions.

DT: [chuckles]

[End of the Interview]

Transcribed by Beverly Hermes

Hermes Transcribing & Research Service
12617 Fairgreen Avenue, St. Paul, Minnesota, 55124
952-953-0730 bhermes1@aol.com