

Emil Freireich, MD

Interview Session 2 — October 6, 2011

Chapter 00B

Interview Identifier

Tacey Ann Rosolowski, PhD

0:00:02.1

This is Tacey A. Rosolowski, interviewing Emil J Freireich, MD. This is the second session on October 6, 2011. The time is about 9:10.

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Chapter 6

B: MD Anderson Past

A Commitment to Training and Education: A Failed Battle to Bring a Medical School to MD Anderson

Story Codes

A: Personal Background

A: Professional Path

A: The Educator

B: Education

B: MD Anderson History

C: Understanding the Institution

B: Building/Transforming the Institution

D: Understanding Cancer, the History of Science, Cancer Research

D: The History of Health Care, Patient Care

C: Portraits

C: Leadership

C: Obstacles, Challenges

C: Controversies

Tacey Ann Rosolowski, PhD

0:00:02.1+

So I wanted to ask you just a couple of followup questions, after the conversation yesterday about your research. Part of it is just my own curiosity. You said in the interview that you did in 2001 that it was kind of unexpected that you ended up going into hematology. It was kind of really a circuitous path. I'm wondering if you felt that coming as you did—from the side, in an unusual path—helped you be innovative in that field.

Emil J Freireich, MD

0:00:59.9

Well, sure. As I've told you, you've read my background. Ever since I was a young man I always wanted to be a family doctor, like my idol, the family doctor. But as my training went along, I kept getting fired from every job for the same reason—I always did more than I was supposed to do. So I had an internship at Cook County Hospital. I got fired because I got into a controversy over patient care with a nurse. Then I went to internal medicine because I figured that I had learned all the surgical techniques and OB and all that, and I didn't know much about medicine. It was very complicated. So I took a year of medicine. My professor, who I adored, got fired and asked me what I didn't know a lot about, and I said hematology. We had a terrible hematology

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professor. So he said, “Go to Boston.” So that was the beginning of a transition from a practice-oriented career to a research-oriented career. The new academic research environment was created in Boston in the ‘60s and ‘50s.

So when I got there, I was offered jobs in hematology, but the one that I was forced to take was the only one that paid. In those days, trainees all came from well-to-do families and didn’t need money, so they worked for free. But Dr. [Merrick] Ross at Massachusetts Memorial had a grant to study iron deficiency, so instead of learning hematology as a general discipline, I was thrown into an academic situation where I had to do research, so that was the beginning of my research career. Then, as I’ve emphasized in previous interviews, just because the dean at Boston University was the first assistant secretary of health in the country, and just because the NIH opened in ‘64 and I got drafted in ‘64, Dr. [Chester Scott] Keefer recruited me to go to the NIH in ‘65. So there I was in a research job, taking care of children with leukemia. Remind me of the question again.

Tacey Ann Rosolowski, PhD

0:03:50.4

I was just curious about the way that—I mean—oftentimes when people are thinking about how an individual’s career has become very successful and why they’ve been so innovative, it turns out that very innovative people often come to a field from outside that field, so they bring a broader perspective or a different way of approaching problems. So I was just curious to what degree you found that the case in your own career.

Emil J Freireich, MD

0:04:18.8

Well, my career was a natural flow. I was never in a position to make any decisions; they were all made for me. When I found myself in the Clinical Center of the NIH, there we were with all these dying children. You had to do something about it. It’s important to recall that when I graduated from medical school in 1949, the undergraduate career in medicine included nothing about cancer. In our pathology, we learned a little bit about how the pathology of cancer looked, but there was no such thing as treatment. There was no such thing as natural history or diagnosis, so from a medical point of view, we knew nothing about cancer. We didn’t know it existed.

As you know, the first inkling of treatment for cancer came out of the Second World War with nitrogen mustard in ‘44—Dr. Farber—’48—6MP—’49. When I went to the Clinical Center, the concept of treating cancer was really a whole new thing. As I pointed out in previous conversations, since Dr. Zubrod inspired us to do formal clinical trials, we began to convert the knowledge about the natural history of leukemia into quantitative data so we knew how many children died and how often, what the complications were. We discovered meningeal leukemia and how to treat it, so we just had to learn quantitatively about the disease. Are we done with that? Because I want to get back to education.

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Tacey Ann Rosolowski, PhD

0:06:13.0

Yeah, let's go on to that.

Emil J Freireich, MD

0:06:17.6

Well, I think in my career the things I'm famous for are patient care and research, but the third leg of the academic stool is education. Education was always a part of my research career. When we went to the Clinical Center, under leadership of Dr. Frei and Dr. Zubrod, we had all these young physicians who had no talents in research who were recruited to the Clinical Center largely because of their military obligation. The Clinical Center was staffed with physicians who were drafted but weren't activated by the Navy, Air Force, and so on. The people who needed them were the Public Health Service, so they recruited all these young, bright physicians usually referred by the chairs of their department as promising, young, intelligent, potentially research oriented doctors.

In my lectures—you mentioned the things I do now—in the lectures I do now, I give a lecture on the origin of science in the United States. As you know, our medicine was based largely on European medicine. In the '40s and '50s, the physicians who were academic were all trained in Europe. The American science began, really, during my lifetime. It's a new thing for this country. Really, the turn of the century is when the Flexner Report and all that stuff—

When we were at the Clinical Center and we had all these young, intelligent physicians, Dr. Zubrod and Dr. Frei and I recognized that there needed to be a formal training program in oncology. There was no oncology discipline. We were just thrown into the Cancer Institute with all these cancer patients, so the young physician scientists who came to spend their two-year military service at the Clinical Center—ninety-plus percent of them ended up in academia because the environment was so academic. That is, the backbone of what we did was create a teaching program for these trainees so that after two years they were oncologists, although the word didn't exist at the time. So really the first formal training program in cancer began at the Clinical Center. The other academic institutions in the country all recognized the power of what had happened in Bethesda.

Tacey Ann Rosolowski, PhD

0:09:33.1

Can you tell me what was involved in that first program? What were the courses offered?

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Emil J Freireich, MD

0:09:37.8

Well, we didn't have courses, they learned by doing. They rotated through my service and learned about who came in and how to treat it and how to classify it and natural history and how to do randomized trials. They rotated through the solid tumor service. They learned how to take care of breast cancer, colon cancer. So we had a complete medical oncology program, and the fact that they rotated became a training program. They had a formal, systematic exposure to all the diseases in a setting where the physicians were scientists and not treating but observing and recording and studying the natural history of these diseases.

I'll come back to that—when I came to MD Anderson—because the environment we created was totally unique, and where it was going at the moment was the rest of the academic medical centers in the country were all created on the basis that academics had three legs of the stool—patient care, research, and education. When the Clinical Center opened, there were no students—there were no medical students—there were no interns, there were no residents. And I recall, in my first year there, my job was to recruit patients, so I traveled to the academic medical centers in the D.C. area. I went to Hopkins and G.W. and Georgetown and all those places to tell them that we were doing research. We were treating children with leukemia. We were trying to figure out how to cure them. The comments I got from the senior—in 1965, I was thirty-eight years old. The professors were sixty-five, and they all came from the old school.

I remember, particularly at Hopkins, Dr.—I forgot his name. He was a very famous physician, one of the founders of the Association of American Physicians. But he got up and he said, "It's ridiculous to have an institution where the physicians do patient care and research and no teaching." The stool is broken. But having a full-time research career was so productive. Then in the period from '65 to '75, academic medicine in the United States was totally transformed. All the medical schools developed academic clinical research programs, and they realized we had something with this teaching. So by 1964, the universities created a board exam in medical oncology patterned after this program that began at the NCI. So the point I'm making is that although we didn't teach undergraduates, graduated medical education was a component of my career from day one.

Tacey Ann Rosolowski, PhD

0:13:13.3

I was curious about how long it took you to recruit a critical mass of students from these other institutions.

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Emil J Freireich, MD

0:13:20.6

Oh, they came automatically. They were all in the Army, Navy—they all had commissions in the military, and their professors, like my professor, were contacted by Zubrod, Frei, or me. We went around and we told them there was an opportunity for these people to serve their—we called them Green Berets, because they could serve their military time in a peaceful situation rather than go fight in Korea, so it attracted the best and the brightest. The clinical associates that came to our hospital were the backbone of our research program. These guys were—they all became giants—[Dr. Vincent] DeVita; [Dr. George] Canellos; David Nathan, who won a Nobel Prize for his work at Hopkins; the guy at Children's Hospital who just got the AAP Kober Medal. All these people became the scientific leaders in the academic medical centers around the world. Two years the NCI converted them not only into oncologists but academic oncologists. So all the universities were populated with the graduates, and all the universities began to mimic that. They had their own training programs, and then finally they realized that, like gastroenterology and cardiology, oncology was a specialty, and the specialty exam was created in '64.

Tacey Ann Rosolowski, PhD

0:14:56.9

I'm starting to see why you said that that period between 1955 and 1965 transformed—

Emil J Freireich, MD

0:15:03.0

It was the golden age of medicine. It transformed all the specialties. What happened in oncology happened in cardiology. All the open heart surgery began at the Cancer Institute. It happened in neurosciences. All the studies of senile dementia—these were all people working around me. They were all like me—forty-year-old guys, fresh out of school, eager beavers in a candy shop. They had everything. They had patients, they had time, they had no responsibility. They did the research. They controlled their practice. They didn't have a service obligation, that's the main thing. I didn't have to see people dying with sickle cell anemia. I just saw leukemia patients. The guys who did neuroscience only saw multiple sclerosis patients.

The secret to clinical research is to be able to control your service work, to manage your research as part of your service. That's what MD Anderson—you hear Dr. [John] Mendelsohn [Oral History Interview] say, "Our patient care is research driven." That's what was discovered in the Clinical Center of the NIH. They discovered that the best patient care is research. The patients in research had the best chance of getting the best care.

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Training was a part of my career from day one. One of the reasons I came to MD Anderson—when Dr. Clark tried to recruit me, I had a job offer to go to University of Toronto, I had a job offer to go to Children’s Hospital, Harvard, in Boston. I could have gone anywhere, but the thing that Clark said that impressed me—I loved Dr. Clark. I hated him because he tortured me and fired me, but I loved him because of his vision of the future. He was a person—you know—when he took the job as director of MD Anderson, Houston was a backwater. The population was 200,000. They still had malaria. It was a very primitive community. But he saw the potential. He saw the oil industry. He felt immediately that Houston was going to be the biggest city in Texas, and when he recruited people, that’s what he told you. He said, “Houston is nothing, but it’s going to be the best, because we have everything. We have money, we have industry, we have culture, we have the water, and we have the climate. Houston is going to be the town.”

When I was thinking about coming here, he met with me and he said—you know—he was very impressed, as was everybody in the world, in what was going on at the Clinical Center. He said, “You know, the one thing the Clinical Center lacked was undergraduate education.” We had graduate medical education, but no undergraduate education. Dr. Clark, when he got a cancer center, he didn’t want it to be a cancer hospital, like Mayo Clinic. The first thing he did was negotiate with the University of Texas. The university—this is an academic institution. It’s not going to be like a TB hospital where people come to die. That was what the legislators wanted. The legislators conceived this place as a place like a TB hospital where you send dying cancer patients, get them off the street, get them out of their homes, put them in this place where they die. Not Dr. Clark. He was a surgeon. “We’re going to treat them.” And to do that, the very first thing he did, when he recruited colleagues from the military—you know—Dr. White to do surgery, Dr. Howe to do medicine—he recruited the backbone. The very first thing he did was recruit a basic scientist. He wanted to have research.

MD Anderson started off as a place with practitioners, not academic. They were all just doing their job, but he recruited basic scientists. He recruited Felix Haas from Galveston. They had three or four PhDs who began the research backbone of the hospital. And he insisted throughout that we be an academic—not a patient care—but an academic institution.

The very first idea he had was the basic scientists had to have post docs and undergraduates, but there’s no university. So they recruited post docs from other graduate programs around the country who came here to learn with the PhD’s working in the cancer hospital. But the post docs didn’t have any undergraduates who wanted to come, so Dr. Clark went to the university and said, “We have to have a graduate school.” Our graduate students had to drive to Austin to take their orals and their final exams. He said, “No. We have to have graduate school.” So the very first thing he did—we’re going to come to that when we talk about the PSTP—is it wasn’t just MD Anderson; it was MD Anderson’s Graduate School of Biomedical Science. And, God bless them, Dr. Grant Taylor, the head of Pediatrics was the first dean of the graduate school.

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When I came here in 1965, they recruited Alfred Leon Knudson, who is an absolute genius. He's a Nobel Laureate class scientist—physician-scientist. He was the dean of the graduate school. I am the longest serving full member of the graduate school. I don't have a graduate degree, but Dr. Clark felt I was qualified, and Dr. Taylor, and I became—I am still the longest-serving member of our graduate school—full member—and I don't have a PhD. But I'm better than all the PhDs.

After he got the graduate school, he convinced the regents that we had to have a health science center like there was in Galveston, like there was in Dallas, because he knew Houston was going to be the biggest city in Texas, and they had to have a medical school. So he convinced them to have the public health school. It's still there.

So when he was recruiting us—Frei and Freireich—he was chair of a committee of deans. There was the dean of MD Anderson—that was him—the dean of the graduate school—one of his employees—the dean of the public health school that he had recruited. Oh, and he also created a School of Allied Health, which is Ahearn [Michael Ahearn, MD ophd]. What was lacking was a medical school. So when he recruited us, I remember him telling me, "Freireich, we need to have a medical school," and it still doesn't exist today. Now you're talking 1965, so that's forty-six years ago. We need to have a medical school where the faculty is physician-scientists, where the first-year students who learn anatomy, physiology, and biochemistry learn it from working physician-scientists, so that all their knowledge is scientific. And when they get to the clinic, they're not going to go to Kelsey-Seybold and follow a doctor around; they're going to learn from professors who really know what they're doing—Freireichs who teach leukemia and DeVitas who teach Hodgkin's.

His vision was never realized, and one of the greatest frustrations in Dr. Clark's life was that when it came to the medical school, when he recruited us, he had already convinced the governing board of the Texas university system—I don't know what it's called, the Texas governing council for the university—that a medical school had to be in Houston. That was already approved. The medical school was coming. It was going to be staffed with outstanding physicians, and it's going to have not only cancer institute and heart institute and blood, everything was going to be research oriented in our academic medical center. Great triumph. I signed on immediately. That appealed to me enormously.

What happened, between the time I resigned my commission and I came here, Dr. Frei was fired, my department was eliminated, and he lost the medical school. It's complicated how it happened, but part of it was the war—Vietnam. That froze all construction. So although he had the money to build the Lutheran Pavilion, he couldn't do it. So although we were promised a whole ward with protected environment and beds, we had nothing but this little teeny lab because we didn't have a hospital. The research institute wasn't finished. We had no lab space. A big part of it was, as I mentioned before, political. That is, once the ball stopped rolling, at the same time the Supreme Court passed the one-man-one-vote thing—because all of west Texas was all Hispanic

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people and they didn't vote—but when the Supreme Court judged, then they got the vote, and the regents overruled the governing board of the medical center and moved our medical school to San Antonio. San Antonio got our medical school.

So our first year was no labs, no building, no money, no medical school. And then as we came out of the war and we began to get more money and we built our program, it was time for the medical school. Clark put his career on the line for that. He wanted a health science center in Houston. But the presidents in Galveston, particularly— See, what was happening is Galveston was the flagship health science center because of the Sealy money. Even though it was totally destroyed in a hurricane, they were still rebuilding it. They can't give it up because the Sealy money is put there and it can't be used for anything else, so you have to have Galveston. But Galveston was the flagship, and when Southwestern became University of Texas and grew in academic stature, that was a threat to Galveston, but they're 300 miles away. But Houston, that's fifty miles upstream, and the flow of patients and academics and grants and everything was flowing into Houston. The major opponent we had was Truman Blocker, who was the president at Galveston. The presidents at Southwestern and in San Antonio—all the university presidents—feared a medical school in Houston. And under Clark—they knew a medical school was coming, but not under Clark, because it was Clark—this would have been the greatest medical center in the country in a decade.

Dr. Clark's right-hand man was named Dr. Morton. Do you know Dr. Morton—Robert Morton? He was a radiotherapist, and he was Dr. Clark's political arm. He dealt with the regents. He made sure that we got all the money. When the medical school was denied to Clark, they not only gave him the medical school, they took the graduate school, the public health school, and the school of nursing away from MD Anderson and made a health science center—which is what they wanted—but they wouldn't give it to Clark. So what they did, because of his seniority, is they carved MD Anderson out of the health science center, and it didn't take very long for them to fire Dr. Clark. He totally lost his political capital in that battle—the battle with the other regents. As Clark was fired, Truman Blocker had retired as chancellor of the Galveston Health Science Center. They appointed him acting head of our health science center in Houston until they recruited a real one. At the first speech he made where I was present in the room, Dr. [Truman] Blocker said, "We're going to incorporate MD Anderson into the health science center."

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You see, there are only two institutions that own their hospital—Galveston, which is hopeless and will never be a major center because of geography, and MD Anderson. The other health science centers all have to use community hospitals. San Antonio has a county hospital, and Dallas has a county hospital. Tragically, our medical school doesn't have a county hospital. It has Hermann Hospital, a private hospital. What's going to happen to our medical school is what happened to Baylor. They're never going to make that an academic medical center. If you don't control your hospital, the financial pressures make it impossible to run the medical school. Ninety percent of your income comes from patient care, so if Hermann Hospital controls ninety percent of the budget, what does the president of the medical school control? Nothing, a few grants. So what happened to Baylor will happen to our medical school. It's never going to succeed. Hermann Hospital's interests are totally contravening to the medical school's interest. So Clark lost the battle. It was within a year that he was fired and they brought in Mickey [Dr. Charles A. LeMaistre [Oral History Interview]] with a search committee and all that stuff.

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Chapter 7

B: Building the Institution

Building the “Best Graduate Medical Education Program in the Country

Story Codes

A: Personal Background

A: Professional Path

A: The Educator

B: Education

B: MD Anderson History

B: Building/Transforming the Institution

Tacey Ann Rosolowski, PhD

0:30:53.9

Would you tell me about what happened to—?

Emil J Freireich, MD

0:30:55.1

I’m getting to education now. Go ahead. I can get back to it.

Tacey Ann Rosolowski, PhD

0:31:00.0

I was interested in how that educational leg of the stool got put back.

Emil J Freireich, MD

0:31:07.7

Well, that’s the thing. So the medical school started with this ad interim dean, and then they hired the guy from the space program. I can’t remember his name. He was hopelessly incompetent, and he had to start a medical school. A private hospital, a couple of rooms, and the only faculty they had was MD Anderson, so all of us became professors at the medical school. I was head of my oncology. Joel Moake was head of hematology. They recruited him. We were very close friends. We worked together.

So the medical school began with MD Anderson faculty, but the money rears its ugly head. They fired Clark. Dr. LeMaistre—others may have told you this, and it’s my—I want you to be careful how you quote all this because it’s going to hurt me. Dr. LeMaistre—I already mentioned this, but he was a very good friend of mine.

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Tacey Ann Rosolowski, PhD

0:32:29.8

Could I interrupt you just a second? All of this is being recorded, so if you prefer it to be off the record, I can turn off the machine right now.

Emil J Freireich, MD

0:32:36.3

No, I just want you to treat it gracefully.

Tacey Ann Rosolowski, PhD

0:32:38.6

Okay.

Emil J Freireich, MD

0:32:39.4

Dr. LeMaistre was chancellor of the University of Texas. I know the story of how he got to that position. It was not because of achievement. He was not competent. He was not a competent chancellor. The reason he got the job is that the guy that was chairman of the board of regents, Frank Erwin, was using the university as his political base to become governor of the state of Texas, and he ran the university. Mickey was, and always has been, a front man. He's handsome, he's thin, he gets his hair coiffed every morning. He has a deep base voice and speaks very beautifully. He's God's send to mankind. He's Mr. Perfect, provided someone else runs the university, which was Frank Erwin. Frank Erwin, as you know, got arrested on a DWI and his political career disappeared, so he resigned as chairman of the regents. They appointed a regular guy. The regular guy didn't want to run the university, so they had Mickey run the university.

Mickey insisted you call him Mickey, by the way. It's demeaning to me, but that's the way he liked to be addressed. I used to call him Dr. LeMaistre. He said, "Call me Mickey." So Mickey couldn't run the university, and it didn't take the regents five minutes to figure that out. So they had to get him a job without scandal. They don't fire people. So they made him chair of the research committee to replace Dr. Clark. At the same time, the no-good president of the health science center resigned. They told him, look, you can have your choice of jobs. Well, if he had a choice of that struggling institution and this enormously successful powerhouse created by Dr. Clark, with a great retirement program and everything in place, he took MD Anderson.

I was a candidate for that job. It's a tragedy that at that point in history they replaced Clark with Dr. LeMaistre. And as I mentioned before, LeMaistre immediately turned all the responsibility over to everybody else, and he became an administrative, not a—he wasn't a general; he was a secretary of state.

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I was running the medical school and teaching at the medical school. The students rotated—the undergraduate students—rotated through our hospital, because we had much better teaching than Hermann Hospital, who had a non-academic faculty. There was no one there who—they were all practitioners. They recruited a few guys like Joel Moake and the guy who ended up running the heart institute, so they had a few academics, but, to a large extent, the students were learning the way they did before Flexner. They learned apprenticed medicine. You follow some doctor around and do what he does. When the guy has swelling, you give him mercury. Why do you give him mercury? Well, we always give him mercury. Okay. So that's how they learned. Then there were us guys who were all scientists. We said, wait a minute—leukemia, hematology, science, blood. Well, the students hated the rotation order, because the kind of students they attracted were not academic types, they were practicing types. The mandate was produce family doctors. Family doctors don't think about science. So when they went to their class and they say, "Why are you giving him mercury?" They say, "Well, that's just the way you do it. Shut up and pass the test and go practice in Wichita."

So that situation didn't work at all. The students hated it. They didn't come. Some of the house staff liked it. That is, the graduate medical education was reasonable. But again, the people who wanted to be internists didn't want to learn the science of oncology; they wanted to learn everything—cardiology, et cetera. So again, the graduate medical program didn't work very well. There was only one thing that worked—graduate medical education. We trained the oncology specialists of the future. If a guy was a surgeon and wanted to treat cancer, come to MD Anderson.

So my situation with the medical school revolved around money. I was head of oncology. Oncology was the biggest service at Hermann Hospital. We were making tons of money. But what about the professional fees? I'm a full-time employee under contract. All the professional fees came to MD Anderson. The medical school guys realized that, "Dr. LeMaistre, we need the professional fees." "Yeah, but they're my faculty." "Well, we'll get our own faculty." LeMaistre compromised so that they kept the fees and we did the work. I staffed oncology with my trainees here—good oncology guys. We didn't have any money, so we applied for a training grant from the federal government. We got a good training grant. We began to—well, I'll tell you how the training program began. But anyway, the long and the short of it is that Dr. LeMaistre, in his perfect style, negotiated a settlement where they kept the fees and they had their own head and that was over. The relationship to the medical school disappeared in one day, because no one can have administrative responsibility when all the money goes to Hermann Hospital.

So that was the end of our connection to the medical school. I still have a joint appointment, but I go over there and give a lecture every two years. It's trivial. But graduate medical education was what we did here. It was just like NIH. What we did was make oncologists—surgical oncologists, medical oncologists.

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My specialty was leukemia. As you know, I started the department of Developmental Therapeutics. Dr. Frei was the first head, but I was deputy head. Frei and Freireich were like two arms of a body. I mean, I loved the man. I respected him, and he respected me. We had totally different personalities, but we were completely complementary. So Frei and Freireich didn't equal two; we equaled four. We were really—our brains were intermingled. We started this fellowship program in developmental therapeutics. It started with—I think I mentioned the other day—with the Japanese exchange students. We had a very good friend in Dallas, Dr. Hill, who ran the blood bank. He tried to recruit me, initially, so we were good friends. He had a number of these Japanese fellows come over to learn blood banking. He would send them up to rotate, and they would stay.

My first fellow was a Mexican. His name was Adolfo Isassi. He ended up being dean of a medical school in Mexico. He is a very smart, academic guy. Then we started to get these Japanese fellows. The important point is that our training program, which began with these foreign nationals, has populated the world. The first fellow we had from Japan, Nazemi Hurano, became president of a new medical school in Nagoya. He's one of the most famous academics in Japan. The second one we had who got the award as a distinguished alumni, Dr. Ohno, became the chair of the first Japanese cooperative chemotherapy group and he became dean of a medical school. He's still working, very famous.

When I go to Japan, and I go often, the famous academics in Japan have a party for me—a geisha party—and we sit and they all talk about old times at MD Anderson. We developed—I don't want to say singlehandedly, but almost singlehandedly—created academic oncology in Japan. From Japan, we began—as I said, we had Mexico. We had a young physician, Carlos Vallejos, who from—not Chile, not Argentina—what's the other one on the west coast? Anyhow, he was director of the cancer center there and very famous. We populated academic oncology around the world with our training program. We attracted them.

Then we began to attract some Americans when Americans realized that oncology, in universities, was a discipline. We began to attract and we trained some of the most famous medical oncologists in the United States—Larry Einhorn, who won the Lasker Prize for his work on testicular cancer; Bob Livingston, who became head of oncology at Washington University. We trained all the giants here—Hagop Kantarjian, who is head of leukemia; Bob Benjamin, who is head of sarcoma; Gabe Hortobagyi [Gabriel Hortobagyi, MD [Oral History Interview]], who is head of the breast cancer service. We populated the institution with academically trained people who came to DT to learn oncology as a science—research-driven patient care. So we created that element at MD Anderson, and we were very successful, which leads to vulcanizing our department.

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When Dr. LeMaistre eliminated DT, the training responsibilities devolved to the various departments. Then, as I say, he gave me hematology, and hematology got too big. They hired this guy, [Al] Deisseroth. They vulcanized hematology. It was too big. So now we have leukemia transplant. See, we brought immunology, infectious disease, pharmacology, hematology, molecular biology—all the disciplines came through DT, so when they finally vulcanized leukemia, I was head of the leukemia service. Dr. LeMaistre realized that he had Conrad running the hospital. He had Fred Becker recruited to run the basic sciences in laboratory medicine. He needed someone to run the clinical science, so he invited me to be vice president for clinical research. There was nothing I would rather do than that. So we had a meeting, a typical LeMaistre meeting.

All the LeMaistre meetings were the same. You do the talking, he listens, and then he says, well, this and that. No decision is ever made. He had a sign on his desk, you never make a decision, because if you make a decision, it's going to come back and bite you. So he never made a decision. It was part of his public presence. So he said, okay, it's a great idea. It was Dr. Hickey's idea, actually. Dr. Hickey was an academic surgeon. He was the director. Dr. LeMaistre appointed him so he wouldn't have to worry about directing the hospital, and he had Conrad running the medical oncology. He said, "It's a great idea, but before we do it, we don't want to upset anybody, so you have to get approval from Dr. Becker, who is vice president of research, and Dr. Conrad, who is the vice president for patient care, and you'll be vice president for clinical research."

At the time we had a clinic center grant, which I was the PI on, and we had a whole ward on Three West, which was our clinical research center, so I was already running the clinical research program here. So I went to Fred Becker [Frederick Becker, MD [Oral History Interview]], and Becker said, "Wow." Do you know Fred Becker?

Tacey Ann Rosolowski, PhD

0:47:07.3

Uh-hunh (affirmative).

Emil J Freireich, MD

0:47:07.7

Is he on the thing?

Tacey Ann Rosolowski, PhD

0:47:09.0

No.

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Emil J Freireich, MD

0:47:09.9

Fred Becker is another giant. He said, "Great idea. Just what we need. Freireich, you and I, we can work together." Then I went to see Conrad. Conrad said, "Clinical research? If it's patient care, it's mine. There's no clinical research." So I went back to LeMaistre and I said, "Dr. Conrad—Becker is totally on, but Conrad"—He said, "Wait a minute, I have a note here from Fred Becker. Fred Becker says, 'There's no such thing as clinical research. If it's research, I'm in charge.'" Becker—swine. What he told me was the inverse of what he wrote. So LeMaistre said, "Okay, you can't get approval from two vice presidents. You're out." So I got fired again. I've been fired eight times by LeMaistre. He couldn't tolerate me. Well, we're actually good friends. He comes up and puts his arm around me. So I'm missing a big part of the story. Can I take notes?

Tacey Ann Rosolowski, PhD

0:48:37.9

Sure.

Emil J Freireich, MD

0:48:45.1

He realized that he had Conrad who did no research. He had Becker who did lab research. He needed this vice president for clinical research. He had a brilliant idea. He needed a chief academic officer, what DuBois is now. That was a brilliant idea. He looked around at who he could get, and he selected Andrew von Eschenbach. He's probably on your list to interview. I hope you interview him. He was the head of GU surgery. He's a surgeon, a very accomplished academic surgeon. He and I were very good friends. We worked together on many projects. He became chief academic officer. Wow.

He did many good things. He was chief academic officer for about a year. The first thing he did was they built this building for all the administrators. He said, no, let's put the faculty over there. We need more room over here. He was a very strong advocate in the faculty. It didn't take long for Dr. LeMaistre to realize he created a monster, because now he had someone who was actually doing something, which was bad for him because there was going to be conflict between him and Conrad and Becker, which was just what he can't stand because he'd have to make a decision. He never made a decision. So [Andrew] von Eschenbach was fired after a year, and he went to the FDA. But von Eschenbach, during his time, realized that although we were the most powerful graduate medical education organization in the country, we didn't have a graduate medical education program. So he called me to his office and said, "Freireich, you've done a lot. Why don't you take over education?" And it was actually his idea to create a core curriculum and have a place where the graduate medical trainees can go for guidance in their careers, other than their subspecialty.

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At the time, grand rounds were being run by Dr. [Aman U.] Buzdar, who is another one of my very good friends, a Neanderthal from India or somewhere. Dr. Buzdar was running grand rounds. I was interested in teaching, and I used to go. We had maybe ten to twenty people coming to the Hickey Auditorium every week. It was a dying enterprise. Von Eschenbach called me up and said, “Why don’t you take over grand rounds?” And I did. So of course I made grand rounds a major teaching thing because we focus on research. We don’t have a one-hour seminar; we have multi-disciplinary presentations. It’s now the number one teaching program in the institution.

Tacey Ann Rosolowski, PhD

0:52:22.0

So that was in 1997?

Emil J Freireich, MD

0:52:24.3

I think so.

Tacey Ann Rosolowski, PhD

0:52:25.7

Yeah, 1997, Institutional Grand Rounds is what the name of the program—

Emil J Freireich, MD

0:52:29.4

The core curriculum became part of the GME program, and then we got an organized GME program. We recruited people to be head of GME, and Dr. [Stephen P.] Tomasovic was made vice president for academic affairs. Then, when von Eschenbach was fired, they hired Margaret Kripke [Oral History Interview], and Kripke, first class lady, scientist, quality person. She looked at what we were doing, and she said, “Freireich, why don’t you take over graduate medical education?” Fine. So she told Tomasovic [Stephen Tomasovic, Ph.D. [Oral History Interview]] to take me.

Tomasovic controls all the education activities in the institution, but he hasn’t got a single doctor on his faculty. All the directors are librarians and stuff. So Tomasovic said, “Good idea.” So they made me director of medical education. But they didn’t want me to be director of anything because, be careful, first thing you know I’d take over the whole thing, so they called me Director of Special Medical Education, meaning that I only control things that Tomasovic lets me control. And that’s a position I still have, which I’m delighted to have. It is seventy percent of my activity that I spend on graduate medical education, and I love it, and the fellows love it. We have the best Graduate Medical Education Program in the country, believe me.

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Chapter 8

B: Building the Institution

A History of Institutional Grand Rounds, The Core Curriculum, and the Physician's Assistant Training Program

Story Codes

B: Building/Transforming the Institution

B: Multi-disciplinary Approaches

B: Research, Care, and Education in Transition

C: Understanding the Institution

B: Education

B: MD Anderson History

B: Institutional Processes

C: Education at MD Anderson

Tacey Ann Rosolowski, PhD

0:54:04.0

I wanted to ask you for a little bit more detail about each of these activities. Institutional grand rounds, what did you do to transform that?

Emil J Freireich, MD

0:54:15.1

What we did is instead of having a speaker talk for an hour about his research, we made it lively. We insisted on there being multiple speakers—three to four—because if you tell a guy, “I want you to tell me about your research. Take an hour,” he’s going to tell you everything. There’s going to be tables and the P values and such. And the only people who attend grand rounds in seminar are the people who are interested in that area—ten people. We’ve got 1,000 faculty. Ten people come. So in order to get people from all disciplines, it has to be multidisciplinary. So I managed grand rounds so that every session limits the speakers to fifteen or twenty minutes, so they have to present summaries. That means that anyone not working in the field will be informed about what he is doing. Secondly, we made the rounds multidisciplinary. So if we talk about laryngectomy, we have a surgeon or radiotherapist or medical oncologist. If we talk about apoptosis, we have a cell biologist, we have a clinician who is working on chemotherapy, and we have a guy working on DNA. So we make the rounds appeal to a broad audience; we make them lively and fast, so grand rounds is successful. We average about 150 people, not ten. It’s a fantastic academic institution. We record it. People who can’t come because of their clinic on Friday can watch it on their TV. I do it when I’m out of town. I watch it. It’s great education.

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The core curriculum is mandatory. The GME committee—I'm an ad hoc member of the GME committee which consists of all the GME program directors. They elect the chair. I'm kind of an ad hoc member. I chair the curriculum subcommittee, and the curriculum subcommittee has members from all different graduate medical education programs. We review our core curriculum on a regular basis, and we analyze—the students testify as to the outcome. We get quantitative measures of whether they're learning. They get a little exam with each thing. We have a report every quarter from Marilyn Greer's department in academic affairs. So our core curriculum is really academic.

Tacey Ann Rosolowski, PhD

0:56:50.1

How were the decisions made about what the core curriculum should include?

Emil J Freireich, MD

0:56:55.1

Well, it's me. I'm absolute dictator.

Tacey Ann Rosolowski, PhD

0:57:00.8

So tell me, Dictator, what does the core curriculum—what was the logic of it?

Emil J Freireich, MD

0:57:03.7

The logic of the core curriculum is if you come here to be a radiotherapist, the least you have to learn is the basics of oncology. You have to know biochemistry. You have to know pharmacology. You have to know medicine. You have to know cancer. You have to know lung, colon, breast, everything. So the core curriculum creates, for the first-year graduate medical trainee here, a core of oncology knowledge that you'll only get at MD Anderson.

So if you're going to be a surgeon, you're going to know everything. If you're going to be a gynecologist, you're going to know everything. If you're in medical oncology, if you're a radiotherapist— So the graduate education committee voted unanimously that all first-year graduate medical trainees here are required to take the core curriculum. They are required to attend eighty percent of the sessions. If they can't attend in person, they are allowed to do it online. We record them all, and they are on video. They are required to assess the course, whether it fulfills their needs or not, and I report regularly to the graduate education committee on how we're doing. As I say, we have a committee of people who are interested in graduate medical education—multidisciplinary. We have educators, physicians, scientists, so on. We meet regularly and supervise it, so it's really an academic exercise. It's institution-wide.

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Tacey Ann Rosolowski, PhD

0:58:31.6

So what are your findings in the performance of the students and how they go on?

Emil J Freireich, MD

0:58:39.0

It's the best training program in the country. It's highly effective. There are some subspecialties. Like people who come here to do radiology, they don't like the core curriculum. But I keep pressing on their program director and say, "Look, you're going to be a radiologist, go to Hermann Hospital, but if you come to MD Anderson, you're an oncologist." And oncologic radiology is going to be a specialty, so you better know about oncology. You've got to know about mitosis and metastasis and cell proliferation and chemotherapy and so on. So, at least for now, the training program directors all embrace it. As I say, the vote is unanimously in support of the program. It's assessed and we present the results of the student evaluations, the faculty evaluations, and the performance of the students regularly at the graduate medical education committee which meets quarterly. We give a report at that meeting on how they're doing.

Tacey Ann Rosolowski, PhD

0:59:42.4

Now, that program started in 2000. How many students did you start with? How many students do you handle now?

Emil J Freireich, MD

0:59:48.5

Well, we currently have—I'd have to look for those statistics, but all of the first-year graduate medical trainees are required to take it, so it's 140 or 150, something of that order.

Tacey Ann Rosolowski, PhD

1:00:03.7

Wow. We haven't talked about the physician-assisted continuing education. That came in the same year.

Emil J Freireich, MD

1:00:11.9

Well, we'll deal with that briefly. I can't recall the year, but it was while I was here that a famous chairman of medicine at Duke University—I'm blocking out his name right now, but it will come to me in a moment. He decided that there should be a career called a physician's assistant. He invented the idea. The idea being that physicians are poor at some aspects of patient care. What they're good at is their brains. He was very academic. His name was Don Seldin. No, that's one of his students in Dallas at Southwestern. His name will come to me in a moment. But anyway, he created the idea of having a medically trained person who is not an MD but can do anything an MD can do physically but not make the decisions. That's the doctor's responsibility.

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He called them physician assistants. It was kind of slow to catch on. A number of academic centers began to do it, but being a great innovator that I am, I sensed immediately that something was important here. I was head of DT—

Tacey Ann Rosolowski, PhD

1:01:49.5

What was it about the idea that you thought was so important?

Emil J Freireich, MD

1:01:53.1

Well, I'm coming to that. I was head of DT, but I never separated myself from patient care. I insisted that if I was going to have a faculty, I would be at least as good, if not better, than everyone on my faculty. So I was the best doctor. If you had to consult somebody on leukemia, you'd come to me. And to maintain your competence, you have to practice. Not in the usual sense of tennis, you have to do patient care.

But as I got more and more administration, the department grew. We ended up with 250 employees and thirty faculty. It began to impinge on my patient care time. When I heard about this idea of a physician assistant, I was the first physician at MD Anderson to recruit a physician assistant. The first problem was how to pay them. So I went to the PRS and I convinced them that my income—that they could pay them the amount of income that my practice accelerated as a result of this PA. And within a year it was proven that the amount of income I generated with a PA was more than the PA's salary, so the PRS has taken over the PA funding. So they're funded just like the doctors, out of fees for service. The consequence is that the way it works is when I get a new patient referral from a doctor, he calls me. "Dr. Freireich, this is the chairman of the board of General Motors. He needs the best doctor in the world." "Yes, sir. I'll take him immediately." He comes in and the first person he sees is my PA. The PA takes a detailed history, a physical. She spends an hour with the patient, and she can afford to because she's not being billed at the rates I'm billing. When she gets done with all that, she orders all the laboratory stuff, and when everything is ready, she presents the case to me and I make the decisions. The consequence of that is the number of patients I can see has expanded by tenfold.

Well, the PA idea—my first PA—I can't remember her name. The PA that really mattered has recently retired. She's an absolute genius. I forget her name. Anyway, so the idea of the PA thing—it didn't take a year for every doctor in the hospital to realize what had happened, particularly the surgeons and the medical people. They realized that if I'm in the OR four hours a day, three days a week, and patients are coming to the clinic every day, if my PA can do the histories and physicals, get all the lab stuff, I can come in and decide what kind of operation is needed and go to the OR. So it caught on immediately, and the number of PAs grew.

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Kathryn Boyer—she's retired. She married one of our faculty and had a baby. I still deal with her every day, but she was just fantastic. It takes a personality of a person who is excellent as a doctor but just goes so far. They don't want the responsibility. So you can find people who love doing— It's like nurses. They love doing the care, but they don't want to make the decision as to whether you get your lung taken out or your brain fixed.

So anyhow, the PA program became very successful, and Kathryn Boyer formed an association—the PA Association. They had seminars, and they worked like the Graduate Medical Education Program. Although she founded it, I was the principal faculty member. Eventually we recruited a gal who was even better, the one who runs it now. I'm blacking out her name. It runs itself, and I just help them. I'm not administratively responsible for it. It's a terrific program.

Tacey Ann Rosolowski, PhD

1:06:17.2

So does MD Anderson train physician assistants?

Emil J Freireich, MD

1:06:22.5

Yes.

Tacey Ann Rosolowski, PhD

1:06:23.1

Okay, and then there's also the Physician Assistant Continuing Education Program?

Emil J Freireich, MD

1:06:26.4

Yes.

Tacey Ann Rosolowski, PhD

1:06:27.5

So tell me about the differences between the two.

Emil J Freireich, MD

1:06:30.4

Well, when the PAs are here, it's like the core curriculum. If they work for a thoracic surgeon, they only do lung cancer, but they have to be competent in cancer. So they have a CME curriculum where they have to be competent in the new developments in leukemia and everything else. And that curriculum is run by the PA people themselves. Then the other question was—?

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Tacey Ann Rosolowski, PhD

1:06:53.2

Oh, I was interested in the actual training program for the physician assistants themselves.

Emil J Freireich, MD

1:06:59.4

Well, there are formal training programs at the academic medical centers all around the country. We don't have one. But the PAs who want to do—we have a graduate medical program for PAs. So the ones who get PA degrees from Baylor—most of them come from Baylor, some from the Health Science Center—they come here to do a clerkship. If they like oncology, they do graduate medical training here, and they become faculty.

Tacey Ann Rosolowski, PhD

1:07:25.6

And then there's the continuing education?

Emil J Freireich, MD

1:07:27.5

Yeah, that's the existing—to stay broadly based, like our core curriculum. They can't just worry about themselves. They have to be competent in chemotherapy, what's going on, what they're patients might be eligible for.

Tacey Ann Rosolowski, PhD

1:07:41.0

So the continuing education came in 2000, and I'm just interested in the way that's the same year as the Core Curriculum. So there was this sense that, wow, there's a lot going on. There's a lot everybody has to keep up with. So that was the rationale. Interesting.

Emil J Freireich, MD

1:07:57.1

Yeah. I give more credit for the PA program to the PAs than to me. I was just psychological support. But the Core Curriculum was my baby. Now, the Physician-Scientist Program—okay. So, when Tomasovic appointed me to this position as Director of Special Medical Education, it reduced my clinical time to thirty percent, and I had seventy percent of my time now for education. Prior to that, the educational activities were always grafted on my clinical administrators. Now I had a position, thanks to Dr. [Margaret] Kripke, where I could focus on education, which I love. And why did I tell you that?

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Chapter 9

B: Building the Institution

Creating Patient-Oriented Research in a Complex Scientific and Institutional Context

Story Codes

B: MD Anderson History

B: MD Anderson Snapshot

B: Institutional Processes

D: On Research and Researchers

D: Understanding Cancer, the History of Science, Cancer Research

D: The History of Health Care, Patient Care

C: Portraits

C: Understanding the Institution

B: Education

D: Cultural/Social Influences

A: Critical Perspectives

Tacey Ann Rosolowski, PhD

1:08:53.0

You were setting up the context for the physician-scientist.

Emil J Freireich, MD

1:08:57.3

The first thing I decided to do is— I learned, when I came to MD Anderson, that the secret to success in anything you undertake is money. The reason I succeeded in DT [the Department of Developmental Therapeutics] is we went and got grants. The reason we had a training program is we got a federal grant for a training program. We paid our students. We didn't have to welch off of the institution. We didn't have to beg for money. We got grants from the Cancer Society and the Leukemia Society. We were active in fundraising. We got endowments from our patients. So when I got this job as Director of Special Medical Education, the first thing I had to do is raise money, so I went to the granting agencies, and I discovered a thing that had just been created either the year of or the year before I applied, which was called the K-30. One of the things I'm most proud of is the Association for Patient-Oriented Research. It's 10:30. How much time do we have left?

Tacey Ann Rosolowski, PhD

1:10:16.5

Another hour.

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Emil J Freireich, MD

1:10:20.9

Okay. The NIH has a—research in general has a structural problem. The structural problem is that the ethos in the country is that if you want to cure a disease, if you want to improve health, you have to hire a bunch of basic scientists, put them in a laboratory, and have them discover everything about everything. If you clone the human genome and you get all the cells and culture and you do tissue culture and biochemistry and you do all the basic science, then these geniuses who get the Nobel prizes—there's never been a Nobel Prize for a doctor. They're all geniuses. When the geniuses work out all the basic knowledge, then they call some stupid doctor who just dispenses pills and he cures cancer. And that's an ethos that exists in the United States and in the world. Everybody believes that the way you solve problems is to do basic research. That's why the director of the NIH is a basic scientist. That's why the director of the National Cancer Institute is a basic scientist. They've never seen a sick person, and they don't know what cancer is all about, but they do know how to clone genes and grow bacteria and culture and lab and get grants, so the entire federal grant money flow goes to laboratory science.

Now there's a minority of us who realize that in fact not ninety-eight percent but 100% of advances in our ability to manage disease in man began at the bedside, with a doctor. The flow of knowledge about disease is not from the lab to the clinic; it's from the clinic to the lab, like we did platelets. I had to have a bleeding patient to go to the lab and figure out that it worked then go back to the patient.

In the United States of America and in the world, this ethos that clinicians are dumb, idiot, stupid guys who just apply basic science is universal. So the flow of funding to this day is to support laboratory science. If you're doing regular research—you're growing leukemia cells in culture, you're throwing a chemical on them—it's called regular science. The philosophers of science call this regular science. Regular science says you just do what comes next. If you know how bacteria grow and you throw it in and they don't grow, well, you want to find out why they don't grow. So you put more in and find out if they grow faster. It's nothing to do with humans. It has nothing to do with disease. It just— Do you mind obscenities? It's just friggin' around. That's what you get from Josh Fidler [Isaiah Joshua Fidler, DVM, PhD [Oral History Interview]]—basic science. He understands metastasis. His understanding of metastasis has nothing to do with metastasis in the clinic. It's all fairytale. Fidler's work will never contribute to the health of anybody. He gets cancer cells out of a mouse. He grows them in culture until they're automatically growing, which never happens with human cells, then he shoots them into the vein of a mouse and he gets spots on the lung—metastasis.

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I've written an editorial on this, and it's been published. Fidler's metastasis models are all laboratory stuff. He's famous because that's what people think is important. They don't care about curing metastatic cancer. They don't want to understand how cancer metastases occur in the clinic. They want to get grants and work out this—and they have this ideology. It's like church—basic science.

So there's a handful of people who do clinical research who recognize that basic science is useful in their thinking, but the important part of advancing and understanding disease is to study disease. Leukemia in a mouse has no relationship to leukemia in man—none. The things learned in a laboratory can be applied by clinicians if the information is relevant, but the observations begin at the bedside. And there are no exceptions to that. If you look at how did chemotherapy begin? Did some genius, like Dr. [Sidney] Farber, go to the patient-scientist lab and put methotrexate—and then it worked in children? Hell no. He had an idea that it would grow and he gave it to children. Everything that's occurred that's advanced the health of man has occurred because someone made an observation in a sick person and then tried to understand it. AIDS was done by doctors in practice.

When I went to medical school, the dean of our medical school was a guy named Andrew C. Ivy. He won the Nobel Prize. He was the most famous physiologist in the United States. He became dean of the school of medicine. The medical schools loved to do this. The dean—to teach people how to be a doctor—is a basic scientist. He doesn't know anything about medicine. His specialty was peptic ulcers. He had worked on animals, and he developed the acid theory of peptic ulcers. When we were medical students, we had more lectures on peptic ulcers than any other disease. We understood it totally, because in dogs, when you do this and you cut the nerves and you put acid—you had ulcers. If you do this and that, it goes away. We understand peptic ulcers, and some dumb doctor in Australia made cultures of the blood and found that there was a bacteria that causes ulcers. The acid has nothing to do with it. If you want to cure peptic ulcers, which is now a historically over disease, you give antibiotics.

And that's the story of medicine in the United States. We have this illusion, and certainly all the managers do, all the directors, all the head of NCI, NIH, all the senators. Everybody believes that we need more basic science. We need Fidler. Do metastasis research. Get a prize—a Nobel Prize. It doesn't do anything for metastasis. Nothing Fidler has done applies to man at all. It's all silly. We have patients—well, he does this in his lectures. We have patients with ovarian cancer, if you take their blood, they've got cancer cells running around 200,000—they don't have metastasis. It just stays in the belly. It's got to do with the biology of the disease that we understand—the doctors.

Anyhow, there are a handful of doctors working at the NIH, working on disease, and they said, look, maybe if ninety-nine percent of the budget goes to laboratory guys—to Fidler—maybe we could put two percent—maybe we could make it three percent—for clinical research. Oh, well, that's—more basic science.

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There were some people who convinced the NIH that maybe there was something to this clinical research thing. They refused to admit that 100%—not ninety-nine—of advances in care of patients occur by physicians taking care of patients who then work it out in the laboratory. But they were willing to say, maybe a couple of them, so they created a program called the K-30.

The K-series of grants were awards for training. They trained, essentially, basic scientists. But some guy made a mistake. It slipped through. So the big basic scientists who ran the cancer—the NIH, all these—you know—what’s the name of the guy who runs the NIH? He’s world famous. He cloned the human genome, never helped anybody. The guy who runs the Cancer Institute got a Nobel Prize for discovering an enzyme. He’s director of the Cancer Institute. But somehow they said, well, we’ve got all these training grants. Maybe we ought to have one for doctors. The number thirty—K-30—the other twenty-nine is— So they made this program, K-30, and they put some money there. I looked through things and said, “Aha. That’s for us.” So I applied for a K-30 grant. I figured the way to succeed in my business is make money.

Well, my friend and mentor, Dr. LeMaistre, discovered that a man named Jon Tyson, who is a pediatrician over at the medical school, had also prepared a training grant for a K-30. So the dean of the medical school, in consort with our leader, decided that the likelihood—since there were a very small number of these grants—that two of them would come to the Health Science Center was very unlikely. So we opt to combine them. So we had a meeting, and I presented my grant, Dr. Tyson presented his—which was totally stupid—and the decision had to be made by people who knew nothing. So the decision was obvious. Mickey said, “Tyson will be the PI and Freireich can be the co-PI.” So we had a joint program.

The title of the K-30 grant was Physician-Scientist Training Program. So we send in the joint award, and we got it. It was an award for five years. Within the first week it was obvious that we were mixing apples and oranges. We planned the curriculum, and Tyson called me into his office. This is an arrogant, young, unaccomplished, stupid guy. He said, “Freireich, let me tell you how we’re going to run this course.” What? He said, “The first thing you have to understand is how to do a randomized clinical trial.” I said, “Gee, you’re the one who can teach me.” You know who did the first published randomized trial in the world? And this young shit—excuse me—this young twerp is going to lecture me in his office. So that meeting was very short. I left.

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To give you an example, in his first lecture he told people how accomplished he was. He chaired a multi-institution cooperative group involving several hundred women who delivered babies. The scientific question being investigated was, when the baby is delivered, do you cut the cord at one inch or two inches? And after two years and millions of dollars, they discovered that one inch was better than two inches. Now that's research from which no one had ever benefitted because we were immediately saying cord-blood stem cells, you have to cut it as short as you can and squeeze out— He was an absolute goon, but he's one of these goons—you know—there are people who are stupid but are humble, but when people who are stupid are arrogant—you know—it's like Adolph Hitler. He thought he knew more than I did. He'd never done anything. I'm the one who cured childhood leukemia, and he's lecturing to me.

So within a week, the courses were— Our students came to our courses, their students went to their courses. A couple of their students listened to our courses. We had it broadcast over there. At first he dominated all of it, and then I insisted that we do some. The first one I gave he sent me an email and said, "I enjoyed your lecture." But the two came totally apart. By the third year, then we had to fight over the money because he needed this for students and I needed this for that.

Tacey Ann Rosolowski, PhD

1:25:07.2

What did you focus on in the portion that you administered?

Emil J Freireich, MD

1:25:11.1

Physician-Scientist Training Program.

Tacey Ann Rosolowski, PhD

1:25:13.4

What were the components of it?

Emil J Freireich, MD

1:25:15.9

Well, the curriculum was how to do clinical research, what the principles are, a little bit of statistics, randomization, clinical trials, observation, and experiences. I give a lecture now to the students still on the essence of clinical research—objectivity, quantization, objective measurements, lack of bias. So our course was terrific. We attracted good students.

As we were going along, I got the idea that our students were learning a subspecialty, but they're not certified. It seemed to me that we ought to separate the academic physician scientist whose profession is research from the physician scientist who is practicing scientific medicine. They needed a credential. So I decided the credential they needed was a graduate degree—a master of science or a PhD So I went to the graduate school and I said, "I want to have a graduate degree in

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patient-oriented research.” Whoa! There’s no such thing as patient—patient care—there’s no research. Inject leukemia cells in the tail of a mouse; that’s research.

If you can get a PhD working on mice, why can’t you get a PhD working on people? It’s a little more complicated. People are a little more difficult to control. They’re not pure, inbred species with the same sets of genes—identical twins. With one kind of leukemia cell—Dr. Fidler is still working with L1210 that was isolated in 1953. There’s 10,000 different mouse leukemias. They all have different patterns of spread. You have to understand what makes them do that. So we went to the graduate school, and we met with what you might call skepticism.

Tacey Ann Rosolowski, PhD

1:28:07.1

I can imagine.

Emil J Freireich, MD

1:28:08.6

It’s difficult for a basic scientist to understand clinical research because the system is inverted. You see, a basic scientist wakes up in the morning and says, “Gee, wouldn’t it be nice to know how—? Let’s work on that.” A physician-scientist comes to work in the morning, and you’re bleeding. Dr. Freireich, what are you going to do? The physician-scientist is presented with his problem. The laboratory scientist can work on anything he wants. [Dr. Joshua] Fidler, take some mice, shoot in cells, Nobel Prize. The guys who got the Nobel Prize this time—immunologists. They put T-cells in mice—Nobel Prize. Sick patients? Some dumb doctor will figure out how to use this basic knowledge to cure humans. That’s the ethos.

So we met some resistance, but I’m not modest, so I was persistent. I convinced people slowly, one at a time, and eventually we got through to the curriculum committee, we got through the academic standards committee, we got through the executive committee.

Dean [George] Stancel [Oral History Interview] —the dean said it would degrade the graduate school if they had a program in patient-oriented research and awarded these inferior degrees to these stupid doctors who are not Fidler—basic scientists. As I say, I’m not modest, and I worked with Dean Stancel. We were good friends. After about a year of pressure, the dean agreed to have a program which he called Patient-Based Research. See, it’s not patient-oriented, it’s not that you’re working on disease, it is that you’re working on basic problems and it’s based in patients—patient-based research. Okay.

So we started that, and the students that we had enough money for, we enrolled them in the graduate school. We created courses that were minimum requirements. We had a curriculum. We started graduating students with masters. And actually, I’ve taken three students in my lab that are PhDs. Okay, the grant runs out in five years. I can’t recall the year. You probably have it.

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Tacey Ann Rosolowski, PhD

1:31:18.7

Yeah, it started in 1999, so I guess it ran out in 2004.

Emil J Freireich, MD

1:31:23.4

Yeah, '04. Okay, in '04, Dr. Freireich is seventy-seven years old. I'm now eight-four. Old age. The second culture, in addition to bias in favor of basic science, is we're very age biased. We have to give young people an opportunity. So the whole granting program is biased against elderly people. One of my students, DeVita, when he was director of the cancer institute, created a program called Outstanding Investigator Awards. The idea being instead of a basic scientist saying—you know—we did this and this, and that's brilliant; it's basic science—maybe we could look at their productivity. Instead of these fairytales that we're writing grants. When you want a grant, you write a fairytale. We know this and we do this and we did these experiments. If the fairytale appeals to other fairytale writers, then you get a grant. But if it's going to be useful, it's supplied by private practice.

So I had two big PO1s, millions of dollars. I had the Clinical Center grant. Over the years, particularly the last decade, all those grants I lost competitively. In about a half of them, I actually got the word—I still keep the reviews. You get anonymous reviews. "Dr. Freireich is very accomplished and very famous. He's had a great career. But maybe it's time for young people." So when I began to see age bias appear in my grant reviews—oh, the time came for the renewal. The arrogant SOB at the medical school, Jon Tyson, wrote a memo to Dr. Tomasovic saying, "I don't think we should go together because they are so different." He felt that our program was a drag on his program, so if he could write his own he would get renewed. So he withdrew, and we were to write our own. So I said, well, if we're going to write our own, it's not going to be Freireich as the PI. I had to recruit someone really smart. So I found one of my ex students who is really a genius, Dr. [Razelle] Kurzrock.

Tacey Ann Rosolowski, PhD

1:34:27.9

I'm sorry, I missed her name.

Emil J Freireich, MD

1:34:35.0

K-U-R-Z-R-O-C-K. First name is Razelle. She's from Canada. She came here after she'd worked with Carlo Croce in Philadelphia, so she had a laboratory basis, but she was a clinician. When she came to DT she became a physician scientist—outstanding. She was teaching in the course, and she and I had regular interactions. I came to trust her as my 2IC. So I asked her to do the grant, and she did. And the outcome was that we got funded and he didn't. And Kurzrock became head of the K-30 program.

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She is totally outstanding—totally outstanding in every way. She's become head of her own department. She runs the Investigational Therapeutics. She runs this program—the Physician-Scientist Program—and had done an outstanding job.

The problem is that we still have Dean Stancel. So we push the dean every academic year. See, the difficulty is that this program is what's called a 'small p' program. So when you look in the catalog, it's not listed as a program and it doesn't have a faculty. It's listed as miscellaneous. So if a student wants to do patient-oriented research, he has to know somebody who tells him how to get into it, because you can't get into it through the graduate school. The catalog and all the administration, which are all PhDs, make every effort to divert graduate students from going into this program. But it's very popular with the PhDs. They love it. In our program, two-thirds of the students are PhD students because they want to do patient-based research. They want to have their research applied to the clinical problem. They're motivated by cancer.

So we face this tremendous impediment, and every year we go to Dr. Mendelsohn and we go to Dr. Kripke and we go to the dean and we go to the executive committee and we went to the graduate education committee. They all say, yeah, it's very good. No one can budge the dean. So when Dr. DuBois became director, we went to DuBois. He said, "I got a good idea; let's get a consultant." So we invited Gordon Williams, who was the head of the K-30 program at Harvard, who has one of the best programs in the country. He's a founder of the Clinical Research Association. He's a very eminent guy, endocrinologist. He's a friend of DuBois. Then we had him down to look at our program.

At Harvard, his program is a program, and ours is just as good as his and it should be a program. He met with Dean Stancel for an hour, and when he came out, I said, "How'd you do?" He said, "No progress. He won't budge." He met with Dr. DuBois, and Dr. DuBois knows all about that recommendation. We have it in writing that patient-oriented research is a legitimate graduate school program and should be a 'large p' program.

So the dean has just retired, but he's still acting dean. We're hopeful that once we get Stancel out of the way, it will be approved. It's been approved by all the committees in the graduate school except the dean. And it's really tragic that one person has that much power. It's just not right, but I think the reason he has that much power is because the Graduate School [of Biomedical Sciences] was taken away from MD Anderson and put in the Health Science Center. Then Dr. Mendelsohn was able to negotiate us back into it. Sixty percent of the faculty and students are all from MD Anderson. The reason people come to our graduate school is not the Health Science Center. That's a backward institution. They come to be at MD Anderson, the number one cancer center in the world.

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So finally, the dean agreed that we jointly issue the MD/PhD degree. The MD/PhD program is still totally based in the medical school. We pay for half the MD/PhD students. I served on the committee for five years, but they—Dr. What's-her-name that runs the program, she decides—it's totally based in the medical school.

So we have a problem with the graduate school establishing. I have advocated from day one that we get the graduate school back. We should have our own graduate school—MD Anderson's—just like Clark. Clark got the graduate school approved when we were just a dink institution. Now we're the number one cancer center in the world. We certainly should have our own graduate school. So we've got the tail wagging the dog. The Health Science Center runs the graduate school, and we're the faculty. It's really ridiculous. It depends on how the new dean goes. Dr. DuBois and Dr. Tomasovic have assured me that to form our own graduate school is just too expensive, so they don't want to do it. They want to stay with this joint arrangement, but we have to have more influence on the graduate school, so that's where it stands.

We have a committee that runs our graduate program. We have two or three PhD's on it who are very good. They are translational scientists. We have a couple of good MDs. We've graduated about fifteen or twenty masters, PhD programs in the decade, and Kurzrock runs it. We fight every year to get upgraded to a full 'p.' We've been to everybody. It will succeed. It's just a question of time. Whoever the new dean is, is going to—he won't have the power that Stancel has because he's based in the medical institute. The new dean is going to be jointly appointed, probably more importantly here, and it should go smoothly. So that's a very important part of our training.

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Chapter 10

A: Professional Service beyond MD Anderson

Advocating for Patient-Oriented Research and Patients

Story Codes

D: Understanding Cancer, the History of Science, Cancer Research

D: The History of Health Care, Patient Care

D: Business of Research

A: Critical Perspectives

D: Cultural/Social Influences

A: Activities Outside Institution

Emil J Freireich, MD

1:34:35.0+

I think that once you come to MD Anderson, you get the Core Curriculum. You're now an academic graduate medical education specialist. If you do research during your training, which you should do, and you get grants and you cure disease, you should have the tools of science that are not taught to physicians. Physicians learn medicine rote. It's not an academic exercise. When you get internal medicine, they tell you how to do things. They don't tell you why you're doing things. So physicians who want to be scientists have to get science training. They have to take our courses. We have courses in clinical research. We have a course in translational research. We have physician-scientists who teach the students and whose labs they work in. So that's going to happen, and then we'll have a complete graduate medical education program. We'll be able to train outstanding specialists or practitioners. We'll be able to train academic scientists who do their practice science-driven. We're going to train physician-scientists whose practice is controlled.

Every doctor on our faculty here practices like NIH. They have their own thing, and they know what to do. Dr. Wood does renal surgery. He's the best in the world, but that's all he does—renal surgery. He's a scientist. So the physician-scientists have to control their practice, they have to focus their research, they have to understand disease. So they have to have those tools, and they get that through the Physician-Scientist Training Program. So you get the core curriculum. Then you do your research. Then you get your PhD degree. Now you're on the faculty. Now you're a professor.

Tacey Ann Rosolowski, PhD

1:43:46.1

The three legs—the three legs of the stool.

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Emil J Freireich, MD

1:43:49.4

And it's going to begin here at MD Anderson. That's what keeps me going. Every year, I get a note from the PRS Retirement Board, which says, "Freireich, you can retire at sixty percent of your salary at thirty years. And your salary goes up, because your optional retirement program kicks in. So if you retire, you're salary will go up." So I go home, and I tell my wife, "This is serious business. If I keep working, I'm losing money." She says, "You have to do what you want to do." So I'm now in my forty-sixth year.

Eventually they're going to fire me because I'm useless, but I really enjoy this phase of my career where my focus is on graduate medical education. I still fully participate in the leukemia research program. The Department of Leukemia is run by one of my students, Dr. [Hagop] Kantarjian, who is an absolute genius. He's like any great professor. You know, you train students who are better than you are. He's better than I ever was. He is really brilliant, competent, energetic, a wonderful person. The Leukemia Department is the best in the world, so I'm very proud to just hang in there. I do a little research on white cells and stuff, but I hang in there. I attend all the teaching sessions. I interact with all of our fellows, because the fellows who come here in medical oncology, they have to decide if they're going to be nephrologists or hematologists or leukemia. We have to attract the best and the brightest from the medical oncology program into the leukemia program. This year has been a very good year. We attracted two really super guys. I play a role in that because I participate in the teaching programs and I try to stimulate the research and I—so I participate in all the departmental activities.

So we have five hours of departmental activities a week. I participate fully in all that. Then I still have—I keep a few patients that—I don't take new patients. I don't attend on the hospital service, because I have a policy that if you're in the hospital, you have to see your doctor seven days a week. I don't work seven days a week.

I had a fatal heart attack in 1987. I was saved by being on the TIMI trial at Methodist, and then I had a bypass by a brilliant surgeon. I've just been very lucky. I'm way outside of the ninety-five percent confidence intervals for survival. But fortunately, my brain still works, so I keep working.

Tacey Ann Rosolowski, PhD

1:46:53.1

And you've been travelling too.

Emil J Freireich, MD

1:46:55.8

Well, I'm going to do less and less of that. Traveling is getting so painful. I went to Dubrovnik, my wife and I. It took us two days to recover. It's thirty hours on a plane. You can't sleep, eat. Who needs it? The days of face-to-face meeting are over. It's too easy to communicate

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electronically. I can talk to anyone in the world in ten nanoseconds. It's nice to have collegiality face-to-face, but the way the world is, in my new world, the work comes to MD Anderson. I don't have to go there. I don't go to Japan anymore. If they want it, they can come here. I decided after this trip to Dubrovnik that I'm not going anywhere. I can travel domestically. I go to Chicago, but I'm not going to go around the world. It's just—it's not worth the effort. It's not worth the time. I spent eight days going to a conference. I learned a little bit, but I could have read the abstracts in one night. And everybody knows me, and I know everybody, so I'm better off thinking.

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Tacey Ann Rosolowski, PhD

1:48:17.2

Can I ask you some questions about those—?

Emil J Freireich, MD

1:48:19.6

So I don't like traveling. Dr. [Michael] Keating [Oral History Interview] is the opposite. He likes to travel. He's gone all the time. Dr. Keating is a person who depends on human contact for him to think. I'm a person who thinks—pencil and paper and Internet. Go ahead, your questions.

Tacey Ann Rosolowski, PhD

1:48:41.2

I wanted to ask you about some of those national and global organizations.

Emil J Freireich, MD

1:48:46.0

Oh, I want to tell you about [Lawrence] Einhorn. So the way that happened is—

Tacey Ann Rosolowski, PhD

1:48:48.4

So this is the Association for Patient-Oriented Research?

Emil J Freireich, MD

1:48:55.2

Yes. More and more—I told you about all the geniuses at the NIH. I actually liked them. They're all nice guys, but they just don't know anything about research. They're lab guys. Fidler is a pet peeve of mine because he gets so much attention, and LeMaistre had a thing for him. They gave him a medal and now to win the Nobel Prize, the president of the AACR, and he's famous. It's all good research, but it's just lab research. It has nothing to do with cancer. That's what I call phenomenology. You want to study goldfish physiology? You get a Nobel Prize for studying immunity in goldfish. That's wonderful. As a society, we can afford that. But it hasn't got anything to do with leukemia.

So 600,000 Americans are going to die of cancer in the United States of America. Why is that? Because we're not working on cancer. We can cure cancer; we just have to do it, but Fidler is not going to do it; physician-scientists are going to do it.

So the geniuses—the money, which began—the grant program began as clinical research. Eventually, the basic scientists said, "Hey, there's money." The peer review committees that review grants are unanimously eighty to 100% laboratory scientists. There is not a single study section at the Cancer Institute where the majority are physician-scientists—not one. So all the money flows to Fidler, clinicians.

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Okay, so if you're working in a university and you want to be chairman of the department of medicine and you have to do research and you have to publish, it means you have to get money. The only way you can get a grant is do lab research. So the smart guys at the medical school say, "I'll do a post doc with Fidler, shoot stuff in mice, write a grant, and I'll get money. Then I can get some fellows, I can write papers, I can get elected to the National Academy of Sciences." See, I'm not in the National Academy of Sciences—Fidler is and Mendelsohn is. No doctor ever gets elected. Only Larry Einhorn—he's the only one. He nominated me for the National Academy of Sciences, and I was rejected, passed over by some guy working in a lab.

So the young academics realized that the only way they can get promoted to assistant professor and associate professor is do lab research, so they did lab research. You have to go where the money is. The reason there is a Cancer Institute is that Congress put money there. Otherwise, no one would work on cancer.

Well, I told you that science in the United States, in medicine, began with the Association of American Physicians. These were professors who trained in Europe and came back and they learned European science and they learned physiology and so on, so they formed a scientific organization. All the professors, they published all the papers. They trained all the doctors. But the medical schools had very little science and research. That was just done by the professors. So if you want to become a professor in the United States, you could either go to Europe or you could study with a professor, and if you studied with a professor, you learned to do what he does and you write papers and you do experiments with mice and test tubes. The first thing you know, you might or might not get a job. But you need a society where you can become presentable. So in 1914 or 1920—I don't remember the exact date—these young associate professors formed another society—The American Society for Clinical Investigation. To get into the Association of American Physicians, you had to be a proven leader/professor, so they were called the Old Turks. To get into the Society for Clinical Investigation, you had to be a Young Turk. You had to be an associate professor or wanted to be a professor who is doing lab research, publishing, and getting ahead in academia.

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When I was working at Boston University, I got elected as a Young Turk when I was—whatever year that was—'65. I was forty. You have to get to be a Young Turk before you're forty-five, because if you can't produce before forty-five, you're never going to be an Old Turk. So how do you get to be a Young Turk? Well, you have to do research, and if you're doing research and you have to have a place to present it and you'd like to go to the meeting—and the Young Turks and the Old Turks met together in Atlantic City. They formed a thing called the American Federation for Clinical Research, the Young Squirts. And the Squirts formed their society about 1930, and we went along happily that way until the late '50s, and the three societies met together back to back. So it started with the Squirts, and the assistant professors presented their papers. The Turks and the Old Turks could hear their papers and could tell if they were good, and they criticized them and helped them get ahead. Then the next day, we go to the Young Turks, and they presented their papers to the Old Turks to prove that they can get ahead. Then there's the AAP, where all the professors talk about how great they are, and they actually have an honorary luncheon and so on. And all of this is honorific.

Now, to get into the Squirts, you only need to write one paper, so there were lots of Squirts. To get into the Young Turks, you have to make it under forty, you have to—only ten a year or something get in, so it's an honor if you're a Turk. But to get into the Old Turks, that's really an honor. You have one out of forty get in, and that's a lifetime badge.

So I climbed the academic ladder the usual way. I was a Young Turk at Boston University. When I went to NIH and I wrote my papers up about white cells—good stuff about mechanism of anemia—laboratory stuff—nobody ever benefitted from it—I got to be a Young Turk. Then I started publishing on leukemia, and when I came here, Rulon Rawson, who was our dean, recommended me, and I got to be an Old Turk. So I have all these credentials.

So the Squirts realized that to be a Young Turk they had to publish. In order to publish, they had to get grants. In order to get grants, they had to do lab research. So the societies—the Squirts—were the first to become a basic science society. They talked about clinical things, but basically they did Fidler stuff—mice and test tubes and chemicals. Gradually, the Young Turks became a basic science group. Gradually, the Old Turks became a basic science group. I went to every Old Turk meeting from the time I got elected in '65, and every year the speaker became more and more lab oriented. The last one I went to, about five years ago, the guy who is now the NIH director, the famous—cloned all the genes—he was guest speaker at the Old Turks. He didn't even have an MD.

So these societies became basic science societies. Well, if you want to move up the academic ladder and become an assistant professor or associate professor, this is the route you have to go. Well, the Young Turks decided that the way to get grants was to work in with the federated societies. These are the basic science societies—the science for physiology, for biochemistry, for pharmacology—all the basic sciences together in what's called the federated societies. So the

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Squirts left the clinical meetings and moved to the federated societies because that's where they had to present their papers in order to get grants. They had to do lab work.

Tacey Ann Rosolowski, PhD

1:58:59.0

That whole clinical focus has just disappeared.

Emil J Freireich, MD

1:59:05.9

This is a catastrophe for the US. This is even worse than the FDA. It's the same problem. You see, things always occur when there are crises. If there's no crisis, the natural trend is to go back to stability, and stability is no innovation. The way you can be stable is everybody does laboratory research, no upset. The money is distributed. It's all peers. The patients die of cancer.

So the AFCR left. Now, there are a handful of Neanderthal physician-scientists who didn't like that, because they didn't want their Young Squirts training in their departments to go to the federated societies. So the guy I mentioned, Gordon Williams, was the one who wrote an editorial in the Young Squirts' newsletter, and he said, "If the Squirts have left clinical research, we need to form a society for the Young Squirts who do clinical research." I read that editorial, and I said, wow, here's my man. So I dashed off a letter to Gordon Williams, and I said, "I am totally in support of this. We ought to start it immediately." He said, "Wait a minute. I got this idea from a guy named Bud Robertson, who works at Vanderbilt."

So we were all Old Turks, and we said what we're going to do is we're going to meet at the next Old Turk meeting—[Dr. L.E. "Bud"] Robertson, Gordon Williams, and Freireich. We're going to invite people we know who are totally committed to this cause. So we invited—my recollection is—four other people, maybe five. One of them was Ed Ahrens, who had written the book—a very famous book on—I can't find it. He wrote a book which documented what I just said, but not just talk. He went to all the NIH review committees, laid out all the peer reviewers, laid out all the grants that were awarded, looked at all the subjects, and he wrote a book with all that data that demonstrated that the flow of money was progressively away from clinical research to basic research, and it was impacting health. We're not getting any clinical research.

So Ed Ahrens came and a guy named Jules Hirsch, who worked at Rockefeller University, a very famous guy in body composition. Then we had the lady who ran the grants program for the research centers, and so on. We had about seven people. We decided to form a society. We're the founding members of that society. We called it the Association for Patient-Oriented Research.

Now, we tried to get back with the Young Turks and Old Turks, but they didn't want us. They were already laboratory guys. So we have to start this whole progression over again. We have to get a clinical Young Squirt, a Young Turk, and an Old Turk Society of Patient-Oriented Research. So we started the society in what year?

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Tacey Ann Rosolowski, PhD

2:03:11.4

In 1998.

Emil J Freireich, MD

2:03:13.4

In 1998, and Bud Robertson was the first president, and I was the eighth president. It's struggling. It's not going to make it.

Tacey Ann Rosolowski, PhD

2:03:26.1

Oh, really? Why?

Emil J Freireich, MD

2:03:28.4

Why? Because they made the mistake of hiring a doctor to be the director of the cancer institution. It was a big mistake. You don't allow doctors—and that's when all this clinical stuff began. I'm blocking on his name right now. He was a radiologist from Hopkins. He became the director of the Cancer Institute. He made the diagnosis immediately that there was no funding for clinical research, so he created the Clinical Translational Grant Program. He got all the directors together. He embarrassed them into realizing that there was no patient-oriented research, there was no ongoing clinical research, and he convinced them all to set aside some money in a pot, under the director, to give grants to people who would do what they call translation research. You don't want to call it clinical research because they've got to do laboratory. But they are going to take all this Fidler stuff and apply it to patients. That's the paradigm. If we discover everything in a lab and we give it to some dumb doctor, he'll know how to cure cancer—translational research.

Unfortunately, he was a radiologist. He was not a doctor, but he was a radiologist, and he still believed in that paradigm. He didn't recognize the fact that the inverse is the case; the translation occurs from the patient to the laboratory because if you're not working on clinically relevant problems in the laboratory, you're working on goldfish. That's fine. You can cure goldfish. And we can have more healthier goldfish, but we're not going to have healthier people.

So the Translational Research Program was funded, and it was called Clinical Translational Science. All the academic institutions that were hurting because they couldn't get any money for clinical research became translational research centers, and all the people at APOR applied for grants, and they all got it. The guy who was the chair elect of APOR decided to form a society—the Clinical Translational Research Society. In the first round of the awards, the Health Science Center got—that shows you—you know—Tyson. Tyson became the thing. And Kurzrock's

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program, our K-30 program, got funded. Now Tyson is back in charge, so you know where that's going to go, straight down the pits. That was the end of funding for our clinical trial program.

Tacey Ann Rosolowski, PhD

2:06:35.7

The existing paradigm is really strong.

Emil J Freireich, MD

2:06:37.4

So the CTSA lasted one round, and then it went away. The guy who started all this, he's gone—I forgot where he went—and they got a basic scientist back in. We're back on track. The people who were in APOR have gone to this CTSA thing to get money. We met with the CTSA at our annual meeting to try—the main purpose is advocacy. We have to convince— We're never going to convince the National Institute of Health that clinical research is important. The only ones we can convince is Congress. Those are people who are sick, and their parents are dying of cancer, and they want progress. And when the Nobel Laureate goes to Congress, they say, "What progress have you made?" He says, "Oh, we've made great progress with translation." "How much money are you spending on clone research?" "Zero."

So eventually we have to—we have an advocacy organization, which includes these guys and APOR and the AFCR. They're coming back, the Young Squirrels. But there's not enough money. APOR has no money. We tried to make money from industry, but industry worries about conflict of interest. They can't mess around. So we have no money. CTSA program is fading down. The money has been reduced. The Young Turks are still getting money from the federated societies—the basic sciences. So we don't have the money. We have a lobbyist, but what could have worked did not, so we need another strategy. Leaders of academic medicine have to get to Congress, because it all starts with money. If Congress says—it's like MD Anderson. There would be no MD Anderson if the legislature didn't pass a bill to have a cancer hospital. Roswell Park is the same thing—the legislature—because the legislature represents the people. The NIH represents the scientists. As they used to say, you lick your own ass. You don't go outside your circle.

So APOR exists, but it's not functioning well. People aren't joining. We don't have any money. But the idea is right. So we're going to have to get to Congress some way. For right now, as we said in the first session, this country is—we're only worried about safety. We don't care if 600,000 Americans are going to die this year from cancer. They're just sick.

Tacey Ann Rosolowski, PhD

2:10:06.1

It sounds like you have to wait out—

Emil J Freireich, MD

Interview Session: 02

Interview Date: - October 6, 2011

2:10:07.6

What we're worried about is the economy, Greece, the stock market, safety, no drilling, no energy, clean air, clean food, no drugs. So we can't make any progress with the legislature, because the legislature— We had a bill in the legislature four years ago to get around the FDA squelching new agent development. It was called the Patient Rights Bill or something, and I was an advocate. We went to court. We won in regional court a couple of times. The general idea was that if a patient and his doctor want to undertake a treatment, the FDA can't stop it. Why should they? But the bill failed, not for lack of support, but for lack of priority. There were just so many things pressing on these people. Now it's can we even run the government. The guys elected to Congress have got to cut the budget. You can't talk about money for clinical research. They're trying to fight for money for NIH even. Can you imagine? Can you imagine reducing the budget for basic research? Fidler? My God! The guy who is director of the NIH goes to Congress and says, "We'll become a backwards society if we don't shoot cells in the tails of mice. Just cure people? That's terrible."

Tacey Ann Rosolowski, PhD

2:11:55.3

Dr. Freireich, we're almost at 11:30, so shall we—?

Emil J Freireich, MD

2:12:05.5

I've worn you out. I've still got a few topics I want to cover, but you've done very well.

Tacey Ann Rosolowski, PhD

2:12:11.1

Absolutely. We've done very well. Why don't we stop for today, and we can make another appointment and continue. The time is 11:25.

2:12:27.7 (End of Audio Session Two)