

Emil J Freireich, MD

Interview Session Two: 30 July 30 2001

Chapter 09

At the NIH: Studying Infection in Leukemic Patients [early 60s]

A: The Researcher;

Codes

A: Overview;

A: Definitions, Explanations, Translations;

C: Discovery, Creativity and Innovation;

C: Discovery and Success;

D: On Research and Researchers;

C: Professional Practice; C: The Professional at Work;

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

D: The History of Health Care, Patient Care;

D: Technology and R&D;

C: Patients; C: Patients, Treatment, Survivors;

Lesley Brunet, MA

0:00:03.9

Would you like to add a little more to your history while you were still at NCI?

Emil J Freireich, MD

0:00:06.2

Yes. We finished the clinical trial designs, the quantitative clinical trials, and the first studies of combination chemotherapy. Then we went to the platelets. Once we reported that when platelet transfusion policy was implemented hemorrhage was substantially eliminated, infection remained the major supportive therapy problem. So we got the idea that we could use the same quantitative strategy to approach the treatment of infection that we did with the treatment of hemorrhage.

Lesley Brunet, MA

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What kind of infections were they?

Emil J Freireich, MD

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Well, they're all kinds of infections—bacterial, fungal, everything. So we did a study to find out what increased the susceptibility of infection, similar to what we did with the platelet count. It turned out that the granulocyte concentration, the so-called neutrophils, were directly related to the risk of infection, and this became another citation classic. Gerry Bodey, who was then a fellow, was the senior author. We showed that there was a quantitative relationship between the amount of neutrophil suppression and its duration and the likelihood that you'd have an infection. It was similar to platelets, so we said, "Well, now all we have to do is replace neutrophils, like we replaced platelets." Well, platelets were tough, because the red cell, which is the classical allogeneic replacement, has a life span of 120 days, approximately 4 months. The platelets have a life span of about 10 days. So it's obvious that the replacement was tenfold magnitude. What we knew about the physiology of the neutrophil was that it had a half-life, as measured with radioisotopes, of about 6 hours.

All of the neutrophils in your peripheral blood are essentially replaced every day. So we thought about that a minute. We said, "Well, in order to elevate the neutrophil count, even transiently for a day in a recipient who had none, it would require all of the neutrophils in the peripheral blood of an adult." We did a little bit of calculating, and we figured that, like we did with the platelets, the exchange transfusion, if we removed a unit of neutrophils, put back the red cells, and kept doing that, that if we processed 2 blood volumes, which is about 10 liters of blood, in a donor, we would remove 90 percent of the neutrophils in the blood.

Lesley Brunet, MA

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Does that affect the donor?

Emil J Freireich, MD

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We were going to find out. We didn't know yet. We began to try to separate neutrophils, and we tried a lot of different ways, like electrophoresis, centrifuging, and capillaries. I was fussing with it when one day a gentleman walked into my office named George Judson. George Judson was an engineer who worked for IBM, International Business Machines, which was big. We're now in 1960. Mr. Judson's son had chronic myelocytic leukemia and became a patient at NIH. His doctor was Jerome Block, one of the physicians on our faculty. Mr. Judson was very concerned about his son, so he said to Dr. Block, "Is there any way an engineer could help in treating my son?" Dr. Block thought about it a minute, and he said, "There's this crazy guy up on the twelfth floor who's trying to build a machine to separate neutrophils, and maybe you can help." So he appeared in my office, and I sat down with him and said, "You can help me. Here's what I need. I need a machine that will do the following 10 things," and I wrote down 10 things. Mr. Judson looked at it, and he said, "Well, I've never worked on blood, and I've never had anything to do with biology, but I'll see what I can do." So he disappeared.

About a month later, he reappeared with a machine. He'd made a centrifuge that he felt would work. He had worked on jet engines, so this machine had a jet-engine philosophy; it flipped the blood. Jet engines work on the basis of the oil being thrown at such a velocity that it lubricates everything. We called the blood bank, got some blood units that were discarded because they

were serology-positive or they were outdated, and we began to work in the lab with these things. Mr. Judson and I worked in the lab for about a year, and it was really fun. Mind you, this is blood that's hepatitis-positive, serology-positive, but there was blood everywhere. We were one catastrophe after another.

Lesley Brunet, MA

0:06:02.3

Because it came out of the machine when it ran?

Emil J Freireich, MD

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Because we had to design pumps. We had to get plastic tubing. We had to get junctures that worked. We had to get a seal. There was a lot of technical development to be done.

Tom Frei was interested in this, and we went to Gordon Zubrod and said, "Look, here's our prototype machine. We're going to show you how it works." We had borrowed a pump from the Heart Lung Institute. We hooked up some units of blood, and we showed him how it worked. We recovered some neutrophils, and he thought this was promising. He said, "Fine. We'll present it to our committee." Then we got a grant. We set a contract with IBM to build us a machine that lived up to these specs. We had stage one, but he needed some help, so we got a federal contract. IBM assigned 2 brilliant engineers to work for Mr. Judson. They were 2 young guys named Vic Kruger and Bob Kellogg.

IBM had a policy that when they sign a contract, if they don't deliver on the estimated date, their project engineer is fired. It was a 2-year project, and when the 2-year anniversary came, sure enough, there was a machine. It appeared in my lab, we ran a patient, and it had a number of problems. The pumps didn't work. The seals didn't work. So I was frustrated, and I told Mr. Judson, "Back to the drawing board. It doesn't work. You've got to fix the seals. It doesn't work." IBM doesn't tolerate that. "This is a machine that was built to your specs, and it works."

So IBM then had a big whoop-de-do with the federal government and with NIH. They decided that they did, in fact, live up to the contract, and since I was the only one objecting to the machine, I was fired. The instrument was moved to one of my worst enemies, a guy named Seymour Perry, who's now dead. Seymour Perry was an opportunist who never discovered anything, but he was a kind of a bureaucrat. He did okay, and he smiled. So they turned it over to him and fired me. It was my worst confrontation with my dear friend, Emil Frei.

Lesley Brunet, MA

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They actually fired you?

Emil J Freireich, MD

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Yes. They changed the project officer. I couldn't work on this project anymore.

Lesley Brunet, MA

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So you were still there but not working on that project?

Emil J Freireich, MD

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Correct. I said, “How can you dare do this? It’s all my idea. It’s my work. This is obscene to take it away.” I was told, “It’s only temporary. We’ll get it back.” That happened late in ’64.

That was bitter, and we were very upset.

Chapter 10

At the NIH: Vincristine, a Cure for Childhood Leukemia, and VAMP

A: The Researcher;

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Emil J Freireich, MD

0:09:08.7+

That was the end of the work on the white cell machine, so we did the platelets. We did combination clinical trials. Now came the big breakthrough, the thing for which I get all the prizes, and that is curing childhood leukemia.

We'd worked out the quantitative stuff. We'd worked out the combination chemotherapy of 2 drugs. One day Irv Johnson from Eli Lilly, who has been my lifetime friend and ended up becoming a vice president, appeared in my office. He said, "We have a drug that I have discovered that we want you to test in children with leukemia."

The drug was a drug called vincristine. Irv Johnson was a little bit like me. As I said, there are people who do regular science. They work and work, they get grants, they get papers, they get famous, and they get to be professors, but they never benefit anybody. Then there are people who are just a pain in the butt who discover things and move the field forward. Irv Johnson was a mover. He was working at Eli Lilly. The vinca alkaloids were taken up by Eli Lilly because they were the major manufacturer of insulin, and insulin was their big product. There was some folklore that the [Vinca rosea](#), which is the periwinkle plant, was used by natives to treat people who had sugar in their urine. They diagnosed the sugar in the urine because the flies like the urine from the people who had this disease; they got very sick. They found out that if you gave them periwinkle they got better. So they decided to pursue that lead. They were trying to treat diabetes with these extracts. It turned out the periwinkle plant manufactures a thousand very complex alkaloids.

Lesley Brunet, MA

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Is this the same vinca that we plant in our yards?

Emil J Freireich, MD

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Yes, Vinca rosea. That's periwinkle; it's the same plant. It makes a thousand alkaloids. We don't know the evolutionary significance of those alkaloids, but that's true of all the natural products. Virtually all chemicals in antibiotics have come from natural products. There's some reason in nature why they're there, but we don't know why. Why does the mold make penicillin?

They were working on Vinca rosea, and Irv Johnson noted that some of his mice had very low white counts. He got the brilliant idea that this drug might be good for leukemia, so he began to treat leukemia in mice. Sure enough, they found that the crude extract did depress the white counts. Then they transplanted leukemia in identical mice, and they found that it worked. But there was a mystery. It didn't work in the leukemia that we used as our prototype model for developing drugs for man, leukemia L1210. It didn't work at all in L1210. Irv Johnson decided that one leukemia was not enough to screen the compound, so he had developed a number of mouse leukemias, and he had a panel. It turned out the mouse leukemia, which we use all the time today, called P388, was extremely sensitive to the vinca alkaloids.

Then they purified these things. There were tons and tons of alkaloids. They got down to one called vincristine, which was present 1 milligram per carload of periwinkle. This was an extremely low concentration but very specific for leukemia. They decided to take it to the clinic. They did a little clinical trial at their small hospital, but they needed a big experience, so they came to NCI. I looked at the data, and I said, "Great idea. Let's meet with Dr. Zubrod."

Lesley Brunet, MA

0:13:57.9

What year was this?

Emil J Freireich, MD

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It would have been about 1960 or '61. So we went to Zubrod, and Zubrod said, "Well, Dr. Freireich, we've spent a million dollars to show that L1210 leukemia predicts. Every drug that works in man works in L1210 leukemia, so this drug is a total bust." I said, "But, Dr. Zubrod, how are we ever going to discover a new drug if we stick to the paradigm that we believe that all the drugs we know came out of L1210? Maybe there are drugs that we don't know that will not work in L1210, and here's one." I said, "Besides, I've got children on the ward right now that are dying, who have no hope for living. What harm is there in doing it?" Zubrod said, "Freireich, on your personal recognizance, I'll let you study this drug."

So we went ahead. The first 11 children we treated, 7 of them achieved a complete remission with a single drug. We had kids in total coma who woke up and dying children in remission. It was unbelievably dramatic. We published this paper very quickly. Myron Karon, who was then a pediatrician working on leukemia service, was the senior author. We published this paper,

“Vincristine Works in Leukemia.” So that was something. I sat down one day, and I said to myself, “Look, we’ve showed that 6-mercaptopurine and methotrexate both are [myelosuppressive](#). If we give them together, it’s better than giving them in sequence.” That’s interesting.

I have to tell you about one other protocol. I’m missing a step here in logic, and I’ll come back to it: Protocol 3. We know that if we add prednisone to this combination, we get the best results of all, because prednisone is not myelosuppressive, and if we add prednisone to 6-MP, you get the full effect of prednisone in 6-MP. It’s additive in terms of percent response. We add prednisone to methotrexate; it’s additive. Now we add vincristine. Vincristine has no myelosuppression. The toxicity was neurotoxicity. So we could give a full therapeutic dose with no myelosuppression and no steroid effect. So instead of screwing around one at a time, what if we went for the home run? What if we give 6-MP and methotrexate and guess that if we add prednisone we get the full effect and guess that if we add vincristine we get the full effect? We’re going to get 4 drugs at a time. Everybody thought I was insane. We had just fought the battle of 2. Now we were going to 4.

Lesley Brunet, MA

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How would you be able to tell the difference between the prednisone and the vincristine?

Emil J Freireich, MD

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They have totally different dose-limiting side effects. They have different biologies. Prednisone affects the blood glucose. Vincristine neurotoxicity and prednisone had totally different mechanisms. We knew that this arrests cells in metaphase. Prednisone causes lysis in T cells. We presumed that they would be additive, not synergistic.

We didn’t know what to do with this, so I wrote a protocol. We called it [VAMP](#). This is the most famous acronym in the history of chemotherapy. It was the first time anyone had used these acronyms. Now they’re universal. All chemotherapy is identified by initials. VAMP was vincristine, amethopterin, mercaptopurine, and prednisone. And not only did we have the idea of combining all 4 drugs at one time, but we had a number of really important new ideas.

First of all, when I proposed this, Dr. Zubrod was very upset. He said, “This is ridiculous. We know how to do the randomized trials. You have to do this first and this first. Then you have to do this.” I said, “Dr. Zubrod, I’ve got children dying on the ward. We just got this vincristine. We’ve got to move ahead aggressively.” “Well, everybody worries about us experimenting on children.” “But these children are hopeless.” So we went through this with the IRB, Dr. Zubrod, and Dr. Mider. They finally allowed me to treat brand-new, untreated children with this regimen. This is the most famous chemotherapy in the history of chemotherapy. We treated 17 consecutive children, and 16 of those 17 children responded immediately, in 2 or 3 weeks.

I said to myself, "Wow! This is something." The purpose of combination is to avoid resistance. Could we possibly, with this enormously potent combination, cure leukemia? No one had ever proposed that any systemic cancer in man could be cured. There was only one systemic cancer that had ever been cured, and that was choriocarcinoma, which is an allograft. It was cured by M.C. Li. It's a disease of the placenta, not of the host, so immune and transplant. Leukemia is a childhood disease; it's a cancer, widespread and metastatic. Everybody thought I was insane. "You can't cure cancer. The only thing is radiation and surgery."

It was a long battle. We had meetings with the parents. I had to get the parents to agree to try this. "Look, I can give you chemotherapy, and they're going to relapse. Then I'll do it again, and they're going to relapse. What if we go all out? It's like a transplant. What if we take a chance? We might kill them with this combination, but we're going to try to cure them." So these 17 children, instead of just putting them in remission, while they were in remission we treated them aggressively. And then we stopped. You can imagine the tension on the ward. Every day, all the parents were asking, "How is Joe?" "How is Sam?" "How is Fred?" We had these 17 children and all their parents. I used to meet with the parents every Tuesday and Thursday to be sure they didn't die of anxiety. By the time I left NCI, we published a paper and clearly made the claim that we had cured leukemia. It was based on what's called the [Kaplan-Meier plot](#), which makes a projection based on the available evidence. Of course, that prediction turned out to be true in 2001. This was 1964.

Now, I told you about Protocol 1 and Protocol 2 in the cooperative group. We started the first cooperative group. We did a protocol that I senior-authored. It was my most famous publication. Protocol 3 is another citation classic. In Protocol 3, the idea was based on this: We said, "Look, we have 3 drugs that work—6MP, methotrexate, and prednisone. We've learned that if we give them together, it's more effective than giving them in sequence. But we don't know what to do for the children in remission, because once the blood and bone marrow is normal and there's no leukemia, if we don't do anything, it comes back. If we treat them continuously, we prolong remission; but it always comes back."

So we got the idea, which is now universally used in chemotherapy, of minimal residual disease. We think they have residual leukemia, and it was based on the fact that the leukemia that came back was identical to the leukemia they had before, so even though we couldn't measure it, we did some fancy calculations and we wrote a paper on it. Based on the rate of regression, we did an estimate. We counted the number of cells per gram of tissue, we estimated how many you kill with each treatment, and we did a number of calculations. We figured out that if this kills x number of cells, and we did it 4 times, and it was just as effective each time, we'd get to zero.

So we said, "The thing to do is, we have to treat these children when they're in remission." So we designed an experiment; it was the most beautiful experiment. It was a prospective, randomized, placebo-controlled trial. We treated all children in the cooperative group with prednisone. That gives you about a 60 percent remission rate. These children are now free of disease. Once they were free of disease, we randomized them to 2 treatments. Treatment number 1 was to give them 6-mercaptopurine when there was no disease. The other was to receive a placebo. The difference in these 2 things was dramatic. The reason this study was so

important and is a citation classic is it was the first adjuvant therapy. It was the first time we treated people who had no disease on the assumption that they had disease and proved it, based on the duration of disease-free period.

This was the first adjuvant chemotherapy study in the world. The median duration of remission with 6-MP maintenance was about 10 months or so. By that time, 95 percent of the children on placebo had relapsed. Now, the children who received placebo and relapsed were subsequently treated with 6-MP for induction so that the overall survival was not terribly worse; it was almost as good. It was a little bit better to give the 6-MP during remission.

While this study was going on, vincristine was brewing, so now you can see the logic. We went from combinations of 2 drugs. We proved that treatment when the patients were in remission could prolong remission. We now had a powerful 4-drug combination that induced remission, and we said, “Wow! What if we gave that as an adjuvant? Could we cure children?” And we did. So that’s the other important step.

Chapter 11

Leaving the NIH for a Turbulent Research Environment at MD Anderson

A: Joining MD Anderson/Coming to Texas;

A: Joining MD Anderson;
A: Personal Background;
C: Portraits;
B: MD Anderson Culture;
B: Working Environment;
B: Growth and/or Change;
C: Leadership;
D: On Leadership;
B: Obstacles, Challenges;
B: Institutional Politics;
B: Controversy;
B: Critical Perspectives on MD Anderson;
B: MD Anderson History; B: MD Anderson Snapshot;
D: On Texas and Texans;
D: Cultural/Social Influences;

Emil J Freireich, MD

0:17:21.1+

We're now in '64, and things are going along famously. I had the biggest and best pediatric leukemia service in the world. We were internationally famous. Everybody was following our lead. People came to learn how to do platelets, how to do white cells, how to do antibiotics, and how to do combination chemotherapy. We were really rolling. But we were too successful. There are 2 ways to get fired: hopelessly ineffective and too successful. You've got to be careful not to be too successful. We had taken over another ward. Our practice was booming. We had people calling us 10 times a day, "Please take my children." We had no room, and we had no resources. We were bursting at the seams.

Now came a tragedy. Dr. Zubrod and I had a third-party relationship. We're like cousins. Frei was like his son. He trained him, and he brought him from St. Louis. So the hierarchy was me, Frei, then Zubrod. Dr. Frei, my dear friend and someone I will always trust, a wonderful person, married a woman who was a very abnormal person. She was not under psychiatric care, but she was an extraordinary person. Everybody loved her, but she was unpredictable. This was his first wife.

When Deanie and I came here in '55, Liz and Tom Frei had us to their house the day we arrived. For 10 years we were social and personal friends. They had 5 children. Their children were poorly cared for. Their household was—what's the word?—casual. There was never food,

everything was dirty, and they slept on the floor. My wife is the inverse. We're meticulous. Or she is meticulous, and I have to go along with it. She runs the house. I'm at work all the time. When we went to their house, it was very uncomfortable. We didn't go very often, and they didn't come to our house, but we were friends.

Liz Frei had a sister who was married to an alcoholic veteran in Boston, and she had 6 children. Her husband was cared for at the VA until he finally died of tuberculosis. He left her with no money and 6 children. Her only living relative was her sister, Liz. Dr. Frei, being a wonderful person, said, "No choice." They moved in with him. So now he's got a 3-bedroom, little matchbox house with 11 children and 3 adults. The sister is like his wife, worthless. She's a very passive, ineffective person. It was a houseful of children raising themselves.

Lesley Brunet, MA

0:29:31.5

Incredible stress.

Emil J Freireich, MD

0:29:38.2

It was incredible financial stress, because we worked for the federal government. I was making \$5,600 a year. Dr. Frei was a big wheel, Chief of Medicine. He's making maybe \$6,600 a year, but he couldn't possibly raise this family with that income. Enter the world's greatest entrepreneur, the builder of MD Anderson Cancer Center, R. Lee Clark

Lesley Brunet, MA

0:30:04.3

I was wondering when you first met him.

Emil J Freireich, MD

0:00:03.7

Now we're in '64, and MD Anderson has been here for 18 years. Dr. Clark had a big vision. He built a clinic based on the Mayo Clinic model of excellent patient service. He built a basic science organization because he felt that just practice wasn't enough. There had to be clinical epidemiology. He hired Eleanor MacDonald, and he hired Felix Haas. He actually applied to UT to get a graduate school. He convinced the legislature we needed a public health school and we needed a medical school. He wanted a complete medical center in Houston. Mind you, Galveston is the home base for the whole UT System. Here's an upstart in Houston that wants to build a medical center 50 miles away from Galveston.

He'd been watching things going on at the NIH. As I mentioned, the 10 years from '55 to '65 revolutionized clinical research in the United States. The people who trained at the NIH, all these young men who came there, went off to medical centers all over the country and built clinical research units. The federal government began making grants to these institutions. Eugene Braunwald, my personal friend, ended up chairman of the Department of Medicine at Harvard. The NIH alumni were chairmen of medicine all over the country. Dr. Clark is watching all this, and he says, "I've got to get some of this federal money." So Bill Russell

wrote a grant, and he got a big grant to do laboratory medicine. He applied for a clinical research center, and he got funded. What he needed was some tiger from the NIH: Frei. “Guess what, Frei. I’m going to pay you a lot of money.” I don’t know what he actually offered him. I’ve never asked him. Dr. Frei looked at his social situation; he looked at NCI, which he adored; he looked at Zubrod, whom he adored; and he said, “Got to take it.”

One day there was an announcement. “Dr. Frei is moving to Texas.” We were shell-shocked. The entire NCI was shaken. Dr. Zubrod called in all his management people and all the bureaucrats. They had meetings after meetings. I was not in that circle. “What are we going to do if Dr. Frei leaves?” There was only one solution. Dr. Frei could not leave. So they made him a general, upped his salary, and he decided to stay. But that only lasted 3 months, and the reason it only lasted 3 months was purely social. I have asked Dr. Frei what changed his mind. He said it was strictly money. There were acute episodes at home. There was one day when he got called at work because one of his children was left in a supermarket, and he had to go get him. He just could not manage this household. Zubrod tried. They made Frei a general; that is the top. That’s as much money as the federal government can pay anybody, but Dr. Clark more than doubled it. So he had to change his mind again. I’ll never forget the day he changed his mind a second time. Remember, now they’ve done all this jostling. What’s going to happen if Frei leaves? Of course, the heir apparent to the empire was Freireich.

So during the first resignation, there was all kinds of jostling. I’m the heir apparent. All the forces were descending on Zubrod, but he was going to appoint me, nonetheless, when Frei left. We even had meetings to that effect. Then Frei stayed. Everything calmed down again. All the forces of evil were gone.

Then within 6 months he changed his mind again. David P. Rall, who was head of Pharmacology, who had another very unusual wife, an artist, had a traditional New Year’s party at his home. We all went there and got very high on martinis. At that party Dr. Frei said, “J, I’ve changed my mind. I’m going.” That was the first of January ’65. That was the only time in my life that I became so intoxicated on gin that I actually became unconscious, anesthetized.

Lesley Brunet, MA

0:06:17.0

Is this with joy or fear?

Emil J Freireich, MD

0:06:18.2

I was so depressed, because I knew there was going to be trouble. I don’t know what happened after that, but my wife told me that several colleagues put me in the backseat of my car, and she drove me home. I threw up all over my car. I was in bed for 2 days in coma. I was really anesthetized. I woke up, and I said, “What day is it?” My wife said, “You’ve been out for 2 days.”

But what followed after he resigned were, again, the forces of evil. Dr. Zubrod appointed Seymour Perry, my archenemy, to be head of the medicine branch. That was bad enough, but I

was still in charge of leukemia. After about 3 months, Dr. Perry called me to his office and said, "Freireich, I've decided to replace you with your student, Ed Henderson," who was my first post doc. He's the author of that book, *Leukemia*. Ed Henderson came to my office, and he said, "J, this is really terrible. I mean, I don't see how I can head the thing." So I said, "Well, it's obvious they want me to leave, so I'm leaving." He said, "Okay," but he promised me that he would carry on the Freireich tradition. The study after VAMP was a study called POMP. Then Dr. Frei started with Dr. de Vita to treat solid tumors, and he started the MOPP in Hodgkin's disease. So this was all going on, and Ed Henderson promised me he would continue those programs without change to all my protocols. We'd at least complete those studies with follow-up; he'd be first author. I said, "Fine."

I decided to leave, but I had to find a job. My personal adviser was Sidney Farber. Sidney Farber was the godfather of the NIH. He worked with Mary Lasker to get the money and create the legislation. So I went to Boston, I spoke to Dr. Farber, and he offered me a job. Then I talked to Tom Hall, my very good friend who was there. I got advice from a lot of people.

Then I got a call from Danny Bergsagel, who used to be here. He had left in '64 to go to the University of Toronto as the chairman of Medicine. He and I were very good friends and colleagues. He offered me a position at the University of Toronto, so I went up there. That was a very attractive position, because I would be clinical director and run the clinical research center.

Then, of course, Tom Frei calls me up and says, "Freireich, Houston is great. Dr. Clark is a great leader. The one problem we had at the Cancer Institute is we'd reached capacity. Dr. Clark has unbridled vision for the future of MD Anderson. He already has in hand the money to build the Lutheran Hospital Pavilion and the research building, and he's got all the things arranged so that we're going to be a complete health science center." The coordinating board for education for the state had approved a medical school for Houston. It was to start in '65. "So this is going to be a great place. Dr. Clark knows the future. You and I have spent our time talking about what we really want to do, which is to revolutionize medical education. We want doctors to be trained by scientists, not physicians."

You see, medicine at the turn of the century was an apprenticeship. You just followed around and did what a doctor did. But at the turn of the century, the Flexner Report made medicine scientific as a basis. So medical schools are dominated by PhD's who teach you science for 2 years, and then when you start your clinical years, you go around and follow doctors around and do what they do, like an apprenticeship. Sometimes you never meet a guy who discovers things. My ambition was to have a medical education where all the teachers were scientists who were doing research.

Dr. Frei said, "We could do it in Houston. Dr. Clark wants to do it. It's never been done in the United States. They tried it at Yale; it didn't work. You've got to come to Houston. You've got a great opportunity."

Dr. Clark appointed Dr. Frei the associate director for Research. So Dr. Frei was in charge of all research, clinical and basic. At that time, there was only one other associate director, and I think

that was Murray Copeland, who was in charge of educational things. So Dr. Frei wrote me a letter.

“Dear J, I am the associate director for Science at MD Anderson Cancer Center. We’re going to give you a chance to do what we did at the Cancer Institute. You’re going to build a clinical research center. You can have your own department, your own personnel, and your own grant money. We want you to build an institute within MD Anderson. Dr. Clark is totally behind it.”

Wow, what an opportunity! At NCI, I was in charge of leukemia, and Dr. Frei was in charge of the solid tumors. We had a guy in charge of the medicine branch, and then we had Dr. Zubrod. Well, Frei was now Zubrod, and I could be Frei. I wanted to finish up the MOPP, to cure lymphoma, to get into the solid tumors. That was hard, to turn down that opportunity. I came down here. I gave a seminar for Dr. Grant Taylor.

Lesley Brunet, MA

0:13:43.4

What time of year did you come down?

Emil J Freireich, MD

0:13:47.8

It must have been in the winter. I would say February. You have to understand that I was working 20 hours a day, every day. There was just too much going on. I just couldn’t contain myself. We were poor. We never went to a movie. We never went out to dinner. Whatever time I had, I spent with my kids. My wife worked to raise enough money to pay the rent. We lived in a little rental.

Lesley Brunet, MA

0:14:26.2

Your wife was working, too?

Emil J Freireich, MD

0:14:27.1

Oh, yes. We couldn’t afford a babysitter, so we took turns. I used to run home, she’d work night shift, then she’d come home, and I’d go to work. She did that for about a year. We were working very hard. After I got the letter from Dr. Frei, I came home from work one day, and who was in my living room? Dr. Clark.

Lesley Brunet, MA

0:14:58.4

Had you met Dr. Clark before?

Emil J Freireich, MD

0:15:01.1

Never. He had an aura about him that really attracted people. He was charismatic. He appeared in my house unannounced. “I’m Dr. Clark. Dr. Frei tells me that you’re very important to his

program, and we want you to come to MD Anderson.” He sat down in the living room in my little teeny rental house. My 4 kids all jumped on him. He had them on his lap. “Oh, cute kids.” He talked to my wife. “How are you, dear?” You have to realize, we’d come from Chicago, trained in Boston, and went to Washington, where things were still black and white. It was the South, but Texas? Good grief.

Lesley Brunet, MA

0:16:02.8

LBJ was president.

Emil J Freireich, MD

0:16:05.6

My wife thought that there would be Indians. She was terrified about going to the South, with all those rednecks and southerners. In 5 minutes she was in the palm of his hand. He had dinner at our house. She cooked something, he ate, and we talked. When he left, my wife said, “We gotta go to Texas. He’s a great man.” That’s typical Clark. He was impressive.

So I called Dr. Farber, and I said I had this opportunity. He said, “Well, I think you’re doing the right thing.” I called my friend, Danny Bergsagel, and I said, “You know Anderson better than I, but things have changed.” He said, “Well, I still think you should come to Toronto, but I can understand.” I resigned somewhere around April. I was to move in July, so I had to finish up a lot of stuff. I had resigned my commission in the Public Health Service. Then I got a really important letter from Dr. Frei. Dr. Frei got fired.

Lesley Brunet, MA

0:17:35.0

That was in ’65?

Emil J Freireich, MD

0:17:42.5

Yes, before I got here. So I got a letter from Dr. Frei, and he said, “Guess what, J? Dr. Clark has reorganized it.” Dr. Frei had started to do what he did at NCI. He used his authority to change Medicine, Pediatrics, and all the research. Dr. Clark, being always loyal to the people who are loyal to him, said “Frei, maybe it would be better if you just work on your own institute and leave the rest of the place alone.” So he was fired as the associate director of research.

Lesley Brunet, MA

0:18:18.6

But he was still in charge of the chemotherapy?

Emil J Freireich, MD

0:18:21.4

His new title is head of the new department. This is my department. Dr. Frei and I had put together the name, “Developmental Therapeutics.” No one had ever had a Department of Developmental Therapeutics before. We just invented it. I invented it because Dr. Farber had

taught me that we don't experiment on people. The focus of this department was going to be therapeutic, not prevention. It's going to be treatment, but all treatment. We used the word *developmental* because we thought that it indicated that it was research, but we don't do research; we're actually developing treatment. Then I got another letter from Dr. Frei. "I'm head of the department, not you. You are going to be deputy head. You and I can work closely together." But I'm not the big power anymore.

Lesley Brunet, MA

0:19:27.3

How did you feel about that?

Emil J Freireich, MD

0:19:28.6

Well, after I got through crying, I went back to Dr. Farber.

Lesley Brunet, MA

0:19:43.1

So you were crushed because you're no longer head of DT?

Emil J Freireich, MD

0:19:45.6

Yes. Now we're in bad shape. Now there's no reason to leave, so I was going to stay at NCI. I talked to Dr. Zubrod, and he said, "Well, they can't undo the damage they've done." I talked to Dr. Farber. He said, "Well, you probably still have a good opportunity." I talked to Danny Bergsagel. He said, "I'm telling you, you should come to Toronto." So Deanie and I had a long conversation.

Lesley Brunet, MA

0:20:16.7

Do you remember what Danny Bergsagel said about things at Anderson, because he had been here?

Emil J Freireich, MD

0:20:20.4

He was positive. He left here for a good reason. He trained in Toronto. He had a good experience here. He had no reason to want to leave, but he had enormous opportunity in Toronto, and he's been very successful. He was the chairman of Medicine. He built a great department. He's retired now. He came to our festschrift, so he's still a very good friend of mine. But Tom Frei put the pressure on me. He said, "You know, even though we're not in charge of everything, we do have enormous opportunities." So I came.

Lesley Brunet, MA

0:20:53.6

Those first few years here must have been tough.

Emil J Freireich, MD

0:20:58.9

The toughness has just begun.

Lesley Brunet, MA

0:21:01.6

You haven't even gotten here yet. I thought you got here about July.

Emil J Freireich, MD

0:21:02.4

No, you see, I love Dr. Clark, but he was brutally cruel to us.

Lesley Brunet, MA

0:21:10.6

Oh, really? In terms of what he had promised and then what was delivered?

Emil J Freireich, MD

0:21:17.3

Correct.

Lesley Brunet, MA

0:21:19.0

You mentioned before it was because of his loyalties to other people.

Emil J Freireich, MD

0:21:23.4

Presumably. I'm giving him the benefit of the doubt. But as far as Dr. Frei is concerned, he had the money, but he was totally crushed. He'd given up a position of enormous authority and power to come here for a new opportunity, and it vanished before he even started. You'll see how he feels about Dr. Clark today. But at that time, in '65, it was a very bitter pill at best.

Lesley Brunet, MA

0:22:05.2

Was it deception on Clark's part?

Emil J Freireich, MD

0:22:09.2

No.

Lesley Brunet, MA

0:22:09.6

Or had he simply been naïve about how people would take this?

Emil J Freireich, MD

0:22:12.3

No. I just saw the *Pirates of Penzance*—honor and duty. Everybody does the best they can do. No one tries to deceive, but it turned out to be a deception. In other words, he'd made a commitment without anticipating the backlash from his own faculty. That was naïve, in a way. Clark was very optimistic, so he presumed that everybody would go along. When Dr. Frei was being recruited, Grant Taylor and Cliff Howe all gave him the red carpet. "Oh, we'll work together." But when he got here and started telling them what to do, then things changed. It was tough to change, but he did change. His loyalty to his commitment to Dr. Frei was second to his loyalty to the people who had worked here for 20 years. So one would say that in retrospect it's a good idea, but as far as Dr. Frei was concerned, it was bitter.

So Frei called me and told me he was bitterly disappointed, but we still had a chance, and he couldn't do anything if I didn't come. As I say, I went back over all my tracks, and it was just too late to change. We had to go to Houston. We continued our commitment, but another very important event occurred in 1965—the war in Vietnam.

Lesley Brunet, MA

0:23:54.2

Of course.

Emil J Freireich, MD

0:23:55.8

So between Frei getting fired and me committing to coming, the next blow was, because of the war, the federal matching funds for the Lutheran Pavilion and our Clinical Research Center were frozen. Secondly, our medical school was shifted from Houston to San Antonio by the legislature. By the time I arrived in July, my department was gone, my medical school was gone, and my hospital was gone.

Lesley Brunet, MA

0:24:45.0

It's pointless to ask you if you were a little nervous about your decision.

Emil J Freireich, MD

0:24:52.9

When you're in that circumstance, all you can do is the best you can do. It's like when you're drowning, you've only got so many choices. So I piled my family in my black Ford station wagon, we went to Chicago to see my relatives, and then we came down from Chicago to Houston. We arrived July 15.

Lesley Brunet, MA

0:25:15.7

Just when it's heating up.

Emil J Freireich, MD

0:25:18.6

We'd never been outside of the Northeast.

Lesley Brunet, MA

0:25:23.3

So you didn't even come down and look at things here?

Emil J Freireich, MD

0:25:25.2

Yes, I did.

Lesley Brunet, MA

0:25:26.7

You did, but did your wife?

Emil J Freireich, MD

0:25:28.0

No. They couldn't afford that. Family life was different in the '60s than in the '90s. My wife was a normal wife, and she would go where my job was. If I decided to go, she was going to make the best of it. We got in our station wagon, and we headed down. Once we got to Kansas and Oklahoma, we began to think that this decision wasn't a very good idea.

Lesley Brunet, MA

0:25:58.1

Was this before air conditioning in cars?

Emil J Freireich, MD

0:26:00.0

No, we had a unit in the car that was installed. It didn't really cool.

Lesley Brunet, MA

0:26:07.6

It blows?

Emil J Freireich, MD

0:26:09.9

It blows. We were in a black station wagon with 4 children. My baby son was 5, and my oldest was 11.

Lesley Brunet, MA

0:26:15.7

No dog?

Emil J Freireich, MD

0:26:20.7

No dog or cat, just the 4 kids, my wife, and myself in a Ford station wagon. When we got to Oklahoma, it began to be bleak. You know how Oklahoma is? There's nothing in sight but a

couple of oil wells. Finally, when we got to Dallas, the car boiled over. So we had to spend a day in a garage getting the radiator fixed, with the 4 kids screaming and shouting, in this hot repair place with no place to go. We were too poor to get a place to stay, so we just sat in this car dealership and waited for him to fix the radiator. We got back in our car, and we drove to Houston. Dr. Frei had recruited 2 other people since I had agreed to come. One was Ti Li Loo. Dr. Loo had been the pharmacologist at the Cancer Institute. So after I agreed to come, and Loo knew I was going, he agreed to go. He's a dear personal friend. His daughter has cancer of the colon, unfortunately.

Lesley Brunet, MA

0:27:43.3

Is he in good health?

Emil J Freireich, MD

0:27:45.0

He's struggling. Also, Dah Hsi Ho, who's still working every day here. She came from Buffalo. Dr. Ho and Dr. Loo were both here. Dr. Frei was trying to make our transition easy, so he rented an apartment in a rental facility. I can't remember the name of it; it's since been condemned. It was on South MacGregor, on the bayou. They were called the Field Town Apartments. I love thinking about it. If my wife were here, you would get a horror story. Dr. Loo and Dr. Ho were there, and Dr. Frei was still there.

Lesley Brunet, MA

0:28:48.7

In these apartments?

Emil J Freireich, MD

0:28:49.7

Yes, in these apartments. He had just arrived in late '64, about 6 months before, and he was still living in this apartment. He thought it was wonderful. It was a slum apartment. This was the filthiest, dirtiest place I have ever seen. This place was strictly a slum apartment. Of course, Dr. Frei and his wife and kids loved it. They had a dirty swimming pool; it was all contaminated with algae. The playground, everything was broken. Nothing worked. We arrived in the afternoon. We got out of our car, we got the keys to our apartment, went to the apartment, and we met the most horrible animal in all Texas.

Lesley Brunet, MA

0:29:32.9

The cockroach. I don't know if other people understand that.

Emil J Freireich, MD

0:29:38.3

We opened the back door, and there were 5000 of them. This place was a garbage dump. There were roaches everywhere. We got back in the car. There were no Kmart's or Wal-Mart's. We bought mattresses, toilet seats, cleaners, everything we could imagine, and we spent a day and a

half, without sleeping, cleaning that apartment just so we could live in it overnight. My wife wouldn't let the children use the john. They had to go outside in the pool. The first thing that happened is my daughter broke her finger on the poolside, and we had to take her to the hospital. Oh, our first few days were so horrible.

I came to MD Anderson. I slipped on the stairs and got a big hematoma in my hip. I was in bed for a day and a half. My wife said, "J, if we don't buy a house today, I am going back to Washington." I told Dr. Frei, "You're not going to see me at work. I have to go buy a house." When we left Washington, our teacher had recommended the Memorial school district, so we looked on the map for the Memorial school district, we got in our black station wagon with our 4 kids, and we drove out to Memorial. We drove up and down the streets until we could find a house that we could move into, and we found a house.

Lesley Brunet, MA

0:01:14.0

That's at least a pretty neighborhood.

Emil J Freireich, MD

0:01:15.2

The same house we live in today, 35 years later. This house was a spec house. It was standing. The guy who built it was a builder named Smith or something, and he had bought a piece of land in Hedwig Village and had subdivided. It was one person's home with a racetrack and everything. He put, I think, 12 homes on this zoned half acre. It was about a 6-acre plot. Our house was standing there, and they had curbs and sewers. You had to be interviewed by the sales guy and by the builder in order to get into this community, because they wanted handpicked people. They liked this young doctor from MD Anderson with 4 kids. He actually gave me one of his poodles from his litter, and we bought this house. We paid \$47,500 for it.

Lesley Brunet, MA

0:02:29.8

That's in 1965?

Emil J Freireich, MD

0:02:31.8

Yes, in '65. You have to understand that when I left the government, my salary was \$5600 per annum. Dr. Clark paid me \$25,000.

Lesley Brunet, MA

0:02:48.7

That's quite an increase.

Emil J Freireich, MD

0:02:52.7

We were rich. I have no idea what Dr. Frei was being paid, but it was more than me. Dr. Clark was very well connected, so I talked to him, and he sent me to a bank, where we got a 5½

percent, 30-year loan. I think our down payment was whatever we sold our house for, maybe \$4,000. Only Dr. Clark could make that deal. We had our house. The day we moved in, the air conditioning broke down, so I fixed the air conditioning. Our transition to Houston was painful.

Chapter 12

The Lay of the Land: Developmental Therapeutics and MD Anderson in 1965

B: Overview;

Codes

C: Portraits;

B: MD Anderson Culture;

B: Working Environment;

B: Growth and/or Change;

C: Leadership; D: On Leadership;

B: Obstacles, Challenges;

B: Institutional Politics;

B: Controversy;

B: Critical Perspectives on MD Anderson;

B: MD Anderson History; B: MD Anderson Snapshot;

Emil J Freireich, MD

0:02:52.7+

Okay. What's happening at work? Dr. Frei is head of the Developmental Therapeutics. I'm deputy head. Grant Taylor agreed that Pediatrics should be a part of our department, so it became part of our department. Remember, 100 percent of my work was in childhood leukemia. I'd never had any pediatric training, but the pediatricians in those days only took care of healthy kids. They didn't like leukemia and cancer. It wasn't popular with pediatricians.

Lesley Brunet, MA

0:04:22.5

Were Dr. Sullivan and Dr. Sutow already doing research here?

Emil J Freireich, MD

0:04:25.2

Yes, they were. They had Sullivan and Sutow. Grant Taylor was the major one who was recruiting me. He loved the childhood stuff. Grant Taylor was the inverse of Dr. Clark. Dr. Clark was ambitious; he would've done anything to reach his goals. Grant Taylor really wanted to do good things. He should've been a minister. He was such a nice man.

Lesley Brunet, MA

0:05:04.2

So he wasn't upset about Pediatrics being part of your department?

Emil J Freireich, MD

0:05:07.8

Oh, he begged us to come. We were curing children, and he wasn't. He wanted us to bring this stuff to his kids. He used to go around and bring food to the children, and he'd hug the mothers. Grant Taylor was just full of compassion. He just loved people. He wanted us to come in the worst way, so we came, and we started to work.

Lesley Brunet, MA

0:05:41.5

When Developmental Therapeutics was established, it had 3 sections: Research Hematology, Applied Molecular Biology, and Pediatrics.

Emil J Freireich, MD

0:05:54.0

Correct.

Lesley Brunet, MA

0:05:58.0

Was C.C. Shullenberger the head of Hematology?

Emil J Freireich, MD

0:06:01.4

We're coming to all that. When we walked in the front door, Dr. Clark was absolute dictator. He answered to nobody. He had 3 department chairmen. Dr. Howe, who was in charge of Medicine, he'd recruited out of the military. The first person he recruited was Bill Russell, Chairman of Pathology, and then Ed White. The fourth person who sat at the table was Felix Haas, who kind of worried about the basic science people. And there was a fifth one. I forgot Gilbert Fletcher. There were 5 guys, and this is who ran MD Anderson. Dr. Frei never liked administrative things, so I always did the administrative work. I did that at NCI, and I did that here. I used to go to the department head meetings to report back to Frei. The only one in this group who cared anything about DT was Dr. Russell. He had a grant, and he cared about DT. Cliff Howe was a very nice man, but no sparks.

Lesley Brunet, MA

0:07:38.4

I got the feeling there was some conflict with the Department of Medicine. Actually, they were in conflict with a lot of people.

Emil J Freireich, MD

0:07:49.8

I walked in, and we're part of this thing, and Dr. Clark said, "You know, J, cancer treatment is surgery. The rest of it is very helpful, but everything supports surgery."

Lesley Brunet, MA

0:08:06.7

Dr. Clark or Dr. Howe said this?

Emil J Freireich, MD

0:08:09.4

Dr. Clark.

Lesley Brunet, MA

0:08:10.4

Well, of course. He was a surgeon. Is that why he said it?

Emil J Freireich, MD

0:08:14.3

Dr. Fletcher is very flamboyant. He stood up and said, “Dr. Clark, you’re almost right, but for your information, radiotherapy can cure cancer.” They were doing work on head and neck and the cervix, and Fletcher was the first person to claim that radiation therapy could cure cancer—not palliate, cure. Being the jerk that I am, I said, “Dr. Clark, chemotherapy can cure cancer, and the future of cancer treatment is going to be with chemicals, systemic cancer, because everybody who dies of cancer dies of systemic cancer. Local control will never control cancer.” So I wasn’t popular, day one, because Cliff Howe was standing there, and Cliff Howe had never dreamt of curing cancer. Also, there was Shullenberger. Shullenberger was a military guy. A lot of the people Clark recruited were military people. You work 8 hours a day. Then you get drunk at the faculty club, and that’s life. You work 5 days a week and you build up your retirement. I’ll tell you some “Shully” stories later, but he had no interest in anything academic. He was just doing his job.

Things were going to get very heated here in the first couple of years, because Dr. Frei came in ’64, but he left in ’72. So it’s obvious things weren’t going well at the beginning. We would sit out on the lawn in the middle of the summer in those Field Town Apartments and commiserate about how unfortunate our situation was. We had no resources. We had zero prospects, and things went from bad to worse.

There were 2 guys who did do some research. Danny Bergsagel had left, but he had a fellow, a trainee, Raymond Alexanian. When I arrived in ’65, Alexanian had inherited the myeloma program from Bergsagel. Bergsagel was the first person to treat myeloma effectively with an alkylating agent. That put MD Anderson on the map as far as treatment was concerned. Bergsagel was an innovator. Alexanian [oral history interview] trained with Bergsagel, and he was here. The other person that was here was a guy named Joe Sinkovics. If you haven’t met him, you have to meet him. He’s in practice in Tampa. He’s a very colorful Hungarian, and he did kind of immunotherapy. It was science, in some sense. During my recruitment, the people who took me around were Sinkovics and Alexanian.

When I was appointed deputy head, Dr. Clark said, “Sinkovics and Alexanian are going to be put in your department because they do research.” Cliff Howe was going to run Medicine. They do service work. They take care of diabetes and hypertension, in what’s now the Department of Medicine.

There was also a guy here named Naguib Samaan, who did endocrinology. Sinkovics and Alexanian in my department were doing fine. Pediatrics in my department was doing fine. So we started work. Well, what did we have to do?

The first thing I had to do was get patients. The department now consists of Frei and Bobby Williams, his exec. He had also hired a gal for me; her name was Sharky Bagdasarian. It was these 3 people and me. What were we going to do? Well, there was no space. It had all been taken away. There were no beds. They'd all been taken away.

Dr. Leon Dmochowski was also involved in our recruitment. He was a scientist who did electron microscopy to look for viruses. He said, "I'll give you a lab and an office." So we shared Dmochowski's office. He had a little threesome on the fifth floor in the old building, and I had a little office and a little lab that was about 300 square feet. That's how we began.

Chapter 13

Getting to Work, Diving into Controversy, and Studies of POMP

A: The Researcher;

Codes

A: The Researcher;

A: Overview;

A: Definitions, Explanations, Translations;

C: Discovery, Creativity and Innovation;

C: Discovery and Success;

D: On Research and Researchers;

C: Professional Practice; C: The Professional at Work;

B: MD Anderson Culture;

B: Working Environment;

B: Growth and/or Change;

C: Leadership; D: On Leadership;

B: Obstacles, Challenges;

B: Institutional Politics;

B: Controversy;

B: Critical Perspectives on MD Anderson;

B: MD Anderson History; B: MD Anderson Snapshot;

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

D: The History of Health Care, Patient Care;

D: Technology and R&D;

C: Patients; C: Patients, Treatment, Survivors;

Emil J Freireich, MD

0:08:14.3+

I started making rounds on Pediatrics, and the first day I went around, I said, “I have just claimed that we can cure leukemia with this VAMP.” Dr. Sullivan said, “Dr. Freireich, we don’t give our children vincristine. It’s too toxic.” Dr. Sutow said, “Anything you say, Margaret.” Dr. Taylor just did compassion. Pat Sullivan ran Pediatrics with an iron hand. Grant Taylor worked for Pat Sullivan, and Sullivan ran the show. I said, “I’ll tell you what we’ll do. We’re going to recruit Myron Karon, who worked for us at the Cancer Institute.” He was on sabbatical in France. Karon agreed to come, and he brought with him 2 PhD’s that he met in France. One was Grady Saunders, who is still here, and the other one is his wife, Priscilla, who is now retired. So Karon came with these 2, and they set up Applied Molecular Biology.

They trained with [Manod](#), who was the first guy to work out all the genetic code. They were into molecular biology, and we knew that was the future. So we had a section in DT, Applied

Molecular Biology. Karon is a pediatrician. I was getting along with Pat. We were bumpy, but we didn't have any major collisions.

Lesley Brunet, MA

0:15:54.7

Did she do a research program on vincristine?

Emil J Freireich, MD

0:15:58.0

Sure. She became famous for vincristine.

Lesley Brunet, MA

0:16:00.9

Did you have to convince her to do that?

Emil J Freireich, MD

0:16:04.4

More than convince. Dr. Karon arrived, and I said, "Look, here's Pediatrics. Grant Taylor's the boss. Pat Sullivan is a bit of a pain, but let's get going." Karon started making rounds. Now, Karon was not diplomatic like me. He had done a study at the Cancer Institute on how children react to fatal illness. He did this study with a psychologist. He interviewed children and parents, and then looked at how they fared, and he proved for the first time that children have to be treated like adults. They have to know about their disease, and the parents have to be treated like parents. He came in, and he found this pediatric service. When we made rounds, you'd see parents sitting in the room with their children who were bleeding, infected, and vomiting. They lived with their children in these rooms. They had a bed for the parents.

Dr. Karon came to me, and he said, "This is intolerable. We're torturing the parents. What happens when a child is sick is you say, 'Okay. He comes to the hospital. Now the doctors are in charge. You go home and rest. We'll take care of the baby. We take care of the vomiting and the bleeding and the dying, and you cry, but you don't have to sit in the room while they're being tortured. You don't have to hold their hands while you're doing a bone marrow and all that.'" Dr. Sullivan said, "We cannot change the way we run the service." So after about 2 months, we received a memo from Dr. Taylor. "Dr. Karon can no longer see children on Pediatrics."

Lesley Brunet, MA

0:18:07.0

That was a big deal for parents to be able to stay with their children. Until you said that, I thought it was something favorable.

Emil J Freireich, MD

0:18:16.6

It's horrible. It's torture for the children. Children don't like to suffer when their parents are there. It's horrible for the parents. It's horrible for the staff. You can't do anything. You can't treat the children when the parents are there. Who's in charge? You tell a kid who's 2 years old,

“You have to get your blood drawn.” Who’s supposed to protect him from pain? His parents are, so he doesn’t know what to do. Parents are supposed to protect him, and they’re standing there and want to torture him. The doctor’s got a white coat. He’s supposed to do that.

When the kids are treated properly, the parents should not be there. Now the child understands. “Parents are not here to protect me. Doctor is here to help me. If it hurts, it’s his responsibility.” It’s clear. That’s the only way to take care of children, and it’s proven quantitatively, by interviews, by cooperation, by how long they live, and how well the treatment goes.

Lesley Brunet, MA

0:19:19.6

I’ll bet that was a real battle.

Emil J Freireich, MD

0:19:22.0

Karon was fired.

Lesley Brunet, MA

0:19:23.8

He was completely fired or fired from Pediatrics?

Emil J Freireich, MD

0:19:26.1

He was fired from Pediatrics. Well, he’s a pediatrician. What’s he going to do? He took care of adults with leukemia for a while. Of course, he immediately started looking for a job, and he went to UCLA, where he was head of Pediatrics at Children’s Hospital and was an enormous success. He died tragically at the age of 35 of a stroke, shortly thereafter.

When Karon was fired, I was fired. We had a confrontation with Dr. Clark. Dr. Taylor said, “We love Dr. Freireich, and we want his advice. But he can’t be in charge of taking care of the children, because he’s not certified in pediatrics.” So I wrote to the board, and I said, “I have cured childhood leukemia. I have run the best leukemia pediatric service in the world for 10 years. I think I should be certified as a pediatrician.” They wrote back, “You can be certified if you take a 2-year residency.” Dr. Clark said, “Well, Freireich, you can make rounds and give advice, but you’re not in charge of Pediatrics anymore.” Pediatrics was taken out of DT. So we have no hospital, no medical school, no children, no Pediatrics, no nothing. Things are not going well. Let’s work on adults. We went over to the Shamrock and drank with Cliff Howe and Shullenberger one night. We went to their house, and we went to their office. I had no referrals; none, zero. So I went to Dr. Howe, and I said, “Dr. Howe, I had discovered this treatment at the Cancer Institute called POMP. It’s an adaptation of the VAMP study. We had 60 percent responses in adults with acute leukemia. Nobody in the world has done that. I’ve written a paper. I need to have referrals. When a doctor calls with a patient with acute leukemia, you have to refer them to me.” “No problem.”

Lesley Brunet, MA

0:21:57.0

Dr. Howe said this?

Emil J Freireich, MD

0:22:00.0

Yes. I went back in the medical record room with Eleanor MacDonald, and in the previous 10 years, they had had 100 patients with leukemia referred here, 10 a year. I'd been here 2 months and didn't get any. I didn't think anything of that. Maybe that's all they get. Nobody knows I'm here. I'm famous. I've written papers. I've got prizes. I can treat leukemia.

Finally, I got my first patient. He was a gastroenterologist on the staff at Baylor. He developed acute myeloid leukemia. He went to the literature, he discovered Freireich, and he found out he was here. He walked into my office and said, "I have leukemia. I want you to be my doctor." My first patient was self-referred. I had a patient. I think his name was [redacted] or something like that. I had this new treatment, informed consent, POMP. This is a disease that is 100 percent fatal; median survival is 6 to 8 weeks. It had 99 percent mortality in 11 months. We're going to try something new. Good idea. Okay, now I've got a patient. What's next? Well, I have to have platelets. Where am I going to get that? There's no blood bank. There are no platelets.

Dr. Shullenberger ran the hematology lab, not Lab Medicine. If you wrote out a slip for a platelet count, Dr. Shullenberger sent back a platelet count, something that didn't make any sense. So I went to his lab, and I talked to the technicians. "How do you do your platelet counts?" Well, they were doing the Dameshek method, which is the method that Dr. Brecher had proved was ineffective in 1954. This is 1965. He's only a decade behind. "You're doing the Dameshek method? That is not acceptable for my patients. We have to use the Brecher-Cronkite method." "Well, we don't know how to do that. We don't have a phase microscope. We don't have any equipment. Sorry." So we wrote a grant, and we got money. We bought a phase microscope, hired a technician, and we did platelet counts in my little lab. Now we knew what the platelet count was. The next problem was we had to get platelets. Where are you going to do that? Well, I discovered this system. I published. I'm getting prizes. We just have to do platelet apheresis, but who's going to do that? No one is going to do it. Dr. Clark gave me a little kitchen that wasn't being used, put in a bed, and we started collecting platelets.

Before we started in 1955, when you treated leukemia, you judged it based on blood alone. I learned to do bone marrow aspiration from Charles P. Emerson, who was the first person in the United States to do bone marrow aspiration, when I was a fellow in Boston. I knew how to do bone marrows. We published a paper that if you have normal blood and still have leukemia in the marrow, your survival is no different than if you didn't have treatment. In order to get prolongation of remission, you had to get a normal bone marrow. So I treated my patient. The blood was okay, but I needed a bone marrow.

Who does the bone marrows? Dr. Shullenberger. How does he do bone marrows? He puts the bone marrow in a hematocrit tube and spins it down and measures the buffy coat. That was

unacceptable. The way we do bone marrows is we make a smear, and we get clot section, and we quantitate the cellularity of the bone marrow. Who's going to do that? Not Shullenberger. John Shively was working in Pathology under Dr. Russell, so I went to Dr. Shively and said, "You know, it's wrong for this clinical guy to be doing hematology. We ought to do it in Lab Medicine." So we started Lab Medicine. They learned how to do the Brecher-Cronkite method. They did platelet counts. Anyone in the hospital could order it. They set up a little pheresis area. They began to collect platelets. More importantly, when I did a bone marrow on my patients, I wrote, "Do not send to Dr. Shullenberger's lab. Send to Dr. Shively in Pathology." He gave me a bone marrow report. The nurses hated me.

Now, you can understand that Shullenberger didn't think I was strictly his ally. We were now in '66. We were moving ahead. So Pediatrics hates me, Shullenberger hates me, and Howe hates me. How about Alexanian? He's in my department, right? We had a meeting. Dr. Grant Taylor had founded the Southwest Oncology Cooperative Group. He asked Dr. Frei to be the chair, and Dr. Frei asked me to chair the leukemia group. We had a grant that we competed for, and then we had a meeting with Dr. Taylor. Dr. Alexanian said, "I think I should be the PI on this leukemia grant, not Freireich, because I was here when he came." I said, "That's unacceptable to me. It's either me or nothing." So I became head of the leukemia thing, and Alexanian also hated me.

Chapter 14

Developmental Therapeutics in the Midst of Opposition to Systemic Treatment of Cancer

B: Building the Institution;

Codes

A: The Researcher;

B: Research;

C: Professional Practice; C: The Professional at Work;

C: Understanding the Institution;

C: Discovery and Success;

B: MD Anderson Culture;

B: The Business of MD Anderson;

C: The Institution and Finances;

B: Working Environment;

B: Growth and/or Change;

C: Leadership; D: On Leadership;

B: Obstacles, Challenges;

B: Institutional Politics;

B: Controversy;

B: Critical Perspectives on MD Anderson;

B: MD Anderson History; B: MD Anderson Snapshot;

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

D: The History of Health Care, Patient Care;

C: Patients, Treatment, Survivors; D: Ethics;

Emil J Freireich, MD

0:22:00.0+

We didn't give up. Even though we had nothing, we kept trying to build. The way Dr. Frei and I built our department was, since nothing was given, we had to create it. Dr. Clark, to his credit, if he couldn't give it to you, he would not interfere with you creating your own resources. Since we knew Dr. Zubrod at the NCI and knew about the granting mechanism, we aggressively pursued grants. We got a grant for our platelet transfusion support, and we got a grant for our infectious disease support.

Lesley Brunet, MA

0:29:41.5

You had a lot of grants.

Emil J Freireich, MD

0:29:43.3

Dr. Clark moved a temporary building from his ranch to the parking lot, and that was our office and laboratory space for Dr. Loo and Dr. Ho. Nothing came from MD Anderson. Everything in DT was created with federal money. When I was fired as head of DT by Dr. Irwin Krakoff in 1983, over DT's 17 years, our overhead exceeded our state budget every year. We never got a dollar from Dr. Clark or the state of Texas. Our department was built entirely with federal money, private money, and money from drug companies. That was our goal. That's what Clark wanted us to do. Our job was to build MD Anderson, not to bleed it. He had gotten as much as he could. We were supposed to add resources, and we did.

So we had this clinical research center. Dr. Clark said, "Okay, Freireich, you can run that," and he gave us 3 West for our research patients. We recruited Gerald Bodey, since retired, and he became head of our infectious disease program. Joe Sinkovics used to be the infectious disease expert, but Dr. Clark came to me and said, "Well, Alexanian and Sinkovics have requested that they come out of your department and go back to Medicine." Okay, so we were back to ground zero. Sinkovics hated Gerald Bodey because he was good. Sinkovics was okay, but not good. We hired Evan Hersh, and Hersh was in charge of our immunotherapy program. He's now at University of Arizona. Hersh has been gone for probably 5 years, maybe 10. Bodey retired maybe 4 or 5 years ago. It was not a voluntary retirement. There were forces. When people leave, there's always pushing and pulling. There's an opportunity, but there's got to be trouble at home or you don't leave.

We had Bodey, Hersh, Loo, Ho, Frei, and Freireich. We had some grants, and we had to expand our program. We had started a reverse isolation research at the NCI, so we got a grant from NCI, and we bought 2 life islands. We convinced Dr. Clark to modify 2 rooms on 3 West to make them germ-free rooms.

Our immunotherapy program was booming. We were doing BCG vaccination. Our chemotherapy program was booming. We were attracting adults with leukemia. We didn't have any beds here, so we leased beds from Hermann Hospital and started a unit over there. Eventually we leased space in the Center Pavilion Hospital, when they converted it to a hospital from an apartment building. It's now been torn down, of course.

Lesley Brunet, MA

0:03:29.0

So at Hermann, they didn't have Anderson patients there before?

Emil J Freireich, MD

0:03:36.6

When we were here there were zero. However, we did have a unit, and they agreed to lease it to us. Joe Boyd worked out a contract, and we had something like 11 beds. We put our patients over there, and we cared for them.

Lesley Brunet, MA

0:03:53.9

They were adults or pediatric?

Emil J Freireich, MD

0:03:55.1

They were adults. I was out of pediatrics. From '65 on, pediatrics is history. I have nothing to do with it. So we started to build, but we were creating a lot of animosity. Everybody in Medicine despised us. We started the first adjuvant chemotherapy for breast cancer. We did the first studies with Adriamycin, which was developed initially in Italy. We got the drug, we began to do studies here, and we found it very active in breast cancer. Jeff Gottlieb was here at the time, in the cooperative group. We said, "If it worked in leukemia, why shouldn't it work in breast cancer?" Adriamycin had an 80 percent objective response rate in breast cancer.

We said, "Okay. So you get women with Stage III disease. They have a 90 percent chance of dying of metastatic cancer. You do an operation, and then what? Then you radiate the hell out of them. Well, what good does the radiation do when they're going to die of metastases? They need systemic therapy. We have good systemic therapy."

We developed our solid tumor practice again. No referrals from Dr. Howe in MD Anderson. If a doctor in Texas calls up MD Anderson and says, "I have a patient," it goes to Medicine. If they call me, they go to DT. If they call Frei, they go to DT. We developed our own practice in Texas. No patients came from them. They never sent us any patients. As a matter of fact, that's the reason Cliff Howe finally got fired.

Lesley Brunet, MA

0:05:42.9

Were DT people doing some of the staffing in the Diagnostic Clinic?

Emil J Freireich, MD

0:05:54.5

No. Two of our fellows went to Diagnostic Clinic—Ed Middleman and Harry Price. Ed Middleman is in practice in Dallas, and Harry Price is still there. But we had nothing to do with Diagnostic Clinic.

We gave adjuvant therapy to women with breast cancer. No radiation therapy. I went to a staff meeting, and Dr. Fletcher stood up in Dr. Clark's presence, and he said, "Freireich, you are a murderer." He hated chemotherapy. Radiation therapy was his life. "You're a murderer, Freireich. You're denying these women radiation to the breast." But we proved that he was wrong and we were right. Of course, adjuvant therapy in breast cancer is now the standard of care everywhere in the world. In fact, they do it for Stage II breast cancer. So Radiotherapy hated us.

We also had Lillian Fuller; she was the Pat Sullivan of radiation therapy. Dr. Fletcher trusted Lillian Fuller. She took care of lymphoma. One of the first things we did was the MOPP in Hodgkin's disease and confirmed that what we had reported to NCI was correct. We did it through the cooperative group. Then, because we had Adriamycin, we developed the CHOP in our department. They were still giving Cytosan to all the patients with Hodgkin's disease. We

were giving them CHOP, and we showed that you got a 90 percent response rate and a 50 percent cure rate. We cured lymphoma.

Lesley Brunet, MA

Once you showed them that, did they continue to treat them differently, or were you able to convince them?

Emil J Freireich, MD

There was a lag of about 5 years.

Lesley Brunet, MA

That's a long lag.

Emil J Freireich, MD

Yes. It was tragic. I used to go to Shully and say, "Look. Here's our data." We used to take him out to drink. I even got drunk with him one time. "Look. Send that to your lymphoma." No, they wouldn't do it. Alexanian did the lymphoma patients.

Lesley Brunet, MA

Is there still a lag?

Emil J Freireich, MD

There's still a lag. We'll come to that later, because eventually Medicine disappears, as you know. So we got CHOP. We're getting along. Radiotherapy hates us. Medicine hates us. We invited Nikos Logothetis to dinner once, and he said, "We used to call it 'Detrimental Therapeutics.'" That's what they used to call it.

Dr. Robert Hickey was a surgeon. The surgeons hated us. Hickey called me in his office one day on a Monday. He said, "Freireich, every time I schedule a patient for surgery, all the beds are full with these goddamn terminally ill DT patients. When are you going to cut this out?" I made a deal with Dr. Hickey that whenever he wanted to admit a patient for surgery, I would move a patient to Center Pavilion Hospital. The surgeons hated us, because we were interfering with their practice. I remember J. Ballantyne, God bless him. I used to fight with all these guys. They hated me, because the treatment of cancer was local. The head and neck surgeons did surgery and radiations. I said, "We have to give them chemotherapy." J. Ballantyne used to send me patients with metastases of the brain and say, "Okay, Freireich, if you're so smart, cure this guy." I'd say, "Okay. I'll do the best I can." We'd give him this and that and everything.

I had a big fight with Dick Martin once over this football player who had a sarcoma, and they did an amputation. I said, "Listen, this kid isn't cured. He needs chemotherapy." He wouldn't do it. He died of metastases. So they all hated me because I wanted to treat adjuvant to surgery all the patients with chemotherapy. They didn't want to refer them. Radiotherapists wanted to radiate them. We wanted to give chemotherapy. They hated us.

The medical people hated our guts because we were ruining their lives. They had to learn all kinds of new things. Pat Sullivan hated us because we were trying to change pediatrics, and she had to give vincristine and do combinations; she had to do all these things. I discovered the intrathecal therapy for meningeal leukemia; she had to do that. She didn't want to do spinals because, "It would hurt the children." Grant Taylor, God bless him, he had to finally say, "Well, maybe the parents don't need to be in the room." So who's left that doesn't hate us? Only Dr. Clark. He doesn't hate us. Well, the Blumenschein thing comes along. That's the interesting part of the story.

Chapter 15

Developmental Therapeutics, the Division of Medicine, and Dr. Clark's Final Years as President

B: Building the Institution;

Codes

C: Professional Practice; C: The Professional at Work;

C: Understanding the Institution;

B: MD Anderson Culture;

B: Working Environment;

C: Leadership; D: On Leadership;

B: Obstacles, Challenges;

B: Institutional Politics;

B: Controversy;

B: Critical Perspectives on MD Anderson;

B: MD Anderson History; B: MD Anderson Snapshot;

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

D: The History of Health Care, Patient Care;

C: Patients, Treatment, Survivors; D: Ethics;

Emil J Freireich, MD

We came in '65, and we got to '72, when Dr. Frei leaves. At the time Dr. Frei decides to go, he was still very bitter about Dr. Clark. I think in his old age it had moderated. I was less so because Dr. Clark didn't promise me anything. Frei promised me everything, and the fact that he couldn't deliver wasn't his fault, so I didn't bother with anybody. We just did our thing. By 1972, when Dr. Frei left, DT was the largest administrative unit at MD Anderson. We had the largest clinic and most outpatient business of any department, including Surgery. We had the biggest referral base. We had the most dollar income from patient care. We had the most dollar income from grants and contracts. When Frei resigned, it was bad news.

Lesley Brunet, MA

He takes the grants?

Emil J Freireich, MD

No, he can't do that, but the last time he resigned, I got fired, remember?

Lesley Brunet, MA

Oh, that's right.

Emil J Freireich, MD

Remember, everybody hates DT, except Dr. Clark. “Okay, Freireich. I don’t know what we’re going to do about this DT thing, but I’m going to make you ad interim chairman, and I’ll try to work out a solution.”

Lesley Brunet, MA

How did you feel about that?

Emil J Freireich, MD

I said, “Dr. Clark, I’ve been here 7 years. I’ve built you the best clinical research unit in the country. If you don’t want to make me head, I’m leaving.” He said, “Okay. You’re head.” So DT survived Frei’s leaving intact.

Emil J Freireich, MD

I became head of DT, and the department boomed. We just kept getting better and better and better.

Lesley Brunet, MA

You got a big award in ’72, also.

Emil J Freireich, MD

We get awards every year. We just got new training grants. We just kept getting bigger and bigger and better and better, and we kept recruiting more faculty. Dr. Frei tried to get me to go to Boston, but that didn’t work, so I stayed here. The next big event that occurred was in 1983, when they hired [Irwin] Krakoff. The hiring of Krakoff was a catastrophe for me, for DT, and for the institution, because he was a very destructive person. He was not intelligent, and he was arbitrary, capricious, and crude. Everybody has some high principle that guides them, but this was a man who had “nuttin’.” I never heard him say anything like he wanted to cure cancer or help people or build a hospital. Part of that perception, of course, is the fact that he and I were designed to be protagonists, by LeMaistre [oral history interview], not by Krakoff.

Rulon Rawson was head of Medicine at Memorial Sloan-Kettering. After Dr. Copeland, Dr. Clark recruited the surgeon who’s with the American Cancer Society, Arthur Holleb, to be director of Education. Then he left to go to the Cancer Society, and Dr. Clark hired Rulon Rawson. Rulon Rawson retired from Memorial, and he brought him here. He’s a very accomplished physician-scientist, a wonderful person, and a very highly principled guy. Dr. Clark hired him as director of Education, and Dr. Rawson hired Blumenschein. Rawson immediately identified us as important. Dr. Rawson and I became very close friends and associates. He was from Memorial, he liked DT, and he knew Medicine was a service department. He was a medical oncologist, and he was excited by our research, so we became strong allies.

George Blumenschein was hired from Northwestern to be director of Education. He and I had a couple of confrontations, but eventually we became very close friends and colleagues. But he, by design, was not in DT. He was in Medicine, because he wanted to treat breast cancer, and Nylene Eckels had created a major breast cancer program in Medicine. Nylene Eckels was an

intern of mine when I was a resident in Chicago, so I knew her for 50 years. She was like Sinkovics, very backward. We had to fight with Nylene Eckels to get anything done in breast cancer.

Rawson was able to maneuver Blumenschein into the breast clinic, so we could do DT in breast, in Medicine. Of course, Rulon Rawson and I and everybody, including Dr. Clark, wanted Medicine and DT to come together. It didn't make any sense for patients with breast cancer to get inferior treatment in Medicine to the excellent treatment they were getting in DT. So that was the Blumenschein function. [Aman] Buzdar [oral history interview] came along. We never made that interaction with Hematology, because Shullenberger stayed active, and Lillian Fuller in Radiotherapy and Jim Butler in Pathology kind of kept lymphoma in Hematology with Shullenberger and Alexanian.

So the next big event is when Blumenschein comes in; he's director of Medical Education. Rawson is there, and he says, "Dr. Clark, we have to bring Medicine up to snuff, like it was at Memorial. You've got DT, which is an outstanding leadership group of scientists, and you've got Medicine; it's a bunch of service people. We've got to bring them up."

Dr. Howe and I had a meeting in Dr. Clark's office, and Dr. Howe said, "DT is a pain, and they're doing all this stuff. They're experimenting on people; they're torturing people. They're ruining the environment. They're doing all these experiments. It's terrible stuff." Dr. Clark said, "Dr. Howe, you have to compete with them. I'm not going to rule in your favor." They couldn't compete. So Rawson and Blumenschein decided that they would put Medicine and DT into one. They appointed a search committee, and the search committee consisted of 4 outside people; I don't recall their names. This must have been about 1975. They did a national search, they interviewed people, and they made a recommendation to Dr. Clark that Dr. Freireich be head of the combined Department of Medicine. Cliff Howe, Shullenberger, Alexanian, Sinkovics, Jess Gamble, Bill Nelson, everybody in Medicine said, "No way. Freireich is terrible." Clark couldn't do it, so he just did nothing. We stayed separated. About 3 years later, they decided, "This time we've really got to do it." They appointed another search committee. They had all the chairmen of the in-house departments, and they had 4 outside Cancer Center directors. It was a big committee. They did another national search over 2 years.

Lesley Brunet, MA

This was when Clark was still here?

Emil J Freireich, MD

Clark's still here, but then there was a problem. Then comes '78, the holocaust. You know what happened in '78?

Lesley Brunet, MA

I know LeMaistre came in.

Emil J Freireich, MD

Dr. Clark was fired.

Lesley Brunet, MA

From what I've seen of the records, even earlier, in like '74 and '75, people are already getting nervous about him retiring.

Emil J Freireich, MD

It was his age. Dr. Clark wasn't going to retire. It's just like giving up your children. Everybody wanted him to retire. The legislature, every dean—they all wanted him to retire. But remember, his ambition was to have a health science center at Houston. What year did the medical school start?

Lesley Brunet, MA

It started in '71.

Emil J Freireich, MD

That's when Dr. Clark lost the game, because the legislature made a health science center, and they took the medical school away from Clark and appointed a dean.

Lesley Brunet, MA

Did he think that he was going to control the medical school?

Emil J Freireich, MD

Yes. He had already controlled it. The coordinating board had agreed. He had the graduate school. He had the public health school. He had the dental school. He had MD Anderson. Each had a dean, and he was chair of the deans. The legislature had approved a medical school, and then it went to San Antonio. When it came up again in '71, apparently he made enough enemies that he didn't get it. He was crushed. They hired a new president in Houston.

Lesley Brunet, MA

They hired a new president of the health science center?

Emil J Freireich, MD

Yes.

Lesley Brunet, MA

Plus he had the head of the medical schools.

Emil J Freireich, MD

He was the guy from the space program.

Lesley Brunet, MA

South Carolina.

Emil J Freireich, MD

It was a total catastrophe.

Lesley Brunet, MA

The one from South Carolina was a total catastrophe?

Emil J Freireich, MD

That comes later. First it was the guy from the space program. He was the dean of the medical school.

Lesley Brunet, MA

He must not have been there very long.

Emil J Freireich, MD

Robert Moreton was a good friend of mine and Dr. Clark's closest confidant. I asked Bob Moreton once, "How did Dr. Clark ever lose that battle?" He said, "Nobody knows. There were some forces that they couldn't identify." Dr. Clark was so popular with the legislature. I think it was his age. He was born in '06, so he was 65 or 66.

Lesley Brunet, MA

Plus, to get the medical school in Houston, they had to come to all kinds of agreements with Baylor, so it gets very complicated.

Emil J Freireich, MD

That's correct. So he lost the medical school, and that was the beginning of the end of Dr. Clark. What happened subsequently is the medical school was a catastrophe. We were a powerhouse. Their first dean was Cheves Smythe. The first president was this guy from the space program, whose name I can't remember. He was a catastrophe. They fired him.

Lesley Brunet, MA

I want to say Sprague, but that's later, isn't it? Did Sprague come in later?

Emil J Freireich, MD

Yes, he came later. He was the doctor for the space program. They fired him. Cheves Smythe hired Walter Kirkendahl, and then they fired Cheves Smythe, and 90 percent of the faculty was MD Anderson. I ran Oncology. I loved Walter Kirkendahl. He and I were buddies. We had a service over there. We were getting along fine with the medical school. Then when the president was fired, they needed a new president. They couldn't recruit anyone because the school was in such a hubbub, so they asked Truman Blocker, who was president in Galveston, to come in as ad interim president. Truman Blocker came in as interim president, called a meeting of the faculty, including us, and he announced that his intention was to have MD Anderson become a part of the Health Science Center. It made perfect sense.

The medical school had Hermann Hospital, but there was nothing they could do with Hermann Hospital; it was a total catastrophe. It's not a city-county, it was a private hospital. They had their own board and their own chairman. That marriage was a marriage of hatred. There are only 2 institutions that get patient care support in the whole UT system, Galveston and MD

Anderson. We got bed support from the legislature. We got a beautiful hospital. This should be the teaching hospital for the medical school. Truman Blocker took one look and said, “It’s obvious.” Dr. Clark said, “Wait a minute. If we become a teaching hospital, then the Cancer Center is gone. I am opposed to it.” We went to the legislature. He used all his political muscle.

Lesley Brunet, MA

Was Blocker actually saying “change Anderson”?

Emil J Freireich, MD

Make it public. Dr. Clark and the legislature came to a truce, and I’m sure that this is correct, because he announced it at a public meeting at MD Anderson. Dr. Clark called a staff meeting, and he said, “Dear faculty, I have resigned as president of MD Anderson Cancer Center. The agreement was that both Dr. Blocker and I would resign at the same time because we could not coordinate our views, and the Coordinating Board and the Board of Regents had no way to resolve our conflicts. We had polar opinions. What the Regents have decided to do is to form 2 search committees for the president of the Health Science Center and the president of MD Anderson. Both committees are going to be chaired by none other than the chancellor of the University of Texas System.”

Lesley Brunet, MA

That was LeMaistre.

Emil J Freireich, MD

Charles A. LeMaistre, Jr.

(End of session two)