Emil J Freireich, MD

Interview Navigation Materials

Date submitted: 30 January 2019

Interview Information:

Four sessions: 23 July 2001, 30 July 2001, 6 August 2001, 13 August 2001

Total approximate duration: 6.5 hours

Interviewer: Lesley Brunet, MA, CA

A CV is available. To request supporting materials, please contact:

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Interview Subject Snapshot:

Name: Emil J Freireich, MD
Interviewed: 2001 and 2012
Primary appt: Leukemia
Research: Leukemia, chemotherapy, combination therapy
Admin: Head, Depart of Developmental Therapeutics (1972-1983); Chair, Depart of Hematology (1983-1985)
Other:
Interview link:

About the Interview Subject

Dr. Emil J Freireich (b. 16 March 1927, in Chicago, Illinois) came to MD Anderson in 1965 to serve as Chief of Research (Hematology) in the Department of Development Therapeutics. He was appointed Professor in the Department of Leukemia in the Division of Cancer Medicine.

A pioneer in clinical oncology research and the development of new treatments, Dr. Freireich is best known for developing an effective treatment –effectively a cure-- for childhood acute lymphoblastic leukemia. Freireich worked closely with Emil Frei, MD, and the collaborators

developed the first combination chemotherapy treatments shown to be effective against childhood leukemia.

Dr. Freireich served as head of MD Anderson's Department of Developmental Therapeutics from 1972-1983, when he became Chairman of the Department of Hematology (1983-1985). He served as Director of the Adult Leukemia Research Program (1985-2015). He served as Special Medical Education Director of Educational Programs in the Division of Academic Affairs (2000-2015). In 2014, Dr. Freireich was honored as a Fellow of the American Association for Cancer Research. He retired in 2016.

Major Topics Covered:

Personal and educational background

Research: the continuous flow-blood separator; innovations in chemotherapy and combination therapy

Collaborations with Dr. Emil Frei, III

The Department of Developmental Therapeutics

Building the culture of clinical research; training clinical researchers and physician-scientists

History of cancer medicine, clinical research, translational research and education

MD Anderson presidents and the history and expansion of MD Anderson

Institutional politics as they influenced: Developmental Therapeutics, individual investigators, the standing of clinical research, the education of physician scientists

The ethics of clinical research

About transcription, the transcript, and the views expressed

This interview had been transcribed according to oral history best practices to preserve the conversational quality of spoken language (rather than editing it to written standards).

The interview subject has been given the opportunity to review the transcript and make changes: any substantial departures from the audio file are indicated with brackets [ ].

The Archives may have redacted portions of the transcript and audio file in compliance with HIPAA and/or interview subject requests.

The views expressed in this interview are solely the perspective of the interview subject.
They do not represent the official views of any other individual or of
The University of Texas MD Anderson Cancer Center.

Emil J Freireich, MD

Interview Contents

Interview Session One: 23 July 2001

Chapter 04: *Oncology Research at a New Hospital at the National Institutes of Health*

Professional Path; The Researcher; Military Experience; Professional Path; Evolution of Career; Personal Background; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; [iv]

In this chapter, Dr. Freireich talks about being drafted into the Army, why he thinks that World War II was won by the defeat of malaria, going to work at the National Institutes of Health, and dedicating himself to curing leukemia. "You could either go to Bethesda and take care of leukemia patients," he was told, "or you could serve in the military and go get blown up in Korea. So what the heck? Cure leukemia—that's what I'll do."

Chapter 05: *Treating Leukemia at the NIH*

The Researcher; Portraits; Collaborations; Discovery, Creativity and Innovation; Discovery and Success; Professional Practice; Overview; Definitions, Explanations, Translations; On Research and Researchers; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; Technology and R&D; Patients, Treatment, Survivors; [v]

In this chapter, Dr. Freireich talks about researching and treating leukemia at the National Institutes of Health and how everything he is famous for was done during his 10 years there. He also talks about why academics hated him and starting the first formal clinical trials.

Chapter 06: *The First Systematic Studies of Chemotherapy for Leukemia*

The Researcher; Overview; Definitions, Explanations, Translations; Discovery, Creativity and Innovation; Discovery and Success; Professional Practice; Multi-disciplinary Approaches; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; Technology and R&D; Patients, Treatment, Survivors; Industry Partnerships; [vi]

In this chapter, Dr. Freireich talks about the first study of chemotherapy for treating leukemia, developing new treatments for childhood leukemia, and combination chemotherapy.

Chapter 07: *Treating Hemorrhage in Pediatric Leukemia Patients*

The Researcher; Overview; Definitions, Explanations, Translations; Discovery, Creativity and Innovation; Discovery and Success; Professional Practice; The

Professional at Work; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; Technology and R&D; Patients, Treatment, Survivors; [vii]

In this chapter, Dr. Freireich talks about treating hemorrhaging in pediatric leukemia patients.

Chapter 08: *A Research Controversy: Treating Hemorrhage in Childhood Leukemia*

The Researcher; Overview; Definitions, Explanations, Translations; Discovery, Creativity and Innovation; Discovery and Success; On Research and Researchers; Professional Practice; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; Technology and R&D; Patients, Treatment, Survivors; [viii]
In this chapter, Dr. Freireich talks about the controversies surrounding treating hemorrhaging in pediatric leukemia patients.

Interview Session Two: 30 July 30 2001

Chapter 09: *At the NIH: Studying Infection in Leukemic Patients [early 60s]*

The Researcher; Overview; Definitions, Explanations, Translations; Discovery, Creativity and Innovation; Discovery and Success; On Research and Researchers; Professional Practice; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; Technology and R&D; Patients, Treatment, Survivors; [ix]

In this chapter, Dr. Freireich talks about his studies of infection in childhood leukemia, conducted at the National Institutes of Health in the early 1960s.

Chapter 10: *At the NIH: Vincristine, a Cure for Childhood Leukemia, and VAMP*

The Researcher; Overview; Definitions, Explanations, Translations; Discovery, Creativity and Innovation; Discovery and Success; On Research and Researchers; The Professional at Work; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; Technology and R&D; Patients, Treatment, Survivors; [x]

In this chapter, Dr. Freireich talks about the development of a drug called vincristine and the work for which he is famous: curing childhood leukemia.

Chapter 11: *Leaving the NIH for a Turbulent Research Environment at MD Anderson*

Joining MD Anderson/Coming to Texas; Joining MD Anderson; Personal Background; Portraits; MD Anderson Culture; Working Environment; Growth and/or Change; Leadership; Obstacles, Challenges; Institutional Politics; Controversy; Critical Perspectives on MD Anderson; MD Anderson History; On Texas and Texans; Cultural/Social Influences; [xi]

In this chapter, Dr. Freireich says that "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." He then talks about why he left the National Institutes of Health and joined MD Anderson Cancer Center, his relationship with Dr. R. Lee Clark, and why his family's transition to Houston was "painful."

Chapter 12: *The Lay of the Land: Developmental Therapeutics and MD Anderson in 1965*

Overview; Portraits; MD Anderson Culture; Working Environment; Growth and/or Change; Leadership; Obstacles, Challenges; Institutional Politics; Controversy; Critical Perspectives on MD Anderson; MD Anderson History; [xii]

In this chapter, Dr. Freireich talks about the Department of Developmental Therapeutics at MD Anderson, the prominent physicians he worked with at MD Anderson in the mid-1960s, more about Dr. R. Lee Clark, and the use of radiotherapy for treating cancer.

Chapter 13: *Getting to Work, Diving into Controversy, and Studies of POMP*

The Researcher; Overview; Definitions, Explanations, Translations; Discovery, Creativity and Innovation; Discovery and Success; On Research and Researchers; The Professional at Work; MD Anderson Culture; Working Environment; Growth and/or Change; Leadership; Obstacles, Challenges; Institutional Politics; Controversy; Critical Perspectives on MD Anderson; MD Anderson History; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; Technology and R&D; Patients, Treatment, Survivors; [xiii]

In this chapter, Dr. Freireich talks about the difficulties of treating children, getting fired as head of Pediatrics, and being put in charge of Leukemia.

Chapter 14: *Developmental Therapeutics in the Midst of Opposition to Systemic Treatment of Cancer*

Building the Institution; The Professional at Work; Understanding the Institution; Discovery and Success; MD Anderson Culture; The Business of MD Anderson; The Institution and Finances; Working Environment; Growth and/or Change; Leadership; Obstacles, Challenges; Institutional Politics; Controversy; Critical Perspectives on MD Anderson; MD Anderson History; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; Patients, Treatment, Survivors; Ethics; [xiv]

In this chapter, Dr. Freireich talk about the Department of Developmental Therapeutics amid opposition to systemic treatment of cancer, the development of the immunotherapy program, and animosity among different departments and personalities at MD Anderson.

Chapter 15: *Developmental Therapeutics, the Division of Medicine, and Dr. Clark's Final Years as President*

Building the Institution; The Professional at Work; Understanding the Institution; MD Anderson Culture; Working Environment; Leadership; Obstacles, Challenges; Institutional Politics; Controversy; Critical Perspectives on MD Anderson; MD Anderson History; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; Patients, Treatment, Survivors; Ethics; [xv]

In this chapter, Dr. Freireich continues talking about the Department of Developmental Therapeutics at MD Anderson, the Division of Medicine, the medical school in Houston, and Dr. R. Lee Cark's final years.

Interview Session Three: 6 August 2001

Chapter 16: *Charles LeMaistre, the New President, Initiates Reorganization, with Impact on Developmental Therapeutics*

Building the Institution; Leadership; Portraits; The Professional at Work; Growth and/or Change; Obstacles, Challenges; Institutional Politics; Controversy; [xvi]

In this chapter, Dr. Freireich talks about the new president of MD Anderson, Dr. Charles LeMaistre; reorganization initiatives of the new president; and how Dr. LeMasitre's changes affected the Department of Developmental Therapeutics.

Chapter 17: *The Beginning of the Division System, Closing Developmental Therapeutics (1983), and the Legacy of the Department*

Building the Institution; Leadership; Portraits; The Professional at Work; Education at MD Anderson; Growth and/or Change; Obstacles, Challenges; Institutional Politics; Controversy; Understanding the Institution; The Researcher; Overview; Definitions, Explanations, Translations; Discovery and Success; Critical Perspectives on MD Anderson; MD Anderson History; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; Technology and R&D; [xvii]

In this chapter, Dr. Freireich talks about the beginning of the division system, the closing of the Department of Developmental Therapeutics in 1983, and the department's legacy.

Chapter 18: *Departments Undergoing Change under Charles LeMaistre*

Building the Institution; Leadership; Portraits; The Professional at Work; C: Education at MD Anderson; Growth and/or Change; Obstacles, Challenges; Institutional Politics; Controversy; Understanding the Institution; The Researcher; Overview; Definitions, Explanations, Translations; Discovery and Success; Critical Perspectives on MD Anderson; MD Anderson History; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; Technology and R&D; [xviii]

In this chapter, Dr. Freireich talks about the many changes that occurred at MD Anderson under the leadership of Dr. Charles LeMaistre.

Chapter 19: *Head of the Adult Leukemia Program and a "Project Reassignment" Year at the NIH*

Building the Institution; The Professional at Work; Education at MD Anderson; Growth and/or Change; Obstacles, Challenges; Institutional Politics; Controversy; Understanding the Institution; Critical Perspectives on MD Anderson; MD Anderson History;[xix]

In this chapter, Dr. Freireich talks about his role as head of the Adult Leukemia Program and a "Project Reassignment" year at the NIH.

Chapter 20: *A "Flexner Report for Cancer" and Commitment to Education*

Building the Institution; The Professional at Work; Education; Growth and/or Change; Obstacles, Challenges; Institutional Politics; Controversy; Understanding the Institution; Critical Perspectives on MD Anderson; MD Anderson History;[xx]

In this chapter, Dr. Freireich talks about developing and writing a "Flexner Report" for cancer and touring medical centers nationwide.

Chapter 21: *Views of Charles A. LeMaistre and MD and R. Lee Clark, MD*

Overview; Leadership; Portraits; The Professional at Work; Institutional Politics; Controversy; Understanding the Institution; Critical Perspectives on MD Anderson; MD Anderson History; [xxi]

In this chapter, Dr. Freireich talks about Dr. LeMaistre; how, in his view, Dr. LeMaistre abused his power as president; and why he thinks MD Anderson was hated by other state institutions.

Chapter 22: *An NCI Audit and Problems with a Protocol and Leadership*

Overview; The Researcher; Leadership; Portraits; The Professional at Work; Obstacles, Challenges; Institutional Politics; Controversy; Understanding the Institution; Overview; Definitions, Explanations, Translations; Discovery and Success; Critical Perspectives on MD Anderson; MD Anderson History; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; Ethics; [xxii]

In this chapter, Dr. Freireich talks about being audited by the NCI and suffering a heart attack.

Interview Session Four: 13 August 2001

Chapter 23: *Charles A. LeMaistre's Administrative Success*

Overview; Leadership; Portraits; The Professional at Work; Critical Perspectives on MD Anderson; MD Anderson History; Institutional Politics; Controversy; Understanding the Institution; [xxiii]

In this chapter, Dr. Freireich talks about some of Dr. Charles LeMaistre's administrative successes.

Chapter 24: *Controversies over Use of Drugs in Clinical Trials (1980s)*

Overview; The Researcher; Leadership; The Professional at Work; Obstacles, Challenges; Institutional Politics; Controversy; Understanding the Institution; Definitions, Explanations, Translations; Patients, Treatment, Survivors; Ethics; Cancer

and Disease; Discovery and Success; Critical Perspectives on MD Anderson; MD Anderson History; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; On Texas and Texans; [xxiv]

In this chapter, Dr. Freireich talks about controversies over the use of drugs in clinical trials in the 1980s.

Chapter 25: *Multi-Disciplinary Clinics in a Politicized Environment and a Review of Key MD Anderson People*

Building the Institution; Leadership; Portraits; Professional Practice; Growth and/or Change; Obstacles, Challenges; Institutional Politics; Controversy; Understanding the Institution; Critical Perspectives on MD Anderson; MD Anderson History; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; [xxv]

In this chapter, Dr. Freireich talks about the operation of multi-disciplinary clinics in a highly politicized environment and discusses his thoughts about key personnel and leaders at MD Anderson.

Chapter 26: *Reflections on Leadership Style, Intellectual Freedom, and MD Anderson*

Overview; Character, Values, Beliefs, Talents; Professional Values, Ethics, Purpose; Critical Perspectives; Personal Background; Leadership; Portraits; The Professional at Work; Obstacles, Challenges; Institutional Politics; Controversy; Understanding the Institution; MD Anderson Culture; Working Environment; Institutional Mission and Values; The Researcher; Critical Perspectives on MD Anderson; MD Anderson History; On Texas and Texans; [xxvi]

In this chapter, Dr. Freireich reflects on leadership style, intellectual freedom, the legacy of Dr. Charles LeMaistre, Dr. John Mendelsohn, and MD Anderson in general.

Chapter 27: *The Development of the ALZA Infusion Pump*

The Researcher; Overview; Definitions, Explanations, Translations; The Professional at Work; Patients, Treatment, Survivors; Ethics; Cancer and Disease; Discovery and Success; MD Anderson History; Understanding Cancer, the History of Science, Cancer Research; The History of Health Care, Patient Care; On Texas and Texans; Personal Background; [xxvii]

In this chapter, Dr. Freireich talks about the development of the ALZA infusion pump.

Chapter 28: *The Physician-Scientist Training Program and Other Activities and Some Thoughts on Retirement*

Overview; Education; Character, Values, Beliefs, Talents; Professional Values, Ethics, Purpose; Leadership; The Professional at Work; Obstacles, Challenges; Institutional Politics; Controversy; Understanding the Institution; MD Anderson Culture; Working Environment; Institutional Mission and Values; The Researcher; Critical Perspectives on MD Anderson; MD Anderson History; On Texas and Texans; [xxviii]

In this chapter, Dr. Freireich talks about the Physician-Scientist Training Program at MD Anderson and shares his thoughts about retirement.

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[i] A: Personal Background; A: Educational Path; A: Character, Values, Beliefs, Talents; A: Inspirations to Practice Science/Medicine; A: Influences from People and Life Experiences; A: The Patient; D: The History of Health Care, Patient Care;

[ii] A: Professional Path; C: Evolution of Career; A: Military Experience; A: Inspirations to Practice Science/Medicine; A: Influences from People and Life Experiences; D: The History of Health Care, Patient Care;

[iii] A: The Researcher; A: Professional Path; C: Evolution of Career; A: Overview; A: Definitions, Explanations, Translations; D: Understanding Cancer, the History of Science, Cancer Research; D: Technology and R&D; B: The MD Anderson Brand, Reputation; C: Patients; C: Patients, Treatment, Survivors; C: Discovery and Success; A: The Researcher; A: Personal Background;

[iv] A: Professional Path; A: The Researcher; A: Military Experience; A: Professional Path; C: Evolution of Career; A: Personal Background; D: Understanding Cancer, the History of Science, Cancer Research; D: The History of Health Care, Patient Care;

[v] A: The Researcher; C: Portraits; C: Collaborations; C: Discovery, Creativity and Innovation; C: Discovery and Success; C: Professional Practice; C: The Professional at Work; A: Overview; A: Definitions, Explanations, Translations; D: On Research and Researchers; 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;

[vi] A: The Researcher; B: Overview; A: Overview; A: Definitions, Explanations, Translations; C: Discovery, Creativity and Innovation; C: Discovery and Success; C: Professional Practice; C: The Professional at Work; B: Multi-disciplinary Approaches; 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; B: Industry Partnerships;

[vii] A: The Researcher; A: Overview; A: Definitions, Explanations, Translations; C: Discovery, Creativity and Innovation; C: Discovery and Success; 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;

[viii] 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; 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;

[ix] 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; 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;
[x] 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; 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;
[xi] 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;
[xii] B: Overview; 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;
[xiii] 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;
[xiv] B: Building the Institution; 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;
[xv] B: Building the Institution; 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;
[xvi] C: Leadership; D: On Leadership; C: Portraits; C: Professional Practice; C: The Professional at Work; B: Growth and/or Change; B: Obstacles, Challenges;
B: Institutional Politics; B: Controversy;
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[xviii] C: Leadership; D: On Leadership; C: Portraits; C: Professional Practice; C: The Professional at Work; C: Education at MD Anderson; B: Growth and/or Change; B: Obstacles, Challenges; B: Institutional Politics; B: Controversy; C: Understanding the Institution; A: The Researcher; A: Overview; A: Definitions, Explanations, Translations; C: Discovery and Success; 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;
[xix] B: Building the Institution; C: Professional Practice; C: The Professional at Work; C: Education at MD Anderson; B: Growth and/or Change; B: Obstacles, Challenges; B: Institutional Politics; B: Controversy; C: Understanding the Institution; B: Critical Perspectives on MD Anderson; B: MD Anderson History; B: MD Anderson Snapshot;
[xx] B: Building the Institution; C: Professional Practice; C: The Professional at Work; C: Education at MD Anderson; B: Growth and/or Change; B: Obstacles, Challenges; B: Institutional Politics; B: Controversy; C: Understanding the Institution; B: Critical Perspectives on MD Anderson; B: MD Anderson History; B: MD Anderson Snapshot;

[xxi]A: Overview; C: Leadership; D: On Leadership; C: Portraits; C: Professional Practice; C: The Professional at Work; B: Institutional Politics; B: Controversy; C: Understanding the Institution; B: Critical Perspectives on MD Anderson; B: MD Anderson History; B: MD Anderson Snapshot;
[xxii] A: Overview; A: The Researcher; C: Leadership; D: On Leadership; C: Portraits; C: Professional Practice; C: The Professional at Work; B: Obstacles, Challenges; B: Institutional Politics; B: Controversy; C: Understanding the Institution; A: The Researcher; A: Overview; A: Definitions, Explanations, Translations; C: Discovery and Success; 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: Ethics;
[xxiii] A: Overview; C: Leadership; D: On Leadership; C: Portraits; C: Professional Practice; C: The Professional at Work; B: Critical Perspectives on MD Anderson; B: MD Anderson History; B: MD Anderson Snapshot; B: Institutional Politics; B: Controversy; C: Understanding the Institution;
[xxiv] A: Overview; A: The Researcher; B: Research; C: Leadership; D: On Leadership; C: Professional Practice; C: The Professional at Work; B: Obstacles, Challenges; B: Institutional Politics; B: Controversy; C: Understanding the Institution; A: The Researcher; B: Research; A: Overview; A: Definitions, Explanations, Translations; C: Patients; C: Patients, Treatment, Survivors; C: Ethics; C: Cancer and Disease; C: Discovery and Success; 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: On Texas and Texans;
[xxv] B: Building the Institution; C: Leadership; D: On Leadership; C: Portraits; C: Professional Practice; B: Growth and/or Change; B: Obstacles, Challenges; B: Institutional Politics; B: Controversy; C: Understanding the Institution; 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;
[xxvi] A: Overview; A: Character, Values, Beliefs, Talents; A: Professional Values, Ethics, Purpose;
A: Critical Perspectives; C: Critical Perspectives; A: Personal Background; C: Leadership; D: On Leadership; C: Portraits; C: Professional Practice; C: The Professional at Work; B: Obstacles, Challenges; B: Institutional Politics; B: Controversy; C: Understanding the Institution; B: MD Anderson Culture;
B: Working Environment; B: Institutional Mission and Values; A: The Researcher; B: Critical Perspectives on MD Anderson; B: MD Anderson History; B: MD Anderson Snapshot; D: On Texas and Texans;
[xxvii] A: The Researcher; B: Research; A: Overview; A: Definitions, Explanations, Translations; C: Professional Practice; C: The Professional at Work; C: Patients; C: Patients, Treatment, Survivors; C: Ethics; C: Cancer and Disease; C: Discovery and Success; 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: On Texas and Texans; A: Personal Background;
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C: Leadership; D: On Leadership; C: Professional Practice; C: The Professional at Work; B: Obstacles, Challenges; B: Institutional Politics; B: Controversy; C: Understanding the Institution; B: MD Anderson Culture;
B: Working Environment; B: Institutional Mission and Values; A: The Researcher; B: Critical Perspectives on MD Anderson; B: MD Anderson History; B: MD Anderson Snapshot; D: On Texas and Texans;

Emil J Freireich, MD

Interview Session One: 23 July 2001

Chapter 01
Growing Up in Chicago with Unusual Opportunities
A: Personal Background;

Codes
A: Personal Background;
A: Educational Path**;**
A: Character, Values, Beliefs, Talents;
A: Inspirations to Practice Science/Medicine;
A: Influences from People and Life Experiences;
A: The Patient;
D: The History of Health Care, Patient Care;

Lesley Brunet, MA
0:00:40.9
Why don't we start with your personal background and things that impacted your future career? You're from Chicago?

Emil J Freireich, MD
0:00:58.2
I was born and raised in Chicago. My earliest recollections were of my mother, Mary Klein Freireich. My father, David Freireich, died when I was 2. I had a sister, Annette Freireich Foosaner, 3 years older than me. I was born in '27. My father died in 1929, undoubtedly related to the great crash. My mother is an uneducated peasant girl from a small town in Hungary. My father, as far as I can tell from talking to my mother, was a much older man. He was older than her by maybe a decade. He served in the military and spent a long time in a prison camp with the Russians. When he got out, he went back to his village and swept up my mother, who came from a very large family. In the very large families in Europe, they didn't get a lot of attention. They fell in love, got married, and ran off to the United States.

Then my sister and I were born, and the Depression hit. My understanding from my mother is that my father opened a Hungarian restaurant in Chicago. When the Depression hit, he was totally wiped out, and then he died suddenly. My own theory is it was a suicide, but you can't tell. No one knows. My mother wasn't very intelligent. She had no relatives in the United States, and she spoke minimal English. My father had a brother here, but apparently, when he

died, they abandoned her. She was all alone with 2 children, so she went to work in a sweatshop, which is what everybody did. According to her, she earned 2 cents for every hat that she put a brim on. She used to do millinery work in Europe.

My first recollections of Chicago, where we lived in a very low socioeconomic area, are that I was raised by an Irish immigrant maid whose name was Mary. I don't know her last name. Mary worked for room and board.

Lesley Brunet, MA
0:04:01.6
She lived with your family for room and board?

Emil J Freireich, MD
0:04:03.7
Yes, for room and board; that was all. She had no money. She couldn't live anywhere else. She raised my sister and me. My sister was kind of a junior mother, and Mary supervised us. I don't remember how long during the day my mom worked, but it was essentially all the time, so I never really saw her much. We grew up in the community. People always ask me if I speak German or Hungarian. I never spoke to my mother or my father, so I just grew up, learning what you could.

Have you ever seen the TV show *Welcome Back, Kotter*? I love seeing *Kotter*. It reminds me of my primary education. In a low socioeconomic school in those days, education was essentially discipline. It was just a matter of keeping you off the street for 4 hours. There was a little bit of teaching, but nothing significant. It was mostly food, shelter, and not getting killed.

Lesley Brunet, MA
0:05:32.5
This was public school?

Emil J Freireich, MD
0:05:35.2
Yes. That's what my early days consisted of. Things got better in the mid-'30s, and I can't tell you exactly when, but my mother eventually had an arranged marriage with a man who had lost his wife and needed someone to take care of his son. They had this arrangement that they would live together if she would take care of the son, and he would give her some money. She could come out of the sweatshop, and she came home. This stepfather was an insurance salesman. He had a big, thick book, and he would go from door to door and collect a policy, which paid 25 or 50 cents a week. You're talking nickels and dimes, so we didn't have any increase in our standard of living. Both of them were destroyed people. When you emerge from horrible situations, you're destroyed, like veterans with their ugly memories. He was a terribly ugly person, and he used to work very long hours.

My mother stopped working at the sweatshop, and they didn't have any money, so she fired Mary. Whoa! Mary was really my mother figure, and when this strange lady came into the

house and got rid of my "mother" I was still very young. I must have been 9 or 10. We'd never had much of a relationship, my mother and I. I didn't care much for her. I never forgave her for firing Mary. We loved Mary. She disappeared, and there was my mother and this ugly, ugly man and his son, who was 6 months older than I. We were practically the same age. My sister was still in the picture.

We went off to middle school and high school. The marriage was impossible. No one could live with this guy. I went to the high school one day with my stepbrother, and I recall my sister coming to school, calling us out of class, and saying, "We no longer live at this place. We live in a different place." My mother had picked up all the belongings and moved out. My sister was working by that time. I forgot what she did; it was some menial job. My mother was working as a salesperson for some department store, and we had enough money to live independently. My stepbrother elected to come with us, because he couldn't stand his father, either.

My life really began during high school. Before the Depression, if you lived in a low socioeconomic area, the children only saw women. There were no men, because the old family structure was still intact. The men worked, and the women stayed at home with the children. But during the Depression, the women got whatever jobs they could, and the men were all in the sweatshops working their heads off. So as a young person growing up, the only people you were in contact with were women. You never saw any men, except occasionally a baseball player on the news or something like that. We only had radios, remember.

When I was ill, we had a family physician living in the community who was a *Tree Grows in Brooklyn*-type. He was one of these guys who worked for exchanges. People gave him food and occasionally a dollar, and he lived and cared for everybody in the community. Everybody knew him. When I got sick with tonsillitis, he would come to the house and tell my mother to give me ice cream. I just loved Dr. Rosenbloom.

Lesley Brunet, MA
0:10:17.1
His name was Rosenbloom?

Emil J Freireich, MD
0:10:19.2
Yes, Rosenbloom. It might have been Rosenburg or something. I think Rosenbloom. I used to have dreams about being a famous doctor when I was around 11 or 13. I used to dream of being like Dr. Rosenbloom, going to people's houses and administering to children and adults. I thought that was a great thing. What else could you be? I didn't have any other ideas.

So that was in the back of my mind, and then I went to high school. As I say, all you thought about was survival. We used to have gangs, and people got beat up. We had to shop for food, so we had to steal tires, hubcaps, and anything to get along. In high school, I was still prepubescent. I don't know if I was a problem or if I was brilliant, but the teachers had advanced me so that I finished high school when I was just 15. I was short, fat, and kind of easy to pick on. I was the kind of kid that other kids would kick around.

In high school, my mother and my sister said I should take shorthand and typing, which I did. That was the major way you made a living in those days. You did shorthand and you did typing. But you also had minimum requirements, so I had to take a course in physics. In physics, we had a teacher who was an educated man from a better socioeconomic class. He deliberately taught in the low socioeconomic schools because he wanted to find the gems there. He taught a physics course, and it was really interesting. It was the first time I found a course that I felt I was really learning something. He was telling us about the laws of motion and this and that. Then he had a science fair, which is what teachers would do. You want to stimulate kids to think. I did a project on the Bernoulli theorem. I can remember it like it was yesterday. The Bernoulli theorem is very simple. If you're dumb and uneducated and barely read and write, you know what the Bernoulli theorem is. It's the basis of flight. It's the fact that the pressure is a function of how fast the gas moves.

The Bernoulli principle appealed to me, so my science project was a jet of water with a Ping-Pong ball. You've seen that. It won't come off, because when it goes off to one side, it goes faster along the light side. So I did that and won the prize for the best science project that year. My professor called me in. I've forgotten his name. He said, "Freireich, you're pretty smart. You ought to go to college." I said, "That sounds like a reasonable idea." I really liked him, and I said, "What is college?" He said, "Well, there's this place in Champaign-Urbana." You see, during the Depression, it's like King of Siam. Your world is 5 square miles. We didn't have cars. We didn't have transportation. You couldn't imagine that there was anything outside of that little area. I used to walk to school and back. I would occasionally ride a streetcar to the baseball park to see the Cubs, and to the lake to swim, but that was the world. "Champaign-Urbana? Where is that?" "Well, it's 150 miles away in the middle of the state," he said, "but it's very inexpensive to get there. You just take this Illinois Central. It goes right to Champaign-Urbana, Illinois." I went home, and I said to my mother, "My teacher says I should go to college." She said, "Well, what do you do about that?" I said, "Well, I'm supposed to have $25." "Twenty-five dollars? Well, we'll see what we can do about it."

My mother had gotten attached to her little community, and within this community there was a lady who was a Christian Scientist. She had a husband who died and left her some money. She didn't have any children, and she was doing good things. My mother had heard about her. She put on my best shirt, and we went off and got interviewed by this lady. She said, "You look promising," and she gave us $25. When the time came, I took my laundry bag and got on the Illinois Central Railroad. It was $6, one-way, to Champaign-Urbana. I got off the train in Champaign-Urbana, and asked someone, "Where do I go?" "Well, you go see the registrar over there and get registered." So I went over there. They asked me, "What do you want to do?" "I want to go to school." "Well, did you send your transcript down or an application?" I said, "Well, I didn't know anything about that. My professor told me if I just have $6, I'd be here." He said, "Well, don't worry about it. We'll take care of that. What do you want to be? What do you want to major in?" I said, "Well, I want to be a doctor," the only thing I could think of. He said, "Okay. Premed." He gave me my curriculum things, and he said, "I'm going to register you on a temporary basis." This was in 1943. I gave him $6, and I registered. He said he would contact my high school, but it turned out that our high school didn't send the transcripts because

they didn't know how to do it. Not many people went to college from Tuley High School in Chicago.

I've been back to Tuley High School. It's now called Roberto Clemente High. The Tuley High School has been closed. Where I lived was mostly European immigrants, and now it's been replaced by Puerto Ricans. You know the Los Angeles riots and Watts? In Chicago it was North Avenue, Humboldt Park. That's where the riots were. They burned down my old neighborhood. It's been rebuilt, to some extent.

I had to get some books, and I had to get a place to live. They had a housing guy who sent me to see this lady. She put me up in a bedroom in her house for $6. After this, the train fare, and tuition, I only had $6 left. I've got to get books, I've got to get food, and I have to pay my rent in the future. So I went to work at a sorority house, cleaning floors. I got $6 a week, and that paid my rent. Then I got a job that got me food for waiting tables, and that paid for my food.

In 1944, when you turned 18, you got inducted into the army. I got notification that I would go to the army. I stopped studying. I said, "Hey, I'm going to be military. I'm going to kill Japs and Nazis." Young kids were very idealistic during the Second World War. We hated Nazis and Japs. When you get the pre-induction physical, you line up, they take all your clothes off, and make sure you don't feel like a person anymore. Then they start poking on you all over. After they're done, at the last stop, the guy says, "Has anyone here ever broken a bone or had any trauma?" I had an illness, which is called Osgood Schlatter disease, when I was in high school. It's a separation of the tibial tubercle. I used to play basketball, and when you're growing, during a growth spurt, your epiphyses are soft, so if you do a lot of jumping, you can separate the tibial tubercle. In those days, they used to put a cast on it, and I walked on a cast. So I said, "I broke my leg."

I went into the next room, and there was this guy who came straight out of *M*A*S*H*. He was smoking a cigarette and was a dirty, filthy guy. He was a foul-mouthed guy. He said, "What are you doing here? This war is stupid. You're a bunch of ugly, dumb people." He terrified me. I mean, I was scared. I was a patriotic American. I want to kill Japs.

So you go to the next room, put your clothes on, and then you come to the end of the line. There's a guy there, and he gives you your paper telling you when you're supposed to report for duty. I was classified 4-F. That means it's physically impossible to serve in the military. That was a humiliation, because 4-F guys were all guys whose parents had influence and convinced the draft board to make them 4-F so they don't have to get killed in the war. Those people were considered traitors and weaklings. I was really humiliated. I'll never forget that. So I had to go back to college and finish school.

Lesley Brunet, MA
0:21:46.5
Was this in the summer or in the middle of the school year?

Emil J Freireich, MD

0:21:56.2
It was in the middle of the school year. I had to go to Chicago to get inducted. Then I had to go back to Champaign. I had to catch up. It was second semester, and I didn't have any money for books, so I went back to the guy, and I said, "I don't have any money for books." The guy said, "Well, you apply for this scholarship." I applied for a scholarship, and I got it, because I got A's in my first year. It was a William J. Cook Scholarship, and they paid my tuition and my books. Now I'm on easy street, because I'm earning $6 a week and I had tuition and books paid. I was in good shape.

One of the fellows I met in my class was a guy who had polio when he was young. He became a very good friend of mine. After the induction thing, I went back to school, bemoaning the fact that I was 4-F. He said, "You know, nothing wrong with being 4-F. I get money from the Illinois Department of Rehabilitation because I have polio. They buy all my books, and I get an allowance every year. It's terrific. They want me to be a normal person." He said, "My counselor is coming around next week. I'll have him come and visit with you."

Lesley Brunet, MA
0:23:12.6
Because of your leg?

Chapter 02
Medical School and Residency in Chicago and a Growing Interest in the Science of Medicine
A: Professional Path;

Codes
A: Professional Path;
C: Evolution of Career;
A: Military Experience;
A: Inspirations to Practice Science/Medicine;
A: Influences from People and Life Experiences;
D: The History of Health Care, Patient Care;

Emil J Freireich, MD
0:23:14.1
Yes. There was nothing wrong with my leg. I was doing fine, but I was 4-F. The counselor comes around, and he interviews me. "Yeah, I'm 4-F. Here are my papers." He says, "Okay. You're eligible for rehabilitation." Now, this is great stuff, because it turned out that to go to medical school, tuition wasn't $6, it was $1,000.

Lesley Brunet, MA
0:23:33.5
A semester?

Emil J Freireich, MD
0:23:35.1
It was $1000 a year. Now it's $25,000. It being $1000 a year, there was no way I could go to medical school. I asked the William J. Cook Fund for money, and they said, well, they could do the books, but they couldn't pay tuition. They didn't have that much money. So this guy saved my life. I was on rehabilitation. I owe my MD degree to the taxpayers of the state of Illinois who paid for my tuition and my books.

I went to medical school in 1945. I intended, from day one, to be a family doctor. I wanted to be like Dr. Rosenbloom. There were a lot of veterans getting discharged at that time. We were scaling down. When the entering class got addressed by the president, he said there were 200 applicants for every position in the University of Illinois. It was very competitive, and they were all military guys. The V-12 and the ASTP were still functioning. Half the class was active military, the other half were veterans, and there were a couple of 4-F guys like me.

Medical school was kind of uneventful. It was in Chicago, so I lived at home with my mother and my sister. I used to ride the L every morning to go to school. It turned out I did pretty well in medical school in terms of grades. When it came time to graduate, you had to pick a place to

be an intern. The University of Illinois has a university hospital. It's kind of like our medical center. Across the street is the largest city-county hospital in the country, Cook County Hospital. Cook County Hospital was the teaching hospital for University of Illinois.

We had the largest medical school class in the country in 1945. Even in 1995, they had the largest medical school class in the country. University of Illinois is a very socially conscious medical school. They want to produce. They want to get poor people, get them educated, and get them back into the lower socioeconomic neighborhoods. They trained a lot of blacks. We had the largest female class in history. In my entering class, we had 30 women and 200 students total. They had a lot of students for the research hospital, so we all trained at Cook County.

It was an interesting experience, because the teaching was done by the house staff. The house staff is very pragmatic. "This is how you do this. This is how you do that." You get theory in your classes, but you go to County and get your hands dirty. When we had to choose our internship, I decided I wanted to go to Cook County. They had an exam, and it was very competitive to be a County intern. I didn't make the first cut, so I was very depressed. My second choice was Michael Reese. That was a private hospital, which was our other teaching hospital. Michael Reese had a lot of very sophisticated scientists. I was beginning to get interested in the science of medicine. I was doing well in school, and I ended up sixth in my graduating class. The science of medicine was appealing to me, but I still wanted to be a family doctor.

It turned out that someone turned down the job to take something else, and I got accepted at Cook County Hospital. I was thrilled and happy to start my internship at Cook County Hospital. At first, having a lot of responsibility over life and death and a lot of power is very appealing to young people who have been students, standing around and watching, but after a while, you begin to recognize your limitations. Being an intern was fun. I did OB; I delivered 100 babies. We used to admit 25 patients a night on Internal Medicine.

Things were going along pretty well, but it was obvious that after a year I had learned everything I could in that place. We were just dealing with things like gunshot wounds, stabs, and septic abortions. To be a family doctor, I thought I ought to know something a little better than that. I had a couple of confrontations with the administration about things that I didn't like the way they were running, and I was the intern. I was supposed to be in charge. I had a couple of spats with my resident.

Finally, on Medicine 55, I had a confrontation with the head nurse. I had a patient that I spent a lot of time on. A guy came in with heart failure, and I had learned a lot about heart failure and how to give mercury, digitalis, dehydrate, and control his rhythm. I spent a lot of time on him, all night long, and I'd finished. We used to work 36 hours on and 12 hours off. I got some time off, and I came back after my 12 hours off. "Where is Mr. So-and-so?" "Don't worry about Mr. So-and-so." I said, "Where is Mr. So-and-so?" "Well, he's in Room 1822." So I went into Room 1822, and it turned out, unbeknownst to the house staff, the nurses ran a hospice, but it wasn't dignified. It was a room where they put the patients they decided were hopeless. They

moved them into this room, bumper to bumper, until they died. They just lie in their feces and urine and die.

This guy was lying in that room, and I walked in, and I said to the nurse, "This is ridiculous. I didn't tell you to put this patient in there." "Dr. Freireich, you're just an intern. I've been here 20 years." The administrator called me in, and he said, "I think you ought to leave." I'd signed up for a 2-year internship, but I said, "Well, I agree." I was depressed that I was fired, but it's been a pattern in my life. I've always been fired. It's kind of like going to school. Most people think that they have a goal and they pursue it, but really you move around in the environment in which you're directed. Virtually everything in my life has been decided by others who are smarter than me.

Chapter 03
On to a Fellowship in Boston and to Hematology
A: The Researcher;

Codes
A: The Researcher;
A: Professional Path;
C: Evolution of Career;
A: Overview; A: Definitions, Explanations, Translations;
D: Understanding Cancer, the History of Science, Cancer Research;
D: Technology and R&D;
C: Patients; C: Patients, Treatment, Survivors;
C: Discovery and Success;
A: Personal Background;

Emil J Freireich, MD

I looked around the medical center. Don't forget, the world is Siam. There was Presbyterian Hospital, which is a private hospital. Presbyterian Hospital had a very famous clinic called the Rush Medical Clinic. It was a medical school. It shut down and became a teaching school, University of Illinois. It's now a medical school again; they resurrected it. At the time it was just a big, fancy private hospital where rich people from the suburbs went to get care. They had just recruited a very flamboyant new chairman of medicine from Boston. His name was Howard Armstrong. He was the messiah who was bringing the new medicine of Boston to Chicago, because in 1950 there was a scientific renaissance in medicine in Boston that was pioneered by a lot of people. Soma Weiss was one. They introduced physiology and science into medicine and began to develop treatment for diseases. It all came out of the war and the malaria program.

Dr. Armstrong had worked with Dr. E. J. Cohn on fractionating blood and developing a replacement for blood for the battlefield, and he had purified albumin. He was quite accomplished as a young man. He was probably only 40 or 45, and they made him chief of medicine to bring this new science to Chicago in Presbyterian Hospital. He was recruiting, so I walked into his office, and I said, "I've been at County for 18 months. I want to be a family doctor, but I've learned everything I can at County." But internal medicine is very complicated, and there's so always something new. EKGs had just been introduced and cardiology and renal function. They had just worked out GFR and creatinine clearance. I told him my medical school hadn't prepared me for the complexity of medicine, so I wanted to do some more medicine. So he said, "Fine," and he hired me. In a year I was the chief resident in the teaching service. I did very well, but I still wanted to be a family doctor.

As fates would have it, Dr. Armstrong got fired by the board. He was fired for a couple of reasons. The major reason was what he did to the house staff; that is, he created a teaching

service like they have in Boston, which they've never had before, and the teaching service had its own practice, which came out of their clinic. These are free. It was called the Central Free Dispensary. He had created a teaching service where the patients were indigent, and it was run by the house staff, but not by County Hospital. It was run by the house staff, with teaching by the full-time faculty. He had recruited 2 full-time faculty members from Boston, so they had 3 professors. Those 3 guys were teaching on a full-time basis. When you make rounds with a private doctor, you're doing conventional apprentice medicine; that is, you just follow what they do, and you do it. If you say to Dr. Sippy, "Why do you stick a tube down his stomach every morning to get rid of the acid?" he says, "Look, Freireich, just do what you're told and shut up." The teaching service was very stimulating, but the consequence was they didn't have any house staff to work for the private doctors. They all gravitated to the teaching service, obviously. So the private doctors said, "Well, it's nice to have Dr. Armstrong, but what he's doing is interfering with our practice. We make the money. They're not making any money because they're all indigents." It was the MD Anderson thing all over again. So they fired Armstrong.

When he got fired, the hospital director called me in. This is early 1950. He said, "Freireich, you're the chief resident on the teaching service. You've attended on all the private services in the year and a half you've been here. You probably have learned everything you need to know." I was fired again. So I went to see Dr. Armstrong. I said, "I've been fired. What do I do?" He said, "Freireich, what do you want to be?" I said, "I want to be a family doctor." He said, "Well, I have heard from all your attendings." I remember Oglesby Paul, a very famous cardiologist. I loved cardiology, EKGs, and all that stuff. I loved neurology. I loved diabetes care with Roland Woodyatt. I loved cardiology with James B. Herrick, who discovered myocardial infarction. That all happened in my residency days, so I was really getting interested in medicine. They all thought I was great. I was a good resident, smart, I worked hard, and I took good care of my patients.

Dr. Armstrong said, "You know, this community is too provincial. You've got to go to Boston to learn the new medicine if you want to be a good doctor. You've got to go where the science is. You've got to learn that stuff. You're not going to learn it in Chicago." I said, "Okay. I'll go to Boston. What should I do in Boston?" He said, "Well, what do you know the least about?" I said, "Dr. Armstrong, I don't want to be offensive, but you've got a rotten hematology program. I mean, I don't understand these black and blue spots. Hematology is very complex, and there's all this turnover in red cell metabolism. I just don't understand it. So I think hematology is my weakness." He said, "Fine."

He wrote me 3 letters to the 3 most famous hematologists in the United States of America: William Dameshek, founder of the journal *Blood* and the first formal hematologist; Joseph F. Ross, who was the first hematologist to do metabolic studies. You see, in 1950, we didn't know that proteins had a life span, red cells had a life span, and white cells had a lifespan. All that turnover business was a product of the war, because we had radioisotopes. Dr. Ross was a pioneer of radioisotopes. And he sent me to the third guy, whose name I can't remember. I think it was Israel or something. I had 3 letters of reference.
I took everything I owned and threw it in my car. I had a 1946 Oldsmobile. That was the first postwar automobile. I drove off to Boston, interviewed with the 3 guys, and they all offered me

a job. The only problem is this was 1950. Nobody paid residents or house staff; you worked for nothing. But Dr. Ross had just gotten a new grant from the Atomic Energy Commission to study red cell metabolism with radio iron, and he offered me a job that paid $3,000 a year. That was big money, so it was an easy choice. Dameshek was very famous, but I went with Joe Ross, and I began working.

Lesley Brunet, MA
0:08:39.2
Was it the money or the position?

Emil J Freireich, MD
0:08:45.1
It was just the money. I would've rather gone with Dameshek, but I couldn't live. He didn't pay his house staff. It was very competitive, and I had nothing to live on. I had no relatives or friends in Boston. I didn't get any money from my mother, and my sister was also poor. I had no money—zero. I had to live on my own resources. I could have gotten a part-time job, but I wanted to devote myself to my research. I'm a hick from the country, the small town of Chicago, and here I am in the big metropolis of Boston. It was money.

I went to work with Joe Ross. That turned out to be a very exciting period. I worked on a very exciting project, my first research ever. Up until then, I was strictly a trainee. The project I worked on was the mechanism of anemia in rheumatoid arthritis. It turned out that there was a paradigm that was created by Maxwell Wintrobe, who is the author of the first textbook of hematology in the world. He was the authority on the mechanisms of anemia. We did some very innovative things with rheumatoid arthritis. Then another fellow in the lab, Aaron Miller, and I got the idea of doing some experiments with a dog, which would nail the problem, and these turned out to be tremendously successful experiments.

My first research projects were enormously successful. There is what's called regular science. Regular science is what you do when you write a grant, you get funded, and you do it. Regular science is all very predictable. Here's this amount of knowledge. If you want to find out if this amount of knowledge will take you a step further, you do the following. There are only 3 possible things you can do, you write it down, you have the facts—that's regular science. But discovering something is a whole different process. Discovering something is tackling a problem that you have no idea what the outcome is going to be. You never get funded for a grant. If you write down, "I want to cure leukemia," the study section will say, "Fishing expedition." You won't get a nickel. It's one of the problems with the way we fund research now. Young people are trained to do what everybody else does. It's like the military, but they're not challenged to think.

It's been established by formal study of scientists who are successful that the secret to a successful career in science and innovation is to hit a home run the first time at the plate, because if you face a number of defeats, you immediately recede into the mode of doing what everybody else is doing, which is regular science. You get funded, you get promoted, and everything is fine. There is no controversy, and you never have to fight a battle. But if you hit a home run, if

you discover something, 2 things happen. The first is that you become controversial, because if you discover something and you're young, this is bad. How come all the experts didn't know that? That's what happened to my first discovery.

We made a breakthrough discovery in the mechanism of anemia infection, but it conflicted with the Wintrobe conclusions. Wintrobe had written 15 consecutive papers on the mechanism of anemia infection. We had proven that the formulated thing was false, and we knew how it happened. We wrote this paper, and it was rejected by the journals, of course. Innovation is always rejected. So we had controversy. How do we get this paper published? I must admit, without Joe Ross's backing, we never would have gotten published. Dr. Ross wrote to the editor and said, "I've gone over this data, I know it's correct. I don't care what Dr. Wintrobe says, and I think it should be published." He was in the Association of American Physicians. He was a famous guy, so we got it published.

At this point, I'm still a fellow. I hadn't gotten a staff position anywhere. I'm still learning. But I realized then that there is enormous reward in discovering something. You can't be admired. It's really a thrill to discover something. It's one of the high points of human intellectual activity, to discover something no one else knows. I hit this home run, but it's still controversial. I'm working my tail off, and I'm a fellow at Mass Memorial.

I should tell you about my wife, Haroldine "Deanie" Cunningham Freireich, who's very important to me right now. We've been married 48 years. She was the head nurse at the clinic in Chicago. When I moved to Boston, all alone, working my head off, and no money and no entertainment, she came to visit, and I took her to the lab. While we were touring the hospital to show her how important I was, someone broke into my car and took all of her luggage. She didn't have her tickets or anything. So to make a long story short, she stayed in Boston, we got married, and we've been together ever since. She's very important in my life. We had 4 children. She's made me acceptable to the world; otherwise, I'd be unacceptable.

So here I am, working my head off. I still want to be a family doctor. I'm just doing research because that was the job they told me to do. I'm learning hematology, and I'm really learning it. I took my boards in internal medicine then, because I figured you can be a family doctor, but if I have boards in medicine, I can charge more. So I took my board exam, and William Castle examined me. He's one of the giants of American hematology. What a man. I still quote him every day. There I was in the Mecca of all new science, and I was learning science. I was excited. That was in 1953.

Chapter 04
Oncology Research at a New Hospital at the National Institutes of Health
A: Professional Path;

Codes
A: The Researcher;
A: Military Experience;
A: Professional Path;
C: Evolution of Career;
A: Personal Background;
D: Understanding Cancer, the History of Science, Cancer Research;
D: The History of Health Care, Patient Care;

Emil J Freireich, MD

In 1954 the United States Congress, passed a law that the doctor draft was illegal. A number of physicians who had been drafted out of their careers went to the courts, and the Supreme Court ruled that the doctor draft was illegal because you were conscripting a person against his will, based on his profession, and that's a bias that could not exist, even though the military had no way to get doctors. But that resulted in them creating the military medical school in Washington, DC, so that the military could train its own doctors rather than draft doctors.

At the time that bill was passed, the military had no doctors, so in order to continue their military medical services, they drafted every physician who was eligible to be drafted but had not served, and I was one. I was deferred because of the Atomic Energy Commission thing that Dr. Ross had. Unbeknownst to me, when the doctor draft began in '53, Dr. Ross wrote a letter saying I'm essential to the war effort because I was working on radioisotopes, so I was drafted. I became a second lieutenant in the army. The military drafted everybody, but they didn't need everybody. You got commissioned, but you didn't have any orders. In other words, they had a pool of several thousand, and when they had a vacancy they called. So I accepted my commission, but I didn't have a report date. I got a call from Dr. Chester Scott Keefer, who was the dean of our medical school, Mass Memorial Hospital, Boston University School of Medicine. In those days, we were very respectful of our seniors, and Dr. Keefer was so brilliant, it was unbelievable. He was smarter than Howard Armstrong. He was someone everybody admired. He was a man of high quality, high character, and enormous intelligence, and he was hardworking. He taught the students every day. But he was very demanding of discipline. When the house staff presented a case to Dr. Keefer, you had to have your shoes shined. You had to have a clean uniform on. You had to have a tie. You had to have your hair fixed properly. The nurses were all meticulous. The beds were all made. The patients were ready. "Dr. Keefer's coming." It was like the Pope.

Dr. Keefer called me to his office. Now, not many people got in to see Dr. Keefer. It's like going to see the Pope. You don't get a hearing with the Pope very often. I walked into his office, trembling. "Dr. Freireich, Dr. Ross tells me you're doing a very good job." "Thank you, sir." "Have you ever heard of a place called the National Institutes of Health?" "No, sir." "Well, let me tell you. There's this place in Washington. The government has decided that they want to put a hospital in the middle of the National Institutes of Health."
During the Eisenhower administration, Eisenhower created the Department of Health, Education, and Welfare. He merged Education, Welfare and Health into a single department, and the first secretary of that department was Oveta Culp Hobby. Oveta Culp Hobby was a publisher, and she knew that she didn't know anything about health, so she created a new position called the Undersecretary for Health, which still exists today. The surgeon general currently holds that post. The first person appointed to that was Chester Scott Keefer, but he accepted the job on the condition he not give up his deaning. He spent 3 days in Washington with the bureaucrats, and then he came home over the weekend and spent 2 days in Boston with his medical school. He had a hot phone, and he worked both jobs simultaneously. His job was to staff the Clinical Center in the National Institutes of Health. It opened in the fall of 1954, and they had a handful of doctors they recruited from Public Health Service units. None of them had done research, just 1 or 2. They had to staff it somehow. Keefer got the idea of finding young people who had been drafted, and sticking them out there to see if they could do anything.

"No, sir, I haven't heard of it." He said, "Well, they have this new hospital there, and there's a big opportunity for people who have talent, like you. So what I want you to do is go up there." He made a telephone call. "Fred? I have a young man in my office named Freireich. He'll be there tomorrow morning." Then he told me, "What I want you to do, Freireich, is I want you to go to Washington tomorrow and see Fred. And here's how you get to Fred in this office, and he'll take care of the rest." "Yes, sir." You wouldn't have an argument with Dr. Keefer.

I left there, and I called my wife and said, "Deanie, I have to go to Washington." We had our first daughter, Debra Ann Freireich. We had her 14 months after we got married. She was not planned. We were poor. My wife worked at Mass General for $3,000, so we had $6,000 a year. We lived in a fourth-floor walkup. My son David Alan Freireich was born 14 months after my first baby. So she was pregnant at the time. She lived in this place, and they didn't turn on the heat. She had to walk up 4 stories, pregnant, to do the laundry, and carry the baby. We struggled. She struggled more than I did, but she said, "Okay." So I got on a bus, went to Washington, and took the streetcar to this place. The guy says, "Go to NIH. It's here. You go there and walk around and talk to people."

I met a guy named Gordon Zubrod. Gordon Zubrod was a Johns Hopkins alumnus. The NIH director at the time was a guy named James Shannon, who was an MD who worked in the malaria program. The malaria program was really revolutionary for medicine, because, as you know, more military died of malaria during the war than of gunshot wounds. We would have lost to the Japanese if we didn't conquer malaria. What won the war was beating malaria, and the way they did it was a crash program, like the atomic bomb program. There are a lot of skeptics who don't believe that you can cure things with money, but the fact is you can.

They had a crash program on malaria. They had to work out something that had never been done before. They had to get the organism. They had to work out a model system. They had to get a screening system. They had to synthesize chemicals. They had to do a clinical trial on people. All this technology of research in medicine was worked out during the war, something that didn't exist before the war. Then they opened the Clinical Center out in Bethesda, in the middle of nowhere. It was really the middle of nowhere. This is in 1954. Now, in 1954, this is 12 miles away from Washington. It was still totally segregated. They had black fountains, white fountains, no department stores, and no apartment buildings. It was a burg, believe me. Bethesda was in Maryland, in the country. It was just trees and grass. The nearest thing was the Naval Medical Center across the street, and it was a military operation. As far as a hospital, you were 40 minutes from any hospital where there were any medical people. Medical schools were downtown Washington and Baltimore: Hopkins, University of Maryland, George Washington University, and Georgetown University.

The idea was to create a place which was not an academic medical center. You're not teaching. You're not doing service work. You're just going to do research, like you do in your laboratory. It's a hospital. You've got beds, laboratories, and you cure disease. Keefer thought that was a good idea.

Gordon Zubrod was in the malaria program with James Shannon. After the war, Gordon Zubrod accepted a job at St. Louis University, which is a Catholic school. In the postwar boom, when all the medical schools realized that all these veterans were coming back and the schools were booming, they recruited a guy named Phil Tumulty from Johns Hopkins, and he recruited Gordon Zubrod. They began to do real science and clinical trials. The first formal clinical trial was published in 1948. Remember now, this is '54. They were busy just using antibiotics. Dr. Keefer was famous because he ran the penicillin program. During the war, penicillin, sulfonamides, streptomycin—it was a dynamic area for the treatment of disease, and science was coming into medicine. They were very academic, and they went into this medical school. If you're going to be innovative, you create antibodies, so they created a lot of antibodies, and Dr. Tumulty got fired. Like Dr. Armstrong, Dr. Zubrod was there, and Tumulty was gone. He had to look for a job, so he found this opportunity at the Cancer Institute. There was Dr. Zubrod. He was head of the medicine branch. The Cancer Institute had surgery, medicine, and radiotherapy. They didn't know what to do, any of them. It was a bunch of empty beds and empty labs in an empty hospital.

The NIH consisted of the Public Health laboratories, which were where all the basic science laboratories run by the government were, and it was all started by an epidemiologist. They began by discovering that the cause of pellagra was the food and discovering that infectious diseases were polluting the water. It was a Public Health Service organization, and there were a lot of basic scientists working on mice and screening and things like that. Then all of a sudden, they put a hospital in there. Now you're going to have doctors and patients. That was a new idea. It opened in '54, and Zubrod was the head of the medicine branch. I walked into his office, and he says, "What do you do?" I said, "I'm a hematologist. I have spent 2 years with the great Dr. Ross in Boston. I'm an expert."

Zubrod was a wonderful, wonderful person. He's dead now. Everybody adored him. He was one of these extremely nice people, and he was very smart and very motivated. He said, "You know, I took this job because I think that what we learned in curing malaria can apply to curing cancer. I think we can develop model systems of cancer in experimental animals. We can get large numbers of chemicals and screen them against these model systems. If they work, we can do clinical trials in man and find out if we can cure cancer. I think we can."

This was in 1954. At that time, the only treatment for cancer was surgery. When Dr. Gilbert Fletcher came here, the surgeons thought radiotherapy was a joke, but by that time radiotherapy was doing a little. Fletcher would claim they could cure a few cancers, but as far as metastatic cancer, forget it. That was hospice stuff. In fact, in medical school you weren't even taught anything about cancer. Zubrod said, "I think we can cure cancer. You're a hematologist? Cure leukemia."

Lesley Brunet, MA
0:30:42.6
That's what he actually said?

Emil J Freireich, MD
0:30:44.0
That's what he said. "Your job. Cure leukemia."

Lesley Brunet, MA
0:30:47.6
So that's what you did.

Emil J Freireich, MD
0:30:49.9
He told me what to do, and I said, "Yes, sir." You could either go to Bethesda and take care of leukemia patients or you could serve in the military and go get blown up in Korea. So what the heck? Cure leukemia—that's what I'll do. Then I went back to Boston on the bus. Forty-eight hours later, I received orders to report for active duty at NCI.

Even though I had a commission in the army, I didn't have orders to report for duty, so I still had an interval where I could accept a commission in the Public Health Service. They raced my commission through in 48 hours, FBI check and everything, and I had orders. Forty-eight hours after I went to Bethesda, I had orders to report for active duty. Then the Public Health Service sends a copy of my orders to the army, and that eliminates my army commission because the army can't give you orders if you're already serving on active duty in a military service. During the Second World War, the Public Health Service was a military service. It was part of the navy. We went in at the rank of second lieutenant. I was a Public Health Service officer, and I had orders. Forty-eight hours after I came home and told my wife about the interview, I had orders.

Lesley Brunet, MA
0:01:03.4

What did your wife think?

Emil J Freireich, MD
0:01:04.5
I had 4 days from the time I went to Bethesda until I had to go to Washington. It was total hysteria. The next 2 days we didn't sleep. First of all, I had to get all my notebooks together. I had to finish my research. I had these 2 great things I had to publish that were in the mill. I had to finish the experiments that I had done. We had to get all of our possessions together, and we didn't have any money. I got my terminal check from my $3,000 thing. I had to resign. I went through all these shenanigans. In 48 hours, we had everything we owned in my 1946 Oldsmobile, including my 5-months-pregnant wife and my 19-month-old daughter. We had everything piled in, and we drove to Washington.

I had been to the NIH, so I had a notion as to how to go. We had a map, and we found Bethesda. Then we found the navy and the NIH building. This was cold turkey, with no money, no nothing. I said, "That's the building where I saw Dr. Zubrod." I pulled up there, and the guy said, "You can't park there." I said, "Well, I've got to report for active duty." He said, "You can park there." I took the elevator to Dr. Zubrod. "Reporting for active duty, sir." "Welcome aboard, Freireich. We'll get right to it. We've got an office for you, and you can start working today. Your office is on the twelfth floor." He was on the tenth floor. "You go up 2 floors. You walk down the corridor." "Yes, sir." He said, "Have you got a place to stay?" I said, "No. I was just here 2 days ago." He said, "Well, take your time. Go find a place to stay. Relax and come to work tomorrow. Take the rest of the day off." "Yes, sir." I'm really frightened. I'm just a kid. I was only 28 years old.

I went up to see my office. I'm walking down the corridor, and I'm looking at the names on the doors. I come to a door, and it says "Emil Frei III." That's just like the government. They can't even spell my damn name. I walk in the door, and there's this skinny guy with no hair. I said, "Sir, you're in my office." He said, "No, no, no. What's your name?" "Freireich." "No, no. I'm Emil Frei. Your office is next door." That's how I met Tom Frei, and he was the biggest factor in my career from that day forward. That was 1955, sometime in February.

Frei said, "Well, look. You haven't got a place to stay. You go down here." He'd just arrived. He came from St. Louis with Zubrod, and he had arrived in the fall, so maybe 3 or 4 months before I did. He already had a place to live, a house that he rented, and he had a family. He had 5 children and a wife. He was about 3 years older than I am.

He said, "You go to this office, and they have a list. You find a place, and you can rent a place for a couple of days, and then you can look for a house you can rent." He said, "Why don't you come over to our house for dinner?" I said, "Great." So that was that. I went downstairs, and there's my wife in the car with the crying baby. I said, "I've got an address. We can find a place here. We go here."

We found a place. It was a lady who had a basement that had been modified, and you had to walk down underground. We had this basement apartment with windows. We rented that for

whatever money we had. It was furnished. I went to the Freis' house for dinner and went to work. That was NIH. Soon after that, we rented a house from a Foreign Service guy who was on assignment, and he gave it to us at a very low rate. Our salary was $5,600. We'd been making $6,000, so we were still in trouble, but we had several advantages. We had the PX. Everything was kind of cheap in Maryland. It wasn't like downtown Boston. We were out in the middle of nowhere, and the rent was low, so we made it.

Chapter 05
Treating Leukemia at the NIH
A: The Researcher;

Codes
A: The Researcher;
C: Portraits; C: Collaborations;
C: Discovery, Creativity and Innovation;
C: Discovery and Success;
C: Professional Practice; C: The Professional at Work;
A: Overview;
A: Definitions, Explanations, Translations;
D: On Research and Researchers;
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

That started our career at NIH, and we were there for 10 years. That was a fantastic 10 years. During that 10-year period, all of American medicine was revolutionized between ’55 and ’65. During that 10-year period, the things that I’m famous for were all done at NIH, and there are long stories about how that all occurred. Tom Frei was my boss. He was an enormously dignified, intelligent, accomplished man. He’s just a wonderful person. We are still very good friends. We always will be. He said, “Freireich, you take leukemia. I’ll do the solid tumors.” So I was in charge of leukemia from day one. The first thing we had to do was we had to get some patients.

Lesley Brunet, MA
0:07:39.3
Where did the hospital get their patients?

Emil J Freireich, MD
0:07:43.2
Getting patients was not easy, because the academic community hated us. The guy who was the chairman of medicine at Johns Hopkins used to make speeches against the NIH, saying to have research without teaching was bad and to have research outside of an academic medical center was ridiculous. All the academic professors thought that was a bad thing.

Lesley Brunet, MA
0:08:17.7

Why?

Emil J Freireich, MD
0:08:25.9
Science began in medicine in Europe at the turn of the century, and they were all trained by the European model, where you start from the bottom and you work up until you become the professor. It's a very competitive environment, and the basis of your advancement is teaching and service, in addition to research. So the concept that you could separate research from patient care and teaching was what offended them, because they were trained to believe that those were inseparably linked. We have this problem here right now, too. The private people were also totally opposed because the federal government built this hospital, and they gave us one advantage that no one else had—care was free. The whole idea came out of the Franklin D. Roosevelt administration. It was part of his whole mentality, with Social Security and Public Health.

Lesley Brunet, MA
0:09:41.0
So the academics hated you.

Emil J Freireich, MD
0:09:43.2
In order to get patients, we went to community organizations. We were all young guys. There were no senior people, except for Zubrod, and he was middle-aged. The Heart Institute, the Cancer Institute, the Infectious Disease Institute—all these guys went around to the local Lions Club. We went to the community. Ninety-five percent of our patients were self-referred. Either they were indigent, or they heard that NIH was doing original things, or their doctors had told them, "You're hopeless, and we can't do anything." There were various reasons they came to us.

In 10 years, we had the largest leukemia practice in the world. The events that followed and made us famous were innovations. That's something I used to impress on Dr. Clark that I could not impress on Dr. LeMaistre. If you want to be successful in academe, it's innovation that drives the boat and it's patients who drive research. Research is a public commodity. AIDS was driven by what people thought they didn't want in the community, and so was cancer.

We built our practice, and we started taking care of kids with leukemia. At that time, Dr. Sidney Farber at Harvard University had discovered the methotrexate thing. He had some temporary remissions. Dr. Burchenal, Dr. George Hitchings, and Gertrude Elion, who got the Nobel Prize, had discovered 6-mercaptopurine, and it had some activity in children. Dr. Olaf Pearson at Memorial Sloan-Kettering and Dr. Farber at Harvard had discovered in 1953 that steroids could control leukemia.

We began by doing what Zubrod had learned from the malaria project. He said, "The first thing we have to do is decide what is good." We needed criteria for what a response to treatment would be. We sat down with a group of people, and we had consultants. We had Dameshek, Wintrobe, and all the big wheels on our consultant staff. We discussed what our goal would be.

What would be a treatment for children with leukemia? We decided the goal would be to restore them to normal hematology. So we created criteria for response, complete remission, partial remission, and improvement. It all sounds very corny today, but it didn't exist in 1955.

Then Zubrod said, "The other thing we learned in the military is you've got to keep objective, quantitative records." So we had criteria response, and we made flow sheets. There had never been flow sheets before, where you put all the blood values in, you evaluate response objectively by these criteria, and you put down that you achieve a percentage of patients and you get remission.

We started the first formal clinical trials, and we copied them after the clinical trials that were done in infectious disease. They were prospective randomized control trials, where all the eligible patients were included in the denominator. We began to systematically look at the treatment of leukemia. We started with chemotherapy.

The first problem we were going to deal with is we have 3 drugs: 6-mercaptopurine, methotrexate, and adrenocortical steroids, each of which had been proven to have some activity. Now, the way they've been proven is they were not in a formal clinical trial, so there were publications which said, "Well, we treated 20 children, and 3 of them got better." Okay, what was "getting better"? It doesn't really say. Burchenal treated 20 children with 6-MP, and 4 got better.

In 1955 an article was written by a good friend of mine, Arthur Haut, who was then a fellow with Max Wintrobe, the guru of hematology. Don't forget, I had confronted Wintrobe on infection. In this article he said, "All this stuff is very good, but it represents experimenting on children. Because it doesn't prolong their life, and they're all toxic drugs, it just makes them sick. Why don't we just let them die happy?" Didn't say that, but it did say the drugs are ineffective. We said, "Okay. Here we have these reports. We have this. We don't know anything about childhood leukemia. Let's start out. We'll collect these children with leukemia, all of them. We need a systematic way to treat them, and we need a question to ask."

We went to our 2 major advisers. One was a guy named Lloyd Law. Lloyd Law had been in the Public Health Service for 20 years. He was the person who discovered the syngeneic mouse. He's one of the founders of the Jackson Lab, where you crossbreed mice until they're identical twins; they have the same genes. Have you heard the word, L1210 leukemia? That *L* is for Lloyd Law. Lloyd Law discovered that if you create a cancer in a mouse by painting coal tar or injecting some chemical carcinogen and you transplant that cancer to another animal, it will be rejected; but if you transplant it to an identical twin, it will grow. Lloyd Law created all the experimental animals that we use to screen drugs.

Chapter 06
The First Systematic Studies of Chemotherapy for Leukemia
A: The Researcher;

Codes
B: Overview;
A: Overview;
A: Definitions, Explanations, Translations;
C: Discovery, Creativity and Innovation;
C: Discovery and Success;
C: Professional Practice; C: The Professional at Work;
B: Multi-disciplinary Approaches;
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

The second person was a guy named Abraham Goldin. Abe Goldin had been working in the NIH, in one of the basic science labs, and he was interested in how drugs affect mice with these transplanted tumors. Abe Goldin showed that if you gave methotrexate at the maximum tolerated dose every week to mice, the mice lived longer than if you give it every day. Now, the way methotrexate was given to children was by mouth, 2 mg a day, in pill form. Dr. Frei and I had a series of meetings with these 2 guys, and we came up with the idea that there's something worth investigating in man. If it's better in a mouse, and this is a model of human leukemia, maybe it would work in a man. We said, "Let's have a study where we randomize children to receive methotrexate daily or once a week."

We had to do some pilot studies to get the maximum tolerated dose in man. The thing that came out of the infectious disease literature that was exciting was that if you wanted to cure a disease, you always did better with 2 drugs than with one, provided they had a different mechanism action. We said, "Well, this is okay, but what if we also gave them 6-MP at the same time?" When this was reviewed by the committees, they gave us hell. "If you give 2 drugs at the same time, think of what you're doing wrong. I mean, when the children fail," and they all failed on 6-MP, "you didn't have methotrexate. So that's wrong. You're using up all your treatments. It's terrible to do. And they're adding that toxicity."

It took us a long time to get this program, but we got it approved. We called it Protocol 1. It was the first systematic study of chemotherapy of leukemia in man. Hypothesis: Is the combination better than single agents? And does the schedule of methotrexate affect outcome? Well, we didn't hit a home run. We got the same response rate. But we were impressed by one thing: the

response rate in both arms was higher than had ever been reported with the single agents. We said, "There may be something to this combination chemotherapy, but we don't have concurrent controls. There have never been quantitative studies, so we can't be sure." We published this in *Blood*. The editorial said, "It's nice for these young guys to keep trying."

Then we came up with Protocol 2. Dr. Law had made an important observation. He was interested in the mechanisms of resistance. If you give a subcurative dose of methotrexate to a mouse and you transplant over several generations, you end up with an L1210 cell line where when you give methotrexate, nothing happens; they're completely resistant. If you did it with 6-MP, the same thing happened, and the same thing happened in man. Everybody who responded became resistant; the disease came back. So there was a mechanism of resistance. So we said, "Well, look, Dr. Law said if he took mice that had 6-MP-resistant leukemia and treated them with methotrexate, the methotrexate worked better than in mice that were not 6-MP-resistant." We now understand the biochemistry. This is what they call collateral sensitivity. When you have 6-MP, the mechanism of resistance, deleting an enzyme which is involved in the salvage pathway, methotrexate involved the de novo pathway. When you're resistant to the salvage pathway, you're enormously sensitive to interfering with the de novo pathway. We know the biochemistry now, but back then we didn't know anything about it. This was just experimental evidence. We went to man, and we did Protocol 2.

What was Protocol 2? This was really exciting. We randomized children to receive 6-MP, methotrexate, or the combination. Now we can test the hypothesis that the combination is better, with good, objective, quantitative prospective data. And since this was both drugs, and you're using them both up, it's obvious that we have to study the effect of the sequence. We had 6-MP followed by MTX, and the reverse—MTX followed by 6-MP. How do you think that came out? The combination of 6-MP and MTX won over the sequence. There was no collateral sensitivity. The response rate as initial therapy here was the same as after prior treatment. The combination was better across the board. Now we were onto something—combination chemotherapy. That's what led to the cure of leukemia and Hodgkin's disease. Everything in chemotherapy is now combinations, and it's all because of this resistance problem.

Chapter 07
Treating Hemorrhage in Pediatric Leukemia Patients
A: The Researcher;

Codes
A: Overview;
A: Definitions, Explanations, Translations;
C: Discovery, Creativity and Innovation;
C: Discovery and Success;
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;

Emil J Freireich, MD

While this was going on, Dr. Zubrod made rounds one day. He was a bureaucrat administrator. He had to run the whole thing, and he used to come on rounds about once a month. He came on rounds on leukemia service with Tom Frei. We walked into a boy's room. He was 5 years old, and he was a lovely child. His name was Scotty Dinsmore, and he had lovely parents who were totally devoted. The father was a minister, and his wife was a homemaker. This child was lying in bed in a coma, and he was bleeding extensively because he had no platelets.

When you're in coma, you have a type of breathing which is called Cheyne-Stokes. The breathing is intermittent because the respiratory center is suppressed. There are periods where you don't breathe, and then you get CO_2 stimulation to the brain, and then you breathe rapidly. So this child was breathing rapidly when we entered the room.

We walked into this room, and the parents are there. They're wearing surgical gowns, and they're covered with blood. Blood is on the ceiling; it's all over the room. It looked like a slaughterhouse. When we walked in, we realized we couldn't stay there very long or we'd get covered with blood, so we walked back out in the hall. Zubrod said, "You know, Freireich, you ought to do something about this bleeding." "Yes, sir." Here we go again.

Lesley Brunet, MA
0:25:24.2
So you did.

Emil J Freireich, MD
0:25:26.6

That's a really terrific story, because to get platelet transfusion going, if you think my first controversy was controversial and that chemotherapy was controversial, *this* was controversial. It was controversial for several reasons. Controversy is always because the established gestalt is not superficial. It's not a bias. It's based on real experiences. There are geniuses who work hard to create the body of certain knowledge that we think we have. If the body of certain knowledge is challenged by someone, it's obvious that the challenge is wrong, and to get it over that hump is very difficult.

At the time, I consulted a lot of people. We had a guy working at one of the basic science labs. His name was Koloman Laki. He was a Hungarian immigrant, and he had figured out how fibrinogen polymerizes. The biochemistry of fibrinogen polymerizing is the thrombin which an enzyme clips a little peptide off the large fibrinogen molecule, and that makes the fibrinogen molecule polymerize. It's just like they now chemically make plastics. The first plastic was fibrinogen. Dr. Laki had isolated this peptide and worked out the chemistry of how it went together, so he was already famous. He's one of the people I consulted. This was around 1956, and at that time, 90 percent of the children died of hemorrhage. That was *the* cause of death. Of course, leukemia caused the bone marrow failure, but they died of hemorrhage. The question was why are they bleeding all over the place?

I read all the literature, and it turned out that one of the world's authorities on hemorrhagic diathesis was in the NIH. His name was George Brecher, and his colleague was a guy named Eugene Cronkite. They had done some absolutely brilliant experiments, before they came to the NIH, on the bleeding associated with radiation injury. They had done all these experiments in dogs and rats and monkeys. And then there was Frank Gardner. Frank Gardner was assistant professor at Harvard when I was in Boston, and he had done some brilliant experiments on platelet transfusion in the dog. He'd worked out phenotypes. A lot was known about this. The geniuses were right there around me. I'm young and stupid, and I don't know up from down, so I read the literature, and I said, "Well, that's interesting. What am I going to do? Zubrod says I have to cure hemorrhage." I had a little lab, so I said, "We'll do some experiments."

I started collecting blood from these children, and I had a neighbor who worked in the Heart Institute. His name was Ed Korn. He's now the scientific director of the Heart Institute. I said, "You know, you guys are working on all these fats, and what I've read in the literature is that the clotting process begins with the blood platelets, and the platelets have lipoprotein. It seems to me that if we could isolate this lipoprotein—it could be made chemically—we could cure bleeding." He said, "No problem. We've just invented the ultracentrifuge." The ultracentrifuge was high centrifugal force, and it was developed primarily to separate lipids. You put solutions that had a given specific gravity in the tube, and then you put it in the very high centrifugal force. Everything that's heavier than this goes to the bottom, and everything light stays on the top. I said, "Okay." I read the literature about lipoproteins. I went to his lab, we got platelets from the blood bank, we broke them up with a Sonicator, we stuck them in this thing, and we cut this. I made a thing which was called platelet factor 3. This had been described by other scientists. Walter Seegers, one of the famous coagulationists, had described platelet factor 3. We knew it was there, but we didn't know how to get it. We got platelet factor 3, and I wanted to give it to children with leukemia, not to animals, so we got approval from our Institutional Review Board.

Lesley Brunet, MA
0:00:12.6
So you still have your IRB even that early?

Emil J Freireich, MD
0:00:17.0
It was even worse then.

Lesley Brunet, MA
0:00:19.2
It was worse then?

Emil J Freireich, MD
0:00:20.7
Sure, because no one had ever experimented on people. We had an external advisory committee that watched everything we did like hawks. They were terrified that we would experiment on patients, because here was this hospital in the middle of nowhere with no professors, a bunch of young guys, and sick patients.

Lesley Brunet, MA
0:00:36.8
They were terrified that you would experiment on patients?

Emil J Freireich, MD
0:00:39.5
Yes.

Lesley Brunet, MA
0:00:39.7
Isn't the point to experiment?

Emil J Freireich, MD
0:00:45.6
We needed supervision. We were young and innocent, young and foolish yet. If you say it fast, it comes out right. We were heavily supervised. I got approval to do this thing. We got it sterilized, we got some children, and we ran this stuff in. And by golly, bleeding stopped immediately. We got it in a bag, I added it to these children's serum, and they all clotted.

We wanted to publish this. The problem was that as soon as we stopped shooting the stuff in, the bleeding started again. So we said, "Now I understand clotting. The problem is that these children don't have any platelets, so they don't have platelet factor 3. If I give them platelet factor 3, it corrects it. So there is no anticoagulant. There is no mystical factor." It was believed that the disease caused an interruption with the fibrinogen-thrombin reaction. The biochemistry is complicated.

I had done it in vivo, so it's obvious to me that what the platelet did was deliver platelet factor 3 to the bleeding site, and the platelets survived for 7 to 10 days. My stuff only survived for 10 minutes, because it got cleared by the R.E. system. So the platelet is a delivery system for platelet factor 3. I can cure the bleeding if I can make their platelet factor 3 level elevated. Now, that's the controversy.

Chapter 08
A Research Controversy: Treating Hemorrhage in Childhood Leukemia
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;
C: Patients; C: Patients, Treatment, Survivors;

Emil J Freireich, MD

As I said, Brecher and Cronkite had proved that platelets don't work. They had irradiated animals. If you irradiate an animal, he has no platelets. He doesn't bleed. They don't need platelets. You have to have something else happen. If you give them a little bit of heparin, an anticoagulant, they bleed like a stuck pig, so the cause of bleeding is some anticoagulant. It's not due to the platelets. Platelets are permissive. But I had discovered something. So I went to Dr. Zubrod, and I said, "I have this idea. If I can elevate their platelet count, I think we can stop the bleeding."

I had this child whose parents were there. How can you give them platelets? Well, platelets are very fragile, and they don't store. We didn't know how to store them. The only platelets I could get would be fresh platelets. So I got a brilliant idea. I said, "What if we did what Dr. Duke did in 1910, that is, an exchange transfusion? What if I got some adults, and I took 50 cc of blood with normal platelet count out, and I took 50 cc out of the child and put the normal blood in, and I kept doing that?" I still have the graph from the first child I did. It never was published.

Lesley Brunet, MA
0:04:22.8
It wasn't accepted for publication? It was too controversial?

Emil J Freireich, MD
0:04:30.3
Couldn't get out of the NIH. So I did this study. We did an exchange transfusion. We got volunteer donors from the church, and each donor donated 500 cc of blood, then 50 cc in type.

Then we got another donor and just kept going. Normal platelet count is about 200,000 per microliter. The child had none.

It turns out, by exchange transfusion, the theory has been worked out. If you do 2 total blood volume exchanges, you will end up with 90 percent replacement of everything. To get up to 200,000, we had to do 2 blood volume changes. He's a little child. He's 1 square meter, so he has about 2,500 cc. So we had to do 5 liters. We had to have 10 donors, 500 cc each. No bleeding.

Lesley Brunet, MA
0:05:36.5
For how long?

Emil J Freireich, MD
0:05:37.1
For 5 days. And we counted the platelets. When the platelets got too low, the bleeding started again. That told you something. The most important factor was the number of platelets in their blood. How can I prove that? Brecher and Cronkite went to the scientific director of the Cancer Institute, not Dr. Zubrod, and said, "Freireich is doing experiments which are dangerous." Dr. Mider called me to his office, so help me God, and he said, "Freireich, Dr. Brecher and Dr. Schmidt tell me you're doing these experiments with platelet transfusions, and they think it's totally reckless. You have to cease and desist or you're going to be fired." I went to Dr. Zubrod, and I said, "Dr. Zubrod, what should I do? You told me to cure hemorrhage. I'm curing hemorrhage." Dr. Zubrod said, "The only thing to do is to make it explicit."

So he called a meeting—grand rounds on the top floor of the Clinical Center. Brecher and Cronkite were there; Dr. Mider was in the room. Schmidt presented the data, and I presented my data. At the end, Brecher said, "Heretofore, we will not allow Dr. Freireich to collect fresh blood for his patients because it's bad for them." Dr. Zubrod stood up and said, "As long as I have the responsibility to be director of the Cancer Institute, if my physicians order fresh blood, you will have to deliver it." It was a tremendous confrontation.

There's a little more background to it, because when I made this observation we said, "Okay. All we need is platelets." Where do you get platelets? Well, we can collect platelets from people and separate them, but we knew that they didn't survive in vitro. At room temperature or at cold temperature, at any temperature, the platelets dissolve. By the time blood has been stored under blood bank conditions for 48 hours, for red cells, you can't find any platelets; they're all dead.

We said, "There's only one way to approach this. We have to do a randomized prospective trial. We have to do it in a circumstance where the data are objective and quantitative to resolve the controversy. Here's what we're going to do. When a child is bleeding, we're going to call the blood bank, and the blood bank is going to decide by random allocation from a statistician whether that child gets fresh blood drawn from donors or blood that's been in the blood bank for more than 48 hours. We'll see if the platelets have any effect on hemorrhage."

Lesley Brunet, MA
0:08:55.8
But you knew that they would.

Emil J Freireich, MD
0:08:57.2
But they knew that it wouldn't. Remember, the opposition is more certain than I am. First of all, Brecher has been doing this for 25 years. The gospel is known. There's a body of data there, 4000 references in literature. All there is is me, dumb Freireich. He doesn't know up from down.

Lesley Brunet, MA
0:09:17.5
What about the data?

Emil J Freireich, MD
0:09:21.6
That's an interesting comment, "the data," because I still deal with this today. The data doesn't speak for itself. Data doesn't exist in the world; it exists in one's brain. When people look at data, they come out with different things. They saw the data, but they didn't believe what we had done. They said the exchange transfusion worked because I removed the anticoagulant. But I knew it was the platelets, because I had taken platelets from a normal donor in vitro and mixed it with the patient's blood in a test tube, and it corrected it. So there was no anticoagulant. But they had another hypothesis based on their own experiments. So everybody knew the answer. Everybody who agreed to this, Brecher and Cronkite, knew that it would be negative. They knew because they had the knowledge. I didn't know anything. I'm a dummkopf from nowhere. I had never worked in platelets or blood. I didn't have any publications. This is a new field for me. They agreed to this experiment. It was double-blind. The blood banker didn't know, the doctor didn't know, and the patient didn't know. We didn't have any stopping rules in those days. Statistics weren't too sophisticated. We treated a number of children, and some number came along.

One day Tom Frei said, "Freireich, we should see how it's going." It was obvious to me, taking care of the children, that in some children, after the blood, they stopped, and in other children, after the blood, they didn't. There was heterogeneity in outcome. Maybe we could see what it did. We had a public meeting. Dr. Schmidt, Dr. Brecher, who knows it's not platelets, Dr. Frei, me, and our statistician, Martin Schneiderman. We're going to break the code. So I go to the board and start with patient number one. "Did this patient stop bleeding or not?" Well, the way I assessed bleeding was not only looking at blood, but we measured the concentration of hemoglobin in urine. We measured the plasma hemoglobin, the rate of fall, so we had quantitative data. Patient number one received transfusion on this day. Hemorrhage change, yes or no? Yes. And we went on for several more patients. A hundred percent of those that I felt had changed had gotten fresh blood. Ninety-five percent of those who I said didn't change had gotten bank blood, because some of the children that got fresh blood didn't respond.

This is a serious problem now. Everybody left the meeting depressed. Two days passed, and I was called to Dr. Zubrod's office. "Dr. Brecher has pointed out that this study is null and void." "Why?" "Because you interviewed the families and asked them whether they donated blood or not, and most of the fresh blood donors were family members who were in the hospital. You knew in advance who was fresh and who was banked, and you cheated."

Lesley Brunet, MA
0:13:14.9
How does knowing in advance affect the results?

Emil J Freireich, MD
0:13:19.2
I could say that the ones who stopped bleeding had fresh blood, because I knew they got fresh blood. It wasn't blind. They were saying I knew they got fresh blood, and I said they stopped bleeding. The whole study is null and void. I'm a liar and a cheat, and I'm about to be fired again. How do you react if someone calls you a liar? I'm working my head off 20 hours a day, and I'm cheating?

I wrote the paper, and Dr. Frei reviewed it. He was my friend, and he believed me. Dr. Zubrod reviewed the paper, but he was in a tough position. Should we accept it or not? His career was at stake. If it was me alone, he would never have approved it, but Tom Frei put himself in between, and he trusted Tom. He couldn't turn his back on Frei and Freireich, so he let it go. It was published in the *New England Journal of Medicine*. It's a classic, of course. It's been confirmed a million times over. And Brecher put his name on the paper. We had a series of meetings after that with Brecher, and we said, "Come on. Let's face it."

Brecher was very famous because he invented the phase microscope method for counting platelets. Prior to that, platelets were counted by an indirect method that was invented by Dr. Dameshek. The indirect method is, you make a blood smear, and you count the number of platelets relevant to the number of red cells. You count the red cells objectively, and that gives you the platelet count. It turns out that tremendously overestimates the number of platelets, because the platelets adhere to glass and the red cells don't, but that wasn't known at the time. So Brecher invented, with Cronkite, the Brecher-Cronkite method for counting platelets directly, in a hemocytometer chamber. He used a phase-contrast microscope. If you change the light phase, then things become doubly refringent when they're in the out-of-phase light. You could see the platelets, so then you could count them objectively, and we knew the real platelet count.

I went down to Brecher's lab, and I learned how to do this. In the children who got the transfusions, I did a platelet count on every child. They accused me of lying, because I interviewed the family. Now, was there any way I could lie when I counted the platelets? They couldn't figure that one out. Well, it turned out that the difference between the children who got fresh blood and the ones that got bank blood was 5000 platelets per microliter, measured 1 hour posttransfusion. The normal is 200,000. No one could believe that 5000 per microliter could make a difference. But Brecher couldn't do anything about it. I had used his method. It was done double blind. There was no way for me to know which child got fresher blood before I

knew they were bleeding or not. I had a technician who counted the platelets, so I know they're objective. That's why I put his name on the paper. He never believed that it was true, that fresh blood was better, but he believed the platelet counts.

Then we did a brilliant study. I had a fellow whose name was Larry Gaydos, and we said, "Look, if the platelet count determines the risk of hemorrhage, let's go through the charts of 130 children that we have treated, examine the nurses' notes, decide on which days they had gross hemorrhage or minor hemorrhage, record that for every day, and then we'll record their platelet counts by the Brecher method." That paper is a citation classic. It's in the 100 most-cited papers in the literature. It was a retrospective review. We showed that there was a direct relationship between the platelet count and the hemorrhage. Aha! It's not an anticoagulant; it's the platelets.

So we said, "Wait a minute. If one transfusion of blood will give you 5000 per microliter, what would 2 units of blood give you?" It would give you 10,000, maybe. If 1 unit causes hemorrhage arrest for 3 to 5 days, how can we get more platelets? Well, we can give them 2 units of whole blood. But these kids only have 2,500 cc. They only have 5 units. They're already volume expanded. If you give them 2 units, they're going to die of a stroke immediately. We have to figure out a way to give them platelets.

I went back to the library. To make a long story short, we invented plateletpheresis. I was working with a young guy who was a fellow in the blood bank. His name is Allan Kliman. You see, the plastic bags had just been introduced into medicine. Fenwal Laboratories had introduced plastic into blood processing. Prior to 1955, all blood was collected in glass bottles with rubber tubes and steel needles, and it turns out that those things are all incompatible with platelets. By the time the blood gets to the bottle, 90 percent are removed by the rubber and the glass. But it's not bad for red cells. Red cells do okay. They don't stick to anything. Fenwal introduced the plastic line into the blood bank. You could collect blood in a plastic bag, and you could ship it, store it in the refrigerator, and it takes less room.

When you studied platelets, we knew that the surface was what determined whether the platelet lived or died. The platelet is designed to recognize a surface that is wettable. Platelets and plastic did very well. We could get a plastic bag, collect a unit of blood, spin it in a centrifuge, take the platelet-rich plasma out, give the red cells back to the donor, collect a second unit of blood, and do the same thing. We'd have 2 units of plasma. Plasma has the great virtue that the space for plasma is at least twice the space for red cells. So we can give 500 cc of plasma and get 2 units of blood from each donor and give it to a child. You just had to collect a lot of platelets, count them, and transfuse them.

Sure enough, when we finally did all the work, it turned out that you got about 12,000 increments per 10^{11} platelets. That's the amount in 1 unit of blood. If you do the plasmapheresis perfectly, instead of 10,000, you actually get 24,000. And 24,000, it turns out, is outside of the hemorrhage range. By the time you get over 20,000, the likelihood of bleeding is very low. We said, "Let's give these kids 2 units of platelet-rich plasma twice a week." The lifespan had been measured when I was working in Boston. Frank Ebaugh, who discovered the chromium method for the turnover of platelets, was working there, so we knew the half-life was 5 days. "Let's do 2

units twice a week." We did that in a 100 children and wrote a paper. It was rejected by every journal. We finally got it published in the *Annals of Internal Medicine*, and the rest is history.

THE UNIVERSITY OF TEXAS
MD Anderson Cancer Center
Making Cancer History®

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
0:01:16.5
What kind of infections were they?

Emil J Freireich, MD
0:01:17.9

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
0:03:27.9
Does that affect the donor?

Emil J Freireich, MD
0:03:29.0
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
0:06:04.2
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
0:09:01.3
They actually fired you?

Emil J Freireich, MD
0:09:03.1
Yes. They changed the project officer. I couldn't work on this project anymore.

Lesley Brunet, MA
0:09:07.2
So you were still there but not working on that project?

Emil J Freireich, MD
0:09:08.7
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;

Codes
A: Overview;
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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
0:11:49.2

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

Emil J Freireich, MD
0:11:59.0
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
0:13:59.4
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
0:17:17.3
How would you be able to tell the difference between the prednisone and the vincristine?

Emil J Freireich, MD
0:17:21.1
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.

THE UNIVERSITY OF TEXAS
MD Anderson Cancer Center
Making Cancer History®

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;
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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 Kmarts or Walmarts. 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.

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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 Cytoxan 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)

THE UNIVERSITY OF TEXAS
MD Anderson Cancer Center
Making Cancer History®

Emil J Freireich, MD

Interview Session Three: 6 August 2001

Chapter 16
Charles LeMaistre, the New President, Initiates Reorganization, with Impact on Developmental Therapeutics
B: Building the Institution;

Codes
C: Leadership; D: On Leadership;
C: Portraits;
C: Professional Practice; C: The Professional at Work;
B: Growth and/or Change;
B: Obstacles, Challenges;
B: Institutional Politics; B: Controversy;

Emil J Freireich, MD
0:00:41.5
We were in 1978. Dr. Charles LeMaistre arrives.

Lesley Brunet, MA
0:00:43.8
He was the head of the search committees?

Emil J Freireich, MD
0:00:56.9
Correct.

Lesley Brunet, MA
0:00:57.4
How was that possible?

Emil J Freireich, MD
0:01:05.9

You'll get the real story from Dr. LeMaistre, but probably you'd get the real story from the guy who was chairman of the Board of Regents, who they named the center after, Frank Erwin. He was the big builder.

Lesley Brunet, MA
0:01:27.8
He's dead, of course.

Emil J Freireich, MD
0:01:28.0
Is he?

Lesley Brunet, MA
0:01:29.9
Oh, yes.

Emil J Freireich, MD
0:01:30.7
What happened? Did he shoot himself or something?

Lesley Brunet, MA
0:01:33.1
No.

Emil J Freireich, MD
0:01:35.5
Disease?

Lesley Brunet, MA
0:01:36.5
I forget, but I worked for the Johnson Foundation then, so I had to stand by his coffin.

Emil J Freireich, MD
0:01:41.5
Oh, my goodness.

Lesley Brunet, MA
0:01:42.5
It was an interesting experience.

Emil J Freireich, MD
0:01:43.8
Mickey [aka Charles A. LeMaistre, MD] was Frank Erwin's guy. Then there was the DWI against Erwin, and he resigned as chairman of the board. Now the board had either a new

chairman or no chairman. It was obvious that Frank Erwin was running the university, so they needed someone to run the university. Remember, this is my perception. Mickey will give you a really gorgeous analysis. So they appointed the business guy, Don Walker, as chancellor. They negotiated with Dr. LeMaistre first, but the university doesn't like scandal, so they had to get rid of LeMaistre. But they didn't want to do it publicly, so they said to him, "Since Dr. Clark and Truman Blocker have resigned at the same time, we're going to form 2 search committees to replace them, and we'll ask you to head these 2 search committees."

Lesley Brunet, MA
0:02:44.6
Dr. Clark resigned before they headed up the search committee?

Emil J Freireich, MD
0:02:47.2
Correct. Dr. Clark announced to the faculty that because Truman Blocker wanted to absorb MD Anderson, he went to the wall, the confrontation occurred, and they both agreed to resign simultaneously.

Lesley Brunet, MA
0:03:01.0
But was he still acting president until someone else came in?

Emil J Freireich, MD
0:03:05.7
Yes, he was acting president, and so was Truman Blocker. Then they asked Mickey to run these 2 searches. Of course, the idea was that he could choose which of the 2 presidencies he wanted, based on what he learned about these 2 institutions. That was probably implied. It may never have been spoken, but it was my perception of what was going on. So here's Dr. LeMaistre chairing these 2 sessions. He's still chancellor of the University of Texas system, and the search goes on. I was a candidate. Someone nominated me. I wrote a really strong support of my own candidacy for the position, but I was never interviewed. They never took it seriously.

Lesley Brunet, MA
0:03:50.6
Who nominated you?

Emil J Freireich, MD
0:03:51.4
I have no idea. The letters came from the search committee and said, "You've been nominated. Please give us all your stuff." So I gave them all the stuff, but I was never considered seriously. But I was a candidate, so Dr. LeMaistre had to look at my curriculum vitae. So he knew who I was before he came here. I was the only one at MD Anderson, to my knowledge, who was nominated. No, I think Dr. Hickey was nominated.

Lesley Brunet, MA
0:04:15.2
There were 3 big candidates.

Emil J Freireich, MD
0:04:18.4
Oh, yes, from the outside. So the searches proceeded. I didn't keep up with the medical school search, but they ended up with a guy who became president of the AAMC. He was pretty good. Our search came down to 3 big candidates, one of whom was Vincent T. DeVita. I was his host when he came because he was one of my fellows when I was at NCI. He'd told me this really looked great. I forgot who the other 2 were. But suddenly it was announced that Dr. LeMaistre had chosen himself, or to put it in university language, the committee insisted that he take the position because they didn't have a better choice.

Of course, the reason he picked MD Anderson is because Dr. Clark had built an ironclad position for himself. The Physicians Referral Service was fully funded, and very well-funded indeed. The institution was booming, and it was a perfect time for someone who wanted to just coast along to take over MD Anderson. The medical school was struggling. They had to hire deans and presidents and everything else. That was no job for Mickey, so he took this one.

Lesley Brunet, MA
0:05:55.4
Did it have to do with this creation of the executive vice presidents, who seemed to take over more of a load for the president in the '70s?

Emil J Freireich, MD
0:06:09.0
At MD Anderson?

Lesley Brunet, MA
0:06:10.3
Yes.

Emil J Freireich, MD
0:06:10.9
We didn't have any such person that I know of.

Lesley Brunet, MA
0:06:14.3
Frederick Becker [oral history interview] came in the late '70s.

Emil J Freireich, MD
0:06:16.0

Oh, yes, but Becker was Chairman of Pathology when he was recruited, and he was a hopeless failure and soon stepped aside. All that was created under Mickey. Dr. Clark ran the place and picked all the people, and when he gave them authority, they had it. He didn't have any deputy that I know of. Dr. Hickey was there, but Dr. Hickey was kind of an adviser. Dr. Hickey was really the person who ran the day-to-day operation around here. But I don't know of any other executive position at the time.

So Dr. LeMaistre is announced as the new president, and soon after he resigns as chancellor of the University of Texas. The papers say he wants to come back to medicine. We all think that's a great idea.

Lesley Brunet, MA
0:07:12.2
You're being facetious?

Emil J Freireich, MD
0:07:16.6
No, we really did. You have to meet Dr. LeMaistre. He's a movie star. I mean, this man comes out from Heaven. Everyone who meets him is impressed by him.

Lesley Brunet, MA
0:07:32.6
So you were all looking forward to him coming.

Emil J Freireich, MD
0:07:34.9
Dr. Clark ran it with an iron hand. We had lots of frustrations, so Dr. LeMaistre sounded wonderful. Everybody was enthusiastic. As you know, I was running the most important part of this institution at the time. He was supposed to be a pulmonary specialist, so we thought we'd get him interested in medicine, and we invited him to see a patient on leukemia service. We took him on rounds, but it only took 5 minutes to realize that he knew nothing about medicine. So in 5 minutes we discovered that medicine was not his forte, and he didn't come to MD Anderson to return to medicine.

I thought his first moves were just wonderful. The guy had a lot of class. Dr. Clark was a good old Texan. It's as if Mickey was from the movies, and he made these speeches. "We're going to reorganize the place. We're going to decentralize authority."

Lesley Brunet, MA
0:09:26.7
There was a big push on that.

Emil J Freireich, MD
0:09:28.6

It's just going to be wonderful. I was chairman of the executive committee at the time, which was the medical staff executive committee, and we had the 5 departments. He came to the executive committee, and he said, "Dr. Clark ran this place with an iron hand, but I am using modern administrative techniques. We are going to decentralize the presidential authority, and the first step is, we're going to pick a vice president for clinical affairs and a vice president for research." I think those are the only 2 positions he created.

Lesley Brunet, MA
0:10:11.1
Education came in later.

Emil J Freireich, MD
0:10:12.9
Yes, it came later. By some mechanism that I don't know, maybe the executive committee of the science faculty, Becker was appointed vice president for research, which everybody was happy with. And the executive committee of the medical staff had to pick a vice president. We had about 6 nominees, and we had some very emotional meetings. Remember, DT is too powerful now. So this is an opportunity for the other chairmen to get some handle on it.

There was a guy named Fred Conrad. Dr. Clark hired Fred Conrad. He was an air force general, and they used to hunt bears in Alaska together, so he liked him. He came into the Division of Medicine under Clifford Howe, and he was just a doctor, no training, no expertise. But someone on the executive committee got the brilliant idea that Fred Conrad was well organized and decisive; he ought to be the vice president. Of course, I said, "We need someone who's academic, who cares about clinical research." But everybody hated clinical research, except DT. So to make a long story short, there were 7 people on the committee, and the vote was 6 to 1 in favor of Conrad, and I was the 1. Of course, Conrad knew about that. So Conrad became vice president, and Fred Becker became the vice president for research.

Lesley Brunet, MA
0:12:00.2
Where was Hickey? Hickey was out?

Emil J Freireich, MD
0:12:04.6
Hickey was special adviser to the president or something of that kind. He had no authority. He was immediately castrated. This was not looking good, so I went to Dr. LeMaistre. This is the world's greatest person. If you talk to him, he sounds like a semi-intellectual. He knows all the right sentences and paragraphs. He's really amazing. When you consider that between the ears there's only air, he is just amazing. Did you see that movie *Being There*? The guy was really retarded, but he just knew how to say sentences. This was Mickey. What a slick guy. I went to Dr. LeMaistre, and I thought, "Here's the really intelligent person who understands it."

I said, "Dr. LeMaistre, you have a vice president for patient care who's a clinic doctor, doesn't know anything about research. You have a vice president for research who's okay. He does research, but he only cares about laboratory people. What you need is a vice president for clinical research." Dr. Hickey was present during all this. Dr. Hickey didn't like this kind of stuff. He was between Mickey and me for all the time he was here. He tried so hard to be helpful. In dealing with Dr. LeMaistre, you soon learned that everyone who came to him left his office feeling that he had accomplished his mission—everyone. Dr. LeMaistre said, "That's a great idea." So Dr. Hickey prepared a job description for this vice president for academic clinical research, and they offered me the position.

Lesley Brunet, MA
0:14:40.7
This is instead of Conrad or in addition?

Emil J Freireich, MD
0:14:45.4
It was in addition. We're going to have 3 vice presidents: one who ran the clinic, one who ran the laboratories, and one who ran clinical research. The department had its own beds. We had a Clinical Research Center. We had built the eleventh and twelfth floors on the Lutheran Pavilion. We had our own operation at Center Pavilion Hospital. The Clinical Research Center was a whole institute within MD Anderson. The responsibilities of this vice president were laid out that he would handle all of the DT part of it, the clinical research, and the office of research. It's all in the job description that was 2 pages long. So I read it carefully, edited a few ifs, ands, and buts. Dr. Hickey wrote it; I accepted it formally.

Then Dr. Hickey, Dr. LeMaistre, and I had a meeting. Dr. LeMaistre said, "This is really wonderful. We're distributing authority from the president, and it's an excellent idea. But what concerns me is the borders between the vice president for clinical research, the vice president for research, and the vice president for the clinics." He said, "I think what you ought to do, Freireich, is you meet with Conrad and Becker, and just be sure you define the borders, and then—"

Lesley Brunet, MA
0:16:07.4
I saw that letter, actually, and I have a note on it.

Emil J Freireich, MD
0:16:09.4
Do you? Where did you get it?

Lesley Brunet, MA
0:16:11.3
It's in the *President's Office Records*.

Emil J Freireich, MD
0:16:14.7
Good.

Lesley Brunet, MA
0:16:15.3
"To ensure clear lines and delineation of authority where possible and definition of deliberately"—it's really wordy—"overlapping areas."

Emil J Freireich, MD
0:16:27.2
I went to Dr. Conrad. Dr. Conrad said, "There's no such thing as clinical research. If it's clinical, I'm in charge of it. No need for that job." I said, "Well, let's go meet with Dr. LeMaistre, because we have a difference of opinion."

We arranged a meeting with Dr. LeMaistre. This is where I suddenly realized what we were dealing with. Dr. Conrad speaks, I speak, and Dr. LeMaistre makes a 10-minute speech. Remember, Dr. Conrad thinks he won. Dr. Freireich thinks he won. We leave the room.

Lesley Brunet, MA
0:17:15.3
What happened?

Emil J Freireich, MD
0:17:20.8
"Well, Fred, I won." "No. You don't understand LeMaistre. You don't pay any attention to him. You lost."

This is the secret of Dr. LeMaistre. He never, ever, acquired responsibility for any decision. That's why he lasted 18 years. That's why he lasted 10 years as chancellor of the university, because everybody he talked to, he agreed with. There were no controversies in his mind. It's not anything deliberate. It was part of his personality. He just wanted so desperately to help people. Everybody around him immediately realized that whatever he said made no difference. So everybody who had responsibility simply used it. And since Conrad was in charge, he said, "It doesn't make any difference what Mickey said. I'm in charge."

I understood LeMaistre, so I went to Becker. I didn't understand Becker. I understood Conrad, because, remember, I voted against him. I knew he was a straight hospital guy. He thought research was trivial. He was a carbon copy of Shullenberger. They just work until they retire. Conrad ran a hospital. He just wanted to retire, and when he got 20 years, he came here. He just wanted another 20 years to retire. He wanted to become Air Force Surgeon General, and he lost that job, so he quit the air force and came here. He just comes to work every day. Conrad was the hardest-working person I've ever seen. He was here every morning at 6:00. He drove a fast car from Conroe. We began to compete. I always used to be the first one here.

I had a practice of being the first one here and the last one out. If I was going to build a department, I had to be a leader. So I came to the parking lot one day; there was a car there. It annoyed me a little bit. After 3 or 4 days, I found out this was Conrad's car. So I came a half hour earlier. That lasted about a week, and then I came, and his car was there. So I came a half hour earlier, and there was his car. You couldn't beat Conrad. He was indefatigable. So I finally gave up on that. He was here at 6:00 a.m. He checked every clinic. This guy ran it like you run your kitchen. He's a military guy. He was really terrific at what he did.

Mickey was terrific at what he did. That's the secret of surviving in the UT system. Never make a decision, but appear decisive. When we met with Mickey, I was certain he agreed with me, but it didn't matter, because he wasn't going to act on it, anyway.

Lesley Brunet, MA
0:20:18.1
He didn't ever follow up with delineating responsibility?

Emil J Freireich, MD
0:20:20.1
He never followed up anything. Then I went to Becker. Now, Becker's a different breed of cat than Conrad. Becker really is academic. He wanted to have all Nobel laureates here. He recruited Isaiah J. Fidler [oral history interview] and Eric Olson and a lot of very good people. Becker and I sat down. Becker said, "Yes, I understand, J." Becker really appreciated DT. He appreciated what we did. He understood clinical research. He knew the difference between clinical research and just patient care. He said, "No problem." So then we had the follow-up meeting with Dr. LeMaistre, Dr. Hickey, and me. How did it come out? Conrad still disagrees. How about Becker? Well, Becker disagrees, too. When I met with him, he thought it was perfectly okay. We understood the difference. He said, "But he has a letter here." That's typical Becker. Becker is a swine, in the real sense. He's the inverse of Mickey. He's not well-intentioned. He's very self-serving. He's very arrogant, and he's very malevolent. When he talked to me, he led me to believe he agreed with me, and then he sat down and wrote a memo to LeMaistre saying he totally disagreed. So he had Becker's letter. He had Conrad's opinion. He said, "I'm afraid this isn't going to work." That was the end of me.

What's the next thing? Well, the next thing is, "I'll tell you what. Since you can't be vice president for patient care, what we ought to do now is combine Medicine and DT, because we can't have 2 departments of medicine, one good and one second-class. You have to bring DT and Medicine together."

There was another deal. The deal was that he would make me VP for clinical research if I gave up the possibility of being chairman of DT. I said, "No way, because DT is what I do. The only reason I want to be vice president for clinical research is to be sure DT prospers."

THE UNIVERSITY OF TEXAS
MD Anderson Cancer Center
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Chapter 17
The Beginning of the Division System, Closing Developmental Therapeutics (1983), and the Legacy of the Department
B: Building the Institution;

Codes
C: Leadership;
D: On Leadership;
C: Portraits;
C: Professional Practice; C: The Professional at Work;
B: Growth and/or Change;
B: Obstacles, Challenges;
B: Institutional Politics;
B: Controversy;
C: Understanding the Institution;
A: The Researcher;
A: Overview;
A: Definitions, Explanations, Translations;
C: Discovery and Success;
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;

Emil J Freireich, MD
0:20:20.1+
Now we're going to put Medicine and DT together. By this time, Cliff Howe had resigned. He actually was fired by Dr. Clark. The acting head of the Department of Medicine was Tom Haynie. Tom Haynie is a lovely guy. He came 2 or 3 years after I did, from Galveston. He is a very good guy, very academic. He was one of the few clinicians who wanted to be academic in the Department of Medicine. So he was ad interim chairman of the Department of Medicine. We had some meetings, and he and I saw completely eye to eye. We need to build up Medicine, put it together with DT, and have a joint training program.

I got the first training grant at MD Anderson, and we had the first clinical oncology training program. It began in '65. So by the time Mickey came in '78, we had a very well-developed fellowship program. Medicine also developed a fellowship program, and they had also some fairly good fellows, but we had totally separate training programs. We combined the training programs. We combined our faculty. We reorganized it. We had meetings of the whole department. Everybody was happy. It was going along well.

We also had 2 searches when Dr. Clark was here. They both ended up with me. Now Dr. LeMaistre says, "Now we're going to have a third search." This search was really big. They had Roger Bulger, president of the Health Science Center, on the committee. They had the guy from Southwestern, a hematologist friend of mine who was dean at Southwestern, Dr. Charles Sprague, on the committee. It was really a big committee, very prestigious.

Lesley Brunet, MA
0:24:53.9
This was going to be a new Division of Medicine?

Emil J Freireich, MD
0:24:56.7
Right. We had Department of Medicine and Department of Developmental Therapeutics. We inverted the way medical schools do. Medical schools have a Department of Medicine and divisions that are subspecialty. But because we had 2 big departments, he said, "Let's use the title 'Division' to put them together." It was the first division.

Lesley Brunet, MA
0:25:18.6
And what was it going to be called?

Emil J Freireich, MD
0:25:20.5
It was going to be the Division of Medicine. That included all the medical specialties and everything. So we're going along fine. He appointed a new search committee. It had a lot of very prestigious people on it. The search committee was asked to make 3 recommendations without priority. The 3 people they picked were Joe Bertino, John Durant, and me. LeMaistre went after Joe Bertino. Joe Bertino came by, and he had a wonderful visit. Everybody was in favor of Joe Bertino, including me. Joe Bertino went back east, where he had a lot of connections. He had social problems. He came back at least 3 times. The search for Joe Bertino went on for over a year. Finally, Dr. LeMaistre decided that he really wasn't going to come. He was just constantly leaving the door open. So he moved on to Durant. John Durant walked in the front door, came to my office and said, "J, this job is ridiculous. Conrad decides everything. What the hell would I come here for?" He was at Fox Chase Cancer Center, in Philadelphia, as head of the center. It was out of the question. He wasn't here 5 minutes, and he knew that was no job for him.

Two presidents and all the chairmen, and there was only one candidate left. What would you do if you were Mickey? It was a bad situation. If he appoints Freireich, everybody at MD Anderson is down his throat, except the people in Medicine. It was a tough decision. What would you do? Only Mickey could come up with this solution. The committee insisted that he interview Freireich. They said, "We gave you 3 names. You've rejected 2. You've got a third. You've got to interview." So Dr. LeMaistre called me. "Come to my office." I went to his

office. This was one of Mickey's greatest performances. I wish I had a video camera. He's very elegant, and he always talks with a deep voice. He had a sunburn on the back of his head from sailing. He had someone who always did his hair, and he wore nice suits. He had a manservant who used to come and turn on the lights and the air conditioning before he went anywhere. This is King LeMaistre. "J, the search committee has recommended you for this job." He talked for 15 minutes, telling me things that I knew better than he.

Lesley Brunet, MA
0:28:41.6
He was telling you things about the situation.

Emil J Freireich, MD
0:28:45.0
Yes. "This is the job." So after about 15 minutes, I was getting a little bored by the performance. I said, "Dr. LeMaistre—" "Do you have any questions?" "I have only one question. Would you ever consider appointing me to this position?"

Of course, he wasn't going to do it. So the next thing he did was he discharged the search committee, and he announced in public to the faculty that the reason he discharged the search committee was that there were leaks. They were supposed to be confidential, and there were leaks; therefore, this search committee was discharged. He appointed a new committee, appointed by only him, which were all slaves. There was only one person from DT on this committee. It was Jeane Hester. God bless her. She told me what happened. This committee was assembled and asked to review the credentials of one Irwin Krakoff.

Who is he? The prestigious search committee had at least 100 names recommended, and he was not in it. He was a hopelessly incompetent, no-good, worthless guy who ran the Rhode Island Cancer Center, and all of a sudden the committee was asked to review Krakoff and approve him. And they did, surprise of surprises. Krakoff was appointed in 1983.

Lesley Brunet, MA
0:00:38.1
Was he already here?

Emil J Freireich, MD
0:00:41.4
No. He was in Rhode Island. So in 1983, Irwin Krakoff is appointed head of the Division of Medicine. Four to six weeks after it was announced, he appeared at MD Anderson, and he called all the department chairmen to his office—Tom Haynie, Evan Hersh, and Gerald Bodey [oral history interview]. He announced that DT was being eliminated. It was the first time in the history of this institution, any academic institution, that the best department had just been eliminated. Took it out of the bylaws, and it was just eliminated. DT was Balkanized, and I was given the responsibility to be head of the Department of Hematology. This is in 1983. The other 3 were department heads, he was head of the division, and that was it.

Lesley Brunet, MA
0:02:01.4
They dissolved the department. Is that what you're saying?

Emil J Freireich, MD
0:02:01.4
Yes, just eliminated it. It was Balkanized.

Lesley Brunet, MA
0:02:07.5
No wonder I couldn't find more records on DT.

Emil J Freireich, MD
0:02:10.0
It just vanished, vaporized.

Lesley Brunet, MA
0:02:12.4
Did they change it into other names?

Emil J Freireich, MD
0:02:18.6
Everybody was given different jobs. I could give you a list of things that DT contributed to MD Anderson. I told you we didn't have any platelet program. We established the platelet within the department and then farmed it to Pathology. I personally invented the blood-cell separator when I was at NCI. I told you I got fired. When I came here, Seymour Perry couldn't do anything with it. He was a total dolt, so the thing languished for a couple of years. Then IBM came to me and said, "This project's going to die unless you move it, Freireich." So they gave me a free machine and some money. We developed the blood-cell separator. It's now used the world over. That was all done in DT. Platelets were all done in DT. If it wasn't for DT, there wouldn't be platelets anywhere, because remember, NCI was a unique place. But to do it at MD Anderson, everybody copied it. We showed it could be done. We did the blood-cell separator. That established the proof of blood stem cells, the granulocyte transfusions, and all that stuff.

We started the IV team. When I came here, doctors administered medications. If I ordered Cytoxan, they used to have 20 bottles of Cytoxan, because we'd give 2 grams, and they came in 200-milligram vials. The doctors had to go mix this stuff and administer it because the nurses couldn't do it. I said, "This is a terrible waste of professional time." So I hired research nurses, and now, of course, we have an IV team. The research nurses mixed the drugs. Then I made a deal with Mr. McKinley, who was then in the pharmacy. "Maybe the pharmacy ought to send the drugs up." And we established the unit-dose system, which is the way the place operates now. That was all done just for DT. Everybody else benefits from what we did.

When I did the blood-cell separator thing, we had to get our own space, so we created a pheresis center. In order to run it, we had to have nurses, because I didn't want doctors sitting by the bedside for 3 hours during these procedures. So I hired a nurse for DT, and we developed the research nurses, which now, of course, everybody has research nurses. When Joyce Alt came in, she immediately said obviously she should have done that. Renilda Hilkemeyer used to hate me. "Freireich, you can't do all this stuff."

So we had our own nursing service, and now we have research nurses on every unit. We had a guy come to us with these catheters in order to figure out if we could use outpatient chemotherapy. We developed the outpatient pumps, the long lines, and the catheters in my department, with our own money and our own grant money. And then, of course, everybody else acquired it, and now we have an IV team, which is run by Surgery, to put in the catheters and long lines, so we can do outpatient chemotherapy.

The people I recruited are all giants in this institution. Even though DT was eliminated, our footprints remain. Evan Hersh became head of the Department of Bioimmunotherapy, which is now headed by Dr. Moshe Talpaz. Dr. Talpaz was one of the fellows in my training program. Razelle Kurzrock, who has won awards, was one of the DT fellows in Bioimmunotherapy. Dr. Rosenblum, who does the monoclonal antibody stuff, was a DT postdoc with Dr. Ti Li Loo. David Farquhar was a postdoc with Dr. Loo. Grady Saunders, who's still retired and still working every day in biochemistry, was in DT. Gerry Bodey, who started the infectious disease program, created the reverse isolation unit with our money on the twelfth floor. He's now retired, but is coming back, I think, part-time. He became Office of Protocol Research director. Gene McKelvey, who was the first or the second vice president for academic affairs, was a DT recruit and alumnus. Ti Li Loo, of course, did the pharmacology program. Robert Benjamin [oral history interview], who's head of sarcoma, was a DT fellow and trainee. Jaffer Ajani, who's still in GI, was a DT fellow and trainee. Yehuda Patt, who still does the liver program, was a DT fellow and trainee. Giora Mavligit, who does the melanoma service, was a DT trainee.

I may not have remembered them all. Barthel Barlogie, who was very important, is now director of the myeloma program at the University of Arkansas. He's one of our distinguished alumni and was a DT trainee. Robert Livingston, who's head of Oncology at University of Washington in Seattle, was one of our DT fellows. And there was Jeane Hester and Ken McCredie. Jeane Hester developed the whole Pheresis Center. If you go down there and see that big Pheresis Center, that was all hand-built by us with our own money. When we were at our peak, before I got fired in 1983, Dr. Clark decided to expand our clinical research program, and we wrote a construction grant. The 2 floors that had been added to the entire building were built entirely by DT. We defended the grant. It was our research program. We built this whole thing.

It was typical Dr. Clark stuff. It was supposed to be all DT, but when we finally finished it and occupied it, it got reduced substantially. He took away the seventh floor entirely. We were given the entire sixth floor. Then he negotiated with Pediatrics, and they got that little bit in the

front there, and he gave the whole Gimbel Wing away. So we only got this piece of it. So that's why my office is here, because we built that, and I put myself in the middle.

Dr. Hersh was here in immunology, this section, and Dr. Bodey was over on that wall with the infectious disease. This was all pharmacology and transplant. DT had a big impact on this place, and it will last forever. That's all written up in our book.

Lesley Brunet, MA
0:09:08.7
What you wrote on DT?

Emil J Freireich, MD
0:09:08.7
Yes. In 1983, it was gone. I was very upset about it. I went to Dr. LeMaistre.

Chapter 18
Departments Undergoing Change under Charles LeMaistre
B: Building the Institution;

Codes
C: Leadership; D: On Leadership;
C: Portraits; C: Professional Practice;
C: The Professional at Work;
B: Growth and/or Change;
B: Obstacles, Challenges;
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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;

Lesley Brunet, MA
0:09:23.2
So what was your position then?

Emil J Freireich, MD
0:09:25.9
I became chairman of a Department of Hematology, because my specialty is hematology.

Lesley Brunet, MA
0:09:32.8
This was after Shullenberger?

Emil J Freireich, MD
0:09:36.2
Shullenberger had retired. The head of Hematology at that time was Raymond Alexanian. Jessie Gamble had died. Fredrick Hagemeister and Peter McLaughlin were there, and Alexanian. They mostly saw some myeloma patients. That was about it.

So in 1983, I went to Dr. LeMaistre, and I said, "Dr. LeMaistre, this is a very bad thing. You have eliminated the most creative, most successful, most financially successful department in your institution. It's not a good thing." He said, "It's very bad, J, but it was Krakoff's decision, and it was up to him."

Lesley Brunet, MA
0:10:43.0
What did you say when he said that?

Emil J Freireich, MD
0:10:45.4
There was nothing more to say. That was Dr. LeMaistre. That's the way he operates. Nothing is his decision. This was Krakoff's decision, and Krakoff was an absolute goon. You couldn't talk to him in more than one sentence at a time. He's a very unintelligent guy.

Lesley Brunet, MA
0:10:45.4
Is he still here?

Emil J Freireich, MD
0:10:59.8
Yes. He stayed for 10 years and then resigned. He created nothing but chaos, nothing positive. He brought nothing to the institution that I can think of. But I had a very negative view of him as a person, because he was the inverse of LeMaistre. LeMaistre was a movie star. Krakoff, he rolled up his sleeves and buttons were popping. He didn't comb his hair. He was crude and spoke in ugly, declarative sentences. Mickey, his prose is like music. When he speaks, you just listen to the words. No content, but beautiful stuff. It's like listening to Mozart. You don't have to walk away with anything. It's just pleasant.

So I'm head of Hematology. Mickey did a few other things before Krakoff that gave me a clue as to what he was going to do. Dr. Jose Trujillo, may he rest in peace, who was a good friend of mine, had gone to LeMaistre. He was upset about the fact that we ran the transfusion service for the hospital. We ran the platelets, we ran the white cells, we ran the Pheresis Center, and all they did was issue blood for the surgeons. He didn't think that was right. He thought that they should have their own platelet program, because Pediatrics used to get platelets from him, and surgeons used to ask him for platelets. I used to just provide DT.

So LeMaistre called me to his office, and Trujillo was in the room. He said, "You know, Freireich, you're not a pathologist. The blood banks are certified by the American College of Pathology, and you're not certified. We think that this center should come under the Department of Laboratory Medicine." "Are you kidding? Laboratory Medicine, they haven't discovered anything. They haven't brought any money in. Why would you put it in Laboratory Medicine? It's ridiculous. This is a research operation. We create peripheral blood stem cells and granulocytes and platelets for support, and we're doing research."

Well, Dr. LeMaistre, see? We both left the room feeling we had won, but I realized there was something amiss here. It was one of those things. They eventually turned the pheresis unit over to Transplant. We had several of these kinds of confrontations that were nurses and stuff, and every time it came up the same way; that is, he never made a decision. The people who were combating had to work it out amongst themselves. Trujillo and I eventually worked out a deal where it worked okay.

I'm head of Hematology, and Krakoff calls me to his office one day. "Dr. Alexanian wants to be in charge of lymphoma." I said, "Dr. Krakoff, if you want me to be head of the Department of Hematology, I'll decide who's in charge of lymphoma. Dr. Fernando Cabanillas is infinitely more qualified than Dr. Alexanian. He's more creative, he's more of a leader, and I have appointed Dr. Cabanillas, period." Confrontation with Krakoff was a bad thing. I won that battle, and Cabanillas is still head of lymphoma. He's the world's greatest. He's one of the DT fellows who came here. Leukemia, of course, is all dominated by DT fellows. Hagop Kantarjian, Susan O'Brien, Elihu Estey, and Michael Keating [oral history interview] were all trained by me. They all came here to work with me.

I had another confrontation with Krakoff. He used to meet with the department chairmen. I've forgotten how often. It was maybe once a month. We had one meeting, and he announced that Dr. Blumenschein was leaving. Blumenschein, you remember, was in Medicine, but he was very close to DT. When we were Balkanized, he became head of medical breast in the Division of Medicine under Krakoff. He built the most magnificent medical breast program in the country.

We started the Adriamycin stuff. We did the first adjuvant therapy for breast cancer. We did the first Adriamycin-Cytoxan. It's still the standard of therapy around the world. The breast cancer clinic was *the* successful clinic in the place, except for hematology, of course.

Why would Blumenschein be leaving? He fired Blumenschein because Blumenschein was one of these physicians that patients simply identified with, and his practice grew and grew and grew. He had his clinics in the afternoon, and he hired more and more personnel, but his patients came to see him. If he had 25 patients, they gladly would wait until 9:00 at night if they wanted to see Blumenschein. They came from Seattle; they were going to sit there until they saw him at 9:00. So his clinic used to run until 9:00 or 10:00 every night. The nurses complained to Krakoff, so Krakoff called Blumenschein into his office and said, "Blumenschein, you have to quit your clinic at 6:00." Blumenschein said, "Dr. Krakoff, I can't do anything about it. These women want to see me. I can only see so many patients an hour. If they want to wait, I have to see them. You have to provide nurses."

He gave him an order, and he didn't follow the order. His clinic still ran till 10:00, so he fired him. Krakoff fired Blumenschein. Blumenschein's in practice in Arlington. He took two-thirds of his patients with him. They all loved him. He's very successful in private practice, and our breast clinic has never recovered. It was typical Krakoff. Krakoff did all kinds of bad things.

He brought in this guy, Robert Newman. He's worthless. He fired Dr. Loo as head of Pharmacology. He almost destroyed Pharmacology.

Lesley Brunet, MA
0:18:10.2
I was going to ask you about Dr. Loo, because there are a lot of hints of something going on, but I couldn't tell.

Emil J Freireich, MD
0:18:16.2
I can tell you about the Loo caper. That's a very long story. That's why he was fired, and it was the reason Krakoff was brought in. I don't have evidence to support it, but my theory is that Dr. DeVita, as NCI director, suggested to LeMaistre that he hire Krakoff, because LeMaistre would have no idea where Krakoff came from. He wouldn't know him from Adam. He didn't know anybody in oncology.

The main reason that DeVita wanted Krakoff here was because we confronted DeVita repeatedly. We were the center of chemotherapy research in the United States, and DeVita wanted to control everything from Washington. He was an NCI director who was an absolute despot. He wanted to run everything. When I confronted him when Clark was here, Clark supported us. He was on the National Cancer Advisory Board. So DeVita couldn't do anything about MD Anderson. I had a confrontation with DeVita over a drug once, and Clark called him up while I was in the office. "Vince, what is this problem?" "Oh, yeah, Vince, you're right."

So the main purpose that Krakoff had in coming here was to make sure that the DT program was responsive to the NCI. That's my personal theory, although there may be many other factors, undoubtedly. Shortly after he came, he hired Newman, and Loo was asked to move out of his office and give up his lab, so he just retired. I can tell you about the Loo caper, but let me finish the Krakoff story, because it gets more interesting.

So after about 2 years of continually conflicting with Dr. Krakoff, I decided that in that 2-year period, what happened to DT happened to Hematology. I'm just a natural born leader, and we recruited dynamite people, the ones I've mentioned. Our practice in leukemia boomed. Our practice in lymphoma took off. It used to only be in Medicine, but we developed new combinations and new intensification. We were curing lymphoma. We hired Karel Dicke. We initiated a transplant program. Karel Dicke is still working in Arlington Cancer Center. Hematology was booming. It was the same problem. What are we going to do about Hematology?

Well, I made a very serious mistake of being arrogant and very self-confident and not yet having proven Mickey completely incompetent. I wrote a memo to Dr. LeMaistre. I said that Hematology had reached the point where it should be elevated to division status, because the hematology practice, the grant support, the number of research patients that we were developing, our beds and our support were such that we needed to be independent of Krakoff. I wanted to

get around Krakoff. This is not known to me for sure, but when the memo arrived to LeMaistre, the first thing he did was call Krakoff and say, "What is this all about?" Krakoff took one look at the memo and called me to his office. He was going to Japan for a meeting or something. He called me to his office and said, "Freireich, you've been fired as head of Hematology, and you're replaced by Bart Barlogie," who was one of my faculty. That was in 1985. The same thing that happened to DT in '83 happened to Hematology in '85. So now I was fired as head of Hematology, and Krakoff left town. So this got really exciting.

Lesley Brunet, MA
0:22:41.2
What happens if they fire you from head of Hematology, but you had so many grants? You're wrapped around the grants.

Emil J Freireich, MD
0:22:51.3
It just so happened that the year that I was fired, I had been awarded the first Outstanding Investigator Award in the United States. There were only 20 awarded in the United States, and I was 1 of 20 outstanding investigators.

The important thing that happened when I got fired is I now understood Dr. LeMaistre. I was very upset about this because I'd been fired 2 years ago as DT head. Now I was really angry. The first thing I did was I went around and talked to all the chairmen of all the division heads, to tell them what I thought was going on. I wanted them to understand. Everybody supported me except one, Jan van Eys. He was really a cartoon character. Jan van Eys said, "Freireich, isn't it obvious to you that they want you to leave?" I said, "Yeah, it's pretty obvious. But shouldn't it be obvious to you that I'm not going to leave?" When Krakoff fired me, I said, "Irv, I'm going to be here long after you're gone."

Lesley Brunet, MA
0:24:12.7
Is that why you stayed?

Emil J Freireich, MD
0:24:16.3
No.

Lesley Brunet, MA
0:24:16.7
I'm sure you've had many offers.

Emil J Freireich, MD
0:24:18.1
I love MD Anderson. This is the best place in the world for anyone to work. There's no question in my mind about it. That's why I've recruited outstanding people. That's why I

trained outstanding people. That's why everybody loves it. It's just a great place. LeMaistre can't ruin it and Krakoff can't ruin it. It's too good. Clark created a gem. It's a unique place in the entire world.

Lesley Brunet, MA
0:24:45.1
Yes, I have that feeling.

Emil J Freireich, MD
0:24:47.1
This place has everything. I have no intention of leaving because of some jerk like Krakoff that doesn't care about anything. I want to cure cancer. So I was very upset about being fired as chief of Hematology.

Lesley Brunet, MA
0:25:08.9
You're still on staff.

Emil J Freireich, MD
0:25:10.7
I counsel with everybody, including Conrad. Oh, maybe Conrad was dead already.

Lesley Brunet, MA
0:25:17.9
Hadn't Conrad been killed?

Emil J Freireich, MD
0:25:18.7
Had he been shot already? I can't tell you what year it is.

Lesley Brunet, MA
0:25:20.3
I think he had. I think it was '82.

Emil J Freireich, MD
0:25:28.1
I went to Fred Becker, and Fred Becker really helped me, because I was so angry, so upset, and I had no idea what the next step would be. Dr. Becker said, "Freireich, I've learned one thing in dealing with Dr. LeMaistre. If you want to accomplish anything, you use 3 words: 'Board of Regents.' That gets his attention."

I said, "Now, that's a very good idea." So I made an appointment with Dr. LeMaistre, and I said, "Dr. LeMaistre, I've been fired as head of Hematology. I think it's bad for the institution, and I wish to bring this to the attention of the Board of Regents." Whoa! Remember, everybody

always gets what he wants from Mickey. That's why they call him Mickey, as in Mickey Mouse.

Lesley Brunet, MA
0:26:28.2
That's not really why they call him Mickey.

Emil J Freireich, MD
0:26:32.0
It's his name. He calls himself Mickey. I call him Mickey Mouse. He's about that level of skill and intelligence. Mickey Mouse was a great entertainer. You'd want to go see him anytime, but you don't want to put your life in his hands.

"Board of Regents," he said. "Well, J, in order to get to the Board of Regents, there's a protocol you have to follow." I said, "Yes. That's why I came to you. What is the protocol?" "Well, first you have to go to the vice chancellor for Health Affairs. Then you have to go to the chancellor, and the chancellor goes to the regents." "Okay. I want to talk to the vice chancellor for Health Affairs, Dr. Charles Mullins." "I'll arrange an appointment." "Thank you."

I went back to doing my thing. One day I get a page from Dr. LeMaistre's office to come to his office. The secretary says, "Dr. Mullins wants to speak to you." "Hello, Dr. Mullins." "Hi." "I want to talk to the Board of Regents." "What's the problem?" "I think that Dr. LeMaistre and Dr. Krakoff are harming MD Anderson. I love MD Anderson, and I don't think it's right. I think the regents should try to straighten it out." He listened very carefully, and he said, "You know, the way the University of Texas is organized, the president, once appointed, has absolute authority. The administration in Austin has no effect on the institution. So whatever Dr. LeMaistre wants to do, that's his privilege." I said, "Well, thank you, Dr. Mullins. I think that's a very good system, but in this instance, I don't think it will work, and I would like to go to the regents." "Well, you can't do that." That was the end of that conversation. I forgot all about it.

Incidentally, Dr. Mullins said, "Put it in writing." So I wrote him a memo. "Dear Dr. Mullins: Here's what's wrong." A couple of weeks later, I get another phone call from Dr. LeMaistre's office. "You have an appointment with Dr. Hans Mark in Austin, the chancellor." So I get in my car and drive to Austin.

So I walked into the chancellor's office in Austin; it was a very impressive place. Sitting in his office was Dr. Mullins, Dr. LeMaistre, and Dr. Mark. "Dr. Freireich, what's your problem?" Well, I told them the problem.

It's interesting, because LeMaistre was in the room, but by that time I understood him. I didn't hesitate to say that I thought he was incompetent, in his presence. I would say it to him anytime. I said that I think that the decision to hire Dr. Krakoff was harmful to the institution, the decision to eliminate the Department of Developmental Therapeutics was harmful to the institution, and the decision to eliminate me as head of the Department of Hematology was harmful for the

institution. I thought it was time for the Board of Regents to review the situation of the administration of MD Anderson Cancer Center. Dr. Mark said, "Dr. LeMaistre, what do you think?" "Well, Freireich's got a good point."

Okay. That's the end of the meeting. It was very emotional for me. I'm just a young whippersnapper, and I'm talking to the kings. I got in my car, drove back to Houston, and went back to work. I've never stopped working. I'm still taking care of leukemia patients. I do what I do every day.

About 3 weeks later, Dr. Mark appears in my office. "Freireich, I want to know what you do." I told him what I do. I took him on a little tour of the protective environment, the Pheresis Center, the clinic, and gave him some figures. "Thank you."

Dr. Mark was too good for chancellor of the University of Texas. It's a position that was perfect for Mickey. You don't have any idea the dimension of the University of Texas. I do, because I was on UT Faculty Advisory Council. But Dr. Mark was a wonderful chancellor. One of the good things he did was he created the Faculty Advisory Council and the Student Advisory Council. It was true what Dr. Mullins said. The way the university ran was the way it ran in 1860. You appoint a president, and he ran the Health Science Center. The administration had nothing to do with it. But when it gets big, there has to be someone who is worried about it. Of course, when Mark became chancellor, he realized that the presidents were not all perfect. In fact, the president of Dallas was fired, and the president of San Antonio was fired. They had to fire Mickey. He was a hopeless case. But Mark realized that the administration of the University of Texas required someone with academics, intelligence, and leadership, who could make the university run. It's too big to be totally Balkanized. Someone has to be thinking about it. The legislature can't run it. The coordinating board can't run it. You need administration in Austin which is strong, and Mark realized that to communicate with these presidents, he had to have some information other than what the president feeds him, because that's all PR. The president just tells him how great he is. He wants to know what's going on there.

So each campus has a faculty. They elect a chairman. The chairman of the Faculty Senate is a faculty member that goes to Austin and tells them what's going on. All the campuses talk to each other. They have committees, and they have structures, so we have common language and common grounds and common degree-granting authority. It's a wonderful thing, all established by Mark.
Mark is a wonderful guy. He really cared about it, so he came to MD Anderson to see how it worked. Before, when I had left his office, I said, "Dr. Mark, I would like to go to the regents with this." Mark was offended. He said, "My responsibility is the regents, not yours, Freireich. So if you have any problem, come to me, and I run to the regents." Then, to represent the regents, he came to my office. He really cared about MD Anderson. He found out how it worked. So I was very flattered that he would take the time. I mean, he's got lots of stuff to worry about.

Two weeks later, the most interesting meeting that ever happened occurred. I was called to the Governor Allan Shivers Conference Room. Dr. LeMaistre was at the head of the table. Also at the head of the table was the chairman of the Board of Regents, and around the table are sitting Krakoff, Becker, and all the division heads. This is the guns of MD Anderson Hospital. They had obviously been in session for many minutes before I arrived. It was like a courtroom proceeding. The secretary takes me in the side door. Whoa! What am I doing here? "Dr. Freireich, please take a seat." Dr. LeMaistre said, "Well, you're concerned about these decisions. Tell us what you're concerned about."

Lesley Brunet, MA
0:06:44.0
Haven't you already told him enough times?

Emil J Freireich, MD
0:06:47.2
Not the chairman of the Board of Regents. This is my hearing with the regents. Dr. Mark is not here, but Dr. Mullins is here. Dr. LeMaistre and the chairman of the Board of Regents are there. They want me to tell Mr. So-and-so what my problem is. Everybody in the room knows what my problem is.

I said, "I just want you all to understand one thing. Emil J Freireich is completely happy. There is nothing that you can do for me. Nor is there anything that I want you to do for me. What I want you to understand is that decisions are being taken here which are harmful to the goals of this institution: research, education, and patient care. That's what I'm talking about." Everybody listened very quietly. They had already had a meeting and decided everything. So then Dr. LeMaistre makes a speech. "J, here's what we decided to do. You're now director of the Adult Leukemia Research Program." "What is that?" "Since your primary interest is in leukemia, and you want to cure it, with Adult Leukemia Research Program, your office will remain intact. You'll have administrative people. You'll have secretaries and clerks. You'll be able to do your work. There will be no interference with progress in leukemia research, and you will have all this authority." "Thank you." End of meeting.

Lesley Brunet, MA
0:08:49.7
Did they just want to make you happier so you would shut up?

Emil J Freireich, MD
0:08:53.5
They wanted to shut me up. After that I repeated, "This will do nothing to the problem, and I don't need anything for me, but I will continue to do the best I can as director of Adult Leukemia Research Program."

Lesley Brunet, MA
0:09:12.0

Is this just an organizational change, or were they actually giving you something?

Emil J Freireich, MD
0:09:14.4
It was organizational change. Don Pinkel had been recruited by Jan van Eys to work in the Department of Pediatrics, and he was having the same problem with van Eys that everybody did. He was hopeless. Van Eys was hopeless. Pinkel had already made a stink, and I had made a stink.

Mickey is very good at administrative things, so he got the idea that I would be director of the Adult Leukemia Research Program and Don Pinkel would be director of the Pediatric Leukemia Program. We would work together, and leukemia would be a prime activity at MD Anderson. Everybody would be happy, and we could leave Krakoff and van Eys alone. Pinkel and I were administratively to report to none other than Fred Becker.

Lesley Brunet, MA
0:10:06.4
Was that a little unusual?

Emil J Freireich, MD
0:10:13.3
Becker was to assure that this clinical research activity continued. In other words, he was going to have an influence on clinical research.

Lesley Brunet, MA
0:10:23.5
Who's in charge of patient care now?

Emil J Freireich, MD
0:10:26.8
When Conrad got shot, it was Stratton Hill [oral history interview]. Howe and I had a confrontation at one point about admissions to leukemia. We were developing our own practice, and Howe wanted to control our practice, and he declared that we could not admit patients without his office approval. We had a meeting with Dr. Clark, and Dr. Clark said, "Cliff, you can't do this kind of thing. It's not useful." So he fired him, and Stratton Hill became Director of Clinics.

So Howe was replaced by Stratton Hill as head of clinics. Then when Conrad was shot, as I recall, Stratton Hill became director of the hospital, because he was already in charge of the clinics. Stratton Hill was a very colorful guy, and he was a positive guy. He was here before I came, and he liked DT. We got along very well.

Our clinic was thriving, everything was going fine, and then he got fired and replaced by Joe Ainsworth. Joe Ainsworth was the perfect solution to running anything, because he was exactly

like Dr. LeMaistre. He didn't care about cancer. He didn't care about anything. He was a retired family doctor. He was the sweetest guy. He still is a lovely guy and very well intentioned, but no academic nothing. Unlike Conrad, he wasn't a cruel, vicious, arbitrary person. He was a very nice, sweet, and kind person. After Joe Ainsworth, I guess Charles Balch came next, and Balch is still alive. Anyhow, we were supposed to report to Becker, and we had a budget and all that, but Becker didn't care about it. Nothing ever came of it. So that was the end of the Freireich caper.

Lesley Brunet, MA
0:13:09.4
That was the end of your caper?

Emil J Freireich, MD
0:13:10.5
That was the end of the caper.

Chapter 19
Head of the Adult Leukemia Program and a "Project Reassignment" Year at the NIH
B: Building the Institution;

Codes
C: Professional Practice; C: The Professional at Work;
C: Education at MD Anderson;
B: Growth and/or Change;
B: Obstacles, Challenges;
B: Institutional Politics;
B: Controversy;
C: Understanding the Institution;
B: Critical Perspectives on MD Anderson;
B: MD Anderson History; B: MD Anderson Snapshot;

Lesley Brunet, MA
0:13:13.2
But you did stay head of the Adult Leukemia Program?

Emil J Freireich, MD
0:13:18.0
Well, it's nothing; it's meaningless.

Lesley Brunet, MA
0:13:19.4
It's just a title?

Emil J Freireich, MD
0:13:21.6
You might as well call me "Chief Janitor."

Lesley Brunet, MA
0:13:22.9
But it didn't affect your grants?

Emil J Freireich, MD
0:13:36.3
I had just gotten this Outstanding Investigator Award grant, which was a 5-year grant. That was a million and a half dollars or something like that. Krakoff called me in his office, and he said,

"We decided you should return the money to NIH." So I made an appointment with Dr. LeMaistre. Jim Bowen was here at the time, and I love Jim Bowen. Jim Bowen sat in his office and listened to me scream until 9:00 one night, telling him how ticked off I was at Krakoff. He was a wonderful guy. So we had this meeting, LeMaistre, Bowen, Krakoff, and I. I said, "If you return this money to the NCI, you're going to read about it on the front page of the *New York Times*."

Lesley Brunet, MA
0:14:59.6
What year was this?

Emil J Freireich, MD
0:15:03.3
I can't tell you exactly, maybe '87. So that grant was agreed. But what he did do was he insisted that I resign as director of our training grant. That was given to McCredie. I also had to resign as head of the 2 program project grants I was in charge of, and they both went down. I also had to give up the Clinical Research Center. The only thing I had was the OIA, Outstanding Investigator Award.

Lesley Brunet, MA
0:15:49.5
How can they make you give up the grant if it's already been awarded to you?

Emil J Freireich, MD
0:15:51.6
They wrote to NCI and said, "We want, as an institution, to change the principal investigator."

Lesley Brunet, MA
0:15:56.4
And they had the privilege of doing that?

Emil J Freireich, MD
0:15:58.3
Evidently. Certainly when DeVita was there, he was more than happy to have that happen.

Lesley Brunet, MA
0:16:03.3
But they couldn't take away your Outstanding Investigator Award?

Emil J Freireich, MD
0:16:08.7
The OIA was the only thing I had, and it turned out to be very important, because I had no support for this so-called Adult Leukemia Research Program. There was a young guy that Trujillo had just recruited named Sanford Stass, and Dr. Stass was a hematopathologist. We

started to talk about leukemia and what we needed to do. Stass wanted to build up hematopathology.

Lesley Brunet, MA
0:16:46.4
What is hematopathology?

Emil J Freireich, MD
0:16:48.5
That's the pathological part of hematology. In other words, bone marrows, bloods. Dr. Stass decided that what was wrong with hematopathology was there was no research; it was all service. He wanted to make it a research department, so he did. He recruited people. He recruited Kun-Sang Chang from Baylor. He recruited Maher Albitar, who still runs hematopathology and leukemia. He recruited Ming Lee, who runs the molecular lab.

We established the first molecular service lab in the country under Dr. Stass, and all this was done with my money. That's what I decided to do. Leukemia was headed by Barlogie; they had plenty of money, and hemopath had nothing. So all these people were hired with my grant, and we built the Hematopathology Department to where it is today.

See, every time I got a big honor I got fired. When I got the GM Cancer Research Foundation prize, my recollection is, it was about '83. That's when Krakoff came in and eliminated DT. When I won the Outstanding Investigator Award in '85, that's when I got fired as Chief of Hematology. I was chairman of the American Society of Clinical Oncology, ASCO, in 1980.

In 1990, I went to the Association of American Physicians. I was the only faculty member at MD Anderson who was in the Association of American Physicians. That's the most prestigious internal medicine research society. All the chairmen in Departments of Medicine are elected to that. It's an honorary society. You have to be elected by the membership based on your research. In 1989 or so I went to the meeting. Dr. Samuel Broder had just been appointed director of the Cancer Institute to replace Vince DeVita. Vince DeVita, after whatever number of years of despotism, was finally fired, and he went to Memorial to be physician-in-chief. He got fired there; he's now at Yale.

Sam Broder was nominated and elected to the Association of American Physicians, and I went to the meeting. We have a dinner and a cocktail reception where 200 or so members get to renew old acquaintances. I just bumped into Broder, and I said, "Congratulations on being head of the Cancer Institute. You can do a lot in that position." He said, "You know, I've been thinking that I don't know a lot about cancer." His research had been in AIDS. He said, "I could use someone to advise me. Why don't you come and spend a year's sabbatical at NCI with me?" That was a period in my life where we thought we could do it. I talked to my wife. But Texas doesn't have sabbaticals.

Lesley Brunet, MA

0:20:56.3
I didn't realize that.

Emil J Freireich, MD
0:20:58.1
University of Texas does not have a sabbatical program, but we have a thing called Project Reassignment. You can remain on the faculty and be assigned to work on a project outside the institution. I went to Jim Bowen, and he said, "Yeah, we can do it." He did the thing through LeMaistre, and, by golly, we went to NCI for a year. It was a fantastic year. It was the best year of my life. We really had a wonderful time. I was there as an adviser to Dr. Broder. I had an office right next to his office. I went to all the big meetings, the National Cancer Advisory Board, and all those things. After I was there for about a week and I had met with Broder every day, he asked me what I thought the most important thing he needed to do was. I said, "What you have to do is improve funding for clinical research, because clinical research is being eliminated. It's all going to PhD's." "How can I do this? There are no grants coming in."
I said, "Well, you have to spend it on training."

Chapter 20
A "Flexner Report for Cancer" and Commitment to Education
B: Building the Institution;

Codes
C: Professional Practice; C: The Professional at Work;
B: Education; D: On Education;
C: Education at MD Anderson;
B: Growth and/or Change;
B: Obstacles, Challenges;
B: Institutional Politics;
B: Controversy;
C: Understanding the Institution;
B: Critical Perspectives on MD Anderson;
B: MD Anderson History; B: MD Anderson Snapshot;

Emil J Freireich, MD
0:20:58.1
Being a scholarly person, I had read about the Flexner Report. At the turn of the century, Abraham Flexner was chairman of a committee of the National Science Foundation. They looked at medical education in the United States, and the idea was that science has to come to medicine somehow. All the medical schools were proprietary schools. Flexner did 30 medical schools in a year and wrote a report saying you had to have basic sciences and you got a doctorate degree. It changed medical education.

I said, "I need to do a Flexner Report for cancer." He said, "Good idea." So we sat down, and I prepared a list of all the major training programs in the major cancer centers, and I did what Flexner did. I went 3 days to each of the major cancer centers, and I wrote a report, which was published. I worked with Brian Kimes, who was the head of the training division, and we organized a workshop with training directors from all the major cancer centers. This led to the "K Program," which is now all over the country, the K30 program for physician-scientist training. We also have the K12 program for patient-oriented research training. We have both K30, which I run, and a K12, which Dr. Robert Bast runs.

That was a very productive year. In one year we turned the direction of NCI funding around. Tragically, Dr. Broder didn't last very long. He got replaced by Richard Klausner, but I liked Sam Broder. He and I were very, very good friends.

Lesley Brunet, MA
0:24:09.4

You weren't doing clinical care during that year you were there?

Emil J Freireich, MD
0:24:15.9
No, not at all. All my students are smarter and better and more accomplished than I am—all of them. I had Ken McCredie, Michael Keating, Eli Estey, and Kantarjian. These guys are so good. I just have trouble keeping up with them. These are real geniuses. They're all motivated. They want to cure cancer. They were doing fine. They didn't need me. They don't need me now. They're nice to me, but they don't really need me. It's like when you fix a car, it will go by itself. They're all self-going.

But what did happen is that I tried to maintain the OIA grant. Dr. Stass administered it ad interim, and we had to do a lot of paperwork with the NCI. They made a lot of exceptions, because it wasn't really legal. They were funding me, and I was there. But we kept it going. It went for another year or 2 after that, but on the recompetition, it went down. So that ended in '92.

When Dr. Trujillo died they did a search for the chief of Laboratory Medicine, and it was so obvious that it should have been Stass, who was a fantastically accomplished person. But Stass had the same problem that I had. He was just too good. The people in Pathology were very resentful that Lab Medicine had all this money and space and publications. They came up with this Armand Glassman recruitment, and he's a catastrophe. He's still here. Stass left and went to the University of Maryland in Baltimore, and there he's become director of their cancer center. He's chairman of the Department of Pathology. He's a fantastic guy. I've told Dr. Mendelsohn [oral history interview] that we ought to recruit him to come back, and he's actually interested.

But that was a terrible, terrible thing that happened. So that was the end of my grant, and that program is also Balkanized. Chang is in Ralph Arlinghaus's [oral history interview] department, and Albitar, too. But we're still going. So that's most of what I had to tell you.

We don't know how Conrad got shot. Do you know the date?

Lesley Brunet, MA
0:27:15.2
I don't have the exact date on here, but I think it's '82.

Emil J Freireich, MD
0:27:23.2
Something of that kind.

Lesley Brunet, MA
0:27:24.1
It's quite a mystery.

Chapter 21
Views of Charles A. LeMaistre and MD and R. Lee Clark, MD
A: Overview;

Codes
C: Leadership; D: On Leadership;
C: Portraits;
C: Professional Practice; C: The Professional at Work;
B: Institutional Politics;
B: Controversy;
C: Understanding the Institution;
B: Critical Perspectives on MD Anderson;
B: MD Anderson History; B: MD Anderson Snapshot;

Emil J Freireich, MD
0:27:27.1
Yes. I had a number of meetings with Dr. Conrad over very controversial issues: money, space, and academic fortunes. Every time I used to say something like, "Fred, this is so important that I think we ought to get Dr. LeMaistre's input," he would say, "Don't worry about that. I'll take care of it." Dr. Conrad had maneuvered himself into a position where he was dealing directly with the regents. He never even spoke to Dr. LeMaistre.

Lesley Brunet, MA
0:00:03.3
Did he have a personal relationship with one of the regents?

Emil J Freireich, MD
0:00:05.2
No.

Lesley Brunet, MA
0:00:07.1
How did he have this access?

Emil J Freireich, MD
0:00:08.0
Because the regents already knew that Dr. LeMaistre couldn't do anything. They dealt with him like I did. You don't have to be stupid to understand how LeMaistre operates. He never makes a decision, so if you wanted a decision made at MD Anderson, it had to be done by Conrad. So

Conrad can go to LeMaistre, in which case nothing would happen, or he can go to the regents, in which case they can decide Conrad can do this or that or the other thing, and he would do it.

Conrad was within 15 minutes of replacing Dr. LeMaistre. Dr. LeMaistre would resign to go into art or something. He was totally excluded from the place. All the big donors were dealing with Conrad. Everybody in the institution went to Conrad. Dr. LeMaistre's office was a desert. No one went there. No one even talked to him. Conrad ran MD Anderson Hospital in every detail. Even Dr. Becker couldn't get to Dr. LeMaistre. Dr. LeMaistre's loaded rich.

Lesley Brunet, MA
0:01:38.1
How was that?

Emil J Freireich, MD
0:01:40.4
He served on the board of directors of every corporation in town. The same thing he did for the University of Texas. Suntanned head, perfect hair, deep speech, he was perfect. If you need someone on the board of directors to approve what you do, you'd hire LeMaistre. There's no one better. Every board of director pays him $20,000 a year. You do 10 of those, it's $200,000.

The university paid for his house. All the people who were here took care of him. He had a driver and a car. His wife had a higher administrative person working only for her than I did when I ran DT. Dr. LeMaistre abused his position of power more than any person who had ever been in the University of Texas System. He had everything. He had a private house. We used to have parties at his house. He had the personnel dressed up in black gowns with white aprons and white gloves working the kitchen. They were MD Anderson employees. He had valets, like you do when you go to a fancy ball. They were all MD Anderson employees. He had the police there with guns directing traffic. I went to one of these parties. He had a lady playing the harp. I couldn't believe it. It was obscene.

Lesley Brunet, MA
0:03:00.1
Didn't you say he was a Frank Erwin protégé?

Emil J Freireich, MD
0:03:07.8
Erwin trained him.

Lesley Brunet, MA
0:03:12.0
It might be a pattern.

Emil J Freireich, MD
0:03:14.5

Dr. LeMaistre abused his power more than any president in history. Dr. Clark probably did, too, but Dr. LeMaistre put his personal interest above everything. He had everything. He had 12 secretaries, 2 private people, and an office. He built a track room for himself to keep his perfect figure. People came in and primped him for every appearance.

Dr. LeMaistre was just like a king. He had everything. The only thing he didn't get was at one point he wanted a helicopter for his personal use to go to Austin. He didn't like driving. But they turned him down; he didn't get his helicopter. But other than a helicopter and a private plane, he had everything. He was very rich. So if you're rich, in a position of free housing, free servants, and with a huge salary—he had $500,000 a year for entertainment from the PRS. He had these big parties for the board of regents. He raised money. He knew how to do all that stuff. He was like a king.

LeMaistre is great. We'd have functions, like when I got my 35-year pin. He put his arm around me. "J, you're terrific." This is a classy guy. My wife used to say, "Dr. LeMaistre doesn't care about you." I'm insignificant in his life. He was significant in my life, but I'm not significant in his. He probably didn't know I was alive. What did he care about me? I was trivial. I like him. He's a very high-quality person. He was just in the wrong job.
I have another anecdote about Dr. Clark's 80th birthday.

Lesley Brunet, MA
0:06:08.3
He'd had a stroke, hadn't he?

Emil J Freireich, MD
0:06:11.1
No, it was after that. He was still compos at 80.
Lesley Brunet, MA
0:06:17.6
But I thought he had difficulty speaking.

Emil J Freireich, MD
0:06:20.3
It comes later. Mickey came in '78, so he was 72. He lasted 2 or 3 years. So by '81 or '82, they physically moved him out of MD Anderson. They had movers come and take his desk out of the hospital that he created.

Lesley Brunet, MA
0:06:45.0
Is that when they moved him to the Medical Arts?

Emil J Freireich, MD
0:06:49.1

Right. It was a horrible, horrible thing. Talk about ingratitude. At least he was a professor emeritus. He was a university professor.

Lesley Brunet, MA
0:06:57.8
What was he doing? Did he have an official function?

Emil J Freireich, MD
0:07:07.1
That's the anecdote.

Lesley Brunet, MA
0:07:33.2
He was born in '06, so he would've been 80 in '86.

Emil J Freireich, MD
0:07:50.4
He had a party at the Houston Country Club, and he invited a handful of us old-timers. I was one of them, along with Dick Martin and a couple of others. He made a nice speech. He had a few drinks, and I said, "Dr. Clark, how could you allow the decision to hire a guy like LeMaistre? Didn't you understand what was going to happen?" I had been fired twice by then, and I was very bitter.

Of course, this is my perception of what he said. It may not be what he said, and he's not here to testify, but this was one on one. He said, "Well, when I resigned as president, I was still young and vigorous, and I was concerned that the person who comes here would change the direction of MD Anderson. I wanted to be sure it continued in the direction that we had pointed. It seemed to me like LeMaistre was the perfect choice because he's a very ineffective person. He was as chancellor, and I'd dealt with him for 10 years when he was chancellor of the university. I just figured that I would continue to run MD Anderson." The legislature and the regents didn't want him as president. He resigned.

Lesley Brunet, MA
0:09:19.7
Wasn't that a little naïve?

Emil J Freireich, MD
0:09:22.5
No, I think it's pretty smart, because everybody here respected Clark and no one respected LeMaistre. In fact, it was very possible that with LeMaistre as the figurehead he could've been the power behind the throne. That was his theory, and that's why he allowed it to happen. But, of course, it didn't happen. It didn't happen because he didn't anticipate the forces outside MD Anderson that were stacked up against him, and that's what Mickey buckled under to. See, all

the other university presidents hated MD Anderson for the same reason everybody here hated DT. MD Anderson had the largest state budget of any UT installation.

Dr. Clark went to the legislature. He always got what he wanted. The president of Southwestern at Dallas said, "You lost all the presidents." They hated MD Anderson because we had everything we wanted and they didn't get anything from the state. We were the only ones who had patient care support. All the other presidents hated Clark and MD Anderson, and they descended on Mickey. He did the Mickey thing, which is you go where you're being pushed. So the forces lined up against Clark were strong enough so that LeMaistre evicted him.

He didn't have any effect on the university, and he was very miserable for the rest of his life, because his whole life was MD Anderson. He was offered a job in the government, Secretary of HHS or something. He talked to me about it. I loved Dr. Clark in the end. In the beginning I hated him because he double-crossed me, but in the end I really loved him. He said, "Freireich, you have to stay where your roots are." When I was given job offers, I went to him, and he told me the same thing. He said, "When you build something, you have to stay with it." So he stayed with MD Anderson. But when they cut off his roots, he was very unhappy. He worked in the UICC, but he was very unhappy.

Chapter 22
An NCI Audit and Problems with a Protocol and Leadership
A: Overview;

Codes
A: The Researcher;
B: Research;
C: Leadership; D: On Leadership;
C: Portraits;
C: Professional Practice; C: The Professional at Work;
B: Obstacles, Challenges;
B: Institutional Politics;
B: Controversy;
C: Understanding the Institution;
A: Overview; A: Definitions, Explanations, Translations;
C: Discovery and Success;
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: Ethics;

Emil J Freireich, MD
0:09:22.5+
Now I'll tell you something about the Loo caper, and then I'll tell you something about why I had my heart attack in 1987.

Lesley Brunet, MA
0:12:00.8
I didn't have that down.

Emil J Freireich, MD
0:12:09.1
That was during the Krakoff period. It was 1987. I'm director of Adult Leukemia Research. I'm nothing.

Lesley Brunet, MA
0:12:35.3
But you had your OAI.

Emil J Freireich, MD
0:12:37.6
Yes.

Lesley Brunet, MA
0:12:38.5
Did you get that about ’87?

Emil J Freireich, MD
0:12:40.6
Yes. The NCI did an audit, and they discovered that for one of our protocols, they charged us with giving an experimental drug to patients that were not approved to be on that protocol. The drug was AMSA. When the audit was completed, and this report was received by Krakoff, he had to take action. The action he took was to suspend the privileges to prescribe investigational drugs from all the doctors in leukemia.

Lesley Brunet, MA
0:13:40.7
Including you?

Emil J Freireich, MD
0:13:42.3
All the doctors in leukemia. So all the leukemia patients, if they were to get an investigational drug, had to take the prescription to Dr. Krakoff’s office, and he had to approve it. The implications of that are pretty serious. First, suspension of privileges is something you’re asked for every appointment you get, for licensing. Suspension of medical privileges is a major black mark on your record. Dr. Krakoff suspended the privileges of the 7 or 8 best doctors at MD Anderson. This is very serious. Secondly, it has implications for the granting agencies, for funding, and for research.

So this got our attention, and we looked into it very carefully. Then it turned out that Dr. Krakoff’s office was incompetent and had failed in their job. When the auditors found it was wrong, they blamed it on us. It was a convenient time to do it. We had a protocol where we were using AMSA for acute lymphoblastic leukemia, ALL, and acute myeloblastic leukemia, AML. It was used for remission induction and for maintenance. After the study had been going for about 3 months, we noticed that the response rate in AML was very good but the response rate in lymphoid leukemia was bad. So we wrote an amendment to the protocol, and we said that ALL patients would not be induced with AMSA. They would be induced with a regimen that was analogous to CHOP, the vincristine-prednisone-based regimen.

But when the patients achieved remission, there was a second phase to the protocol. The patients were maintained on AMSA or Adriamycin. I forgot what the comparison was. So although the patients didn’t get AMSA for induction, when they were in remission, they were randomized to receive AMSA. When the auditors came out, they found that the protocol said the ALL patients

shouldn't get AMSA, but they were getting AMSA in remission. So they said, "You violated the protocol." But we didn't violate the protocol. All they needed to do was look at the amendment.

After the suspension came out and we did this investigation, we immediately responded to the NCI, to Dr. LeMaistre, and to Dr. Krakoff, and we had a series of these very unpleasant meetings. I had one with Gene McKelvey, who was then director of the Office of Protocol Research, one of my trainees. I called him up, and I said, "Gene, this business is clearly not a violation of protocol on our part, and this suspension of privileges is totally out of order." He said, "I agree with you, J, but we have to get the NCI taken care of. I was present at the proceedings, and there is nothing pejorative that will reflect on you or your faculty. This is strictly a thing that we can get straightened out with the NCI, so don't worry about it."

I went to Dr. Ainsworth. Dr. Ainsworth said, "Well, suspension of privileges, this is very bad." So within 2 days, we got a memo from Dr. Ainsworth that said, "There is no suspension of privileges. Nothing will go in your medical record for your medical career. You don't have to worry about your licensing. This is strictly a procedure within house that has to do with research drugs. Don't worry about accreditation." So we fixed that with Dr. Ainsworth, thank goodness.

Finally, I wrote a memo, and I said, "I insist that Dr. LeMaistre write a memo to the Cancer Institute clearing our name and saying that there was no violation of protocols," and I had it documented. I also went back and looked at the patient outcomes, which is very interesting. This is duration remission for ALL patients who had AMSA. Their remission duration was actually better if they got AMSA than if they didn't. So not only was the protocol not violated, but it was a good treatment, and we've subsequently learned that that's correct.

Lesley Brunet, MA
0:18:42.8
So did LeMaistre write the letter?

Emil J Freireich, MD
0:18:45.4
Come on. You know better than that. Dr. LeMaistre never did anything. We're sitting in his office, Dr. Krakoff—the bull in the china shop—Dr. Freireich, and Dr. LeMaistre. What would the hollow balloon do? We'll get Dr. McKelvey on the phone, and he'll decide. LeMaistre was always performing. He was always onstage. When you went to his office, he always had his secretary call. "Hello. Senator So-and-so? Yes, I'll be right there, J. Just a minute. Yes, Senator, I'll be in Austin." You had to be impressed with his importance. He stood up dramatically, went to the desk, and he put on the speakerphone. "Mary, get Dr. McKelvey on the phone." Dr. McKelvey was on the phone, and we're sitting in the room. "Gene, Freireich says we should write a letter to the NCI."

McKelvey totally folded. I never forgave him for that. I've never spoken to him since then. He retired and became a minister or an evangelist. I still see him every once in a while. I couldn't believe he would do that. It was because he was terrified of Krakoff.

That weekend, I had my heart attack. I was so insanely angry about this. I had a bad heart attack. It was a transmural infarct. I was in the 80 percent mortality category, but in 1987 they were doing the TPA trial, and I was one of the fortunate ones that got TPA. It saved my life. They did a bypass, and 14 years later, I'm still cooking. I should be dead, but here I am. I attribute it entirely to LeMaistre and Krakoff. Oh, I was so insanely angry about that. They never did do it; they never fixed it. They just let it be. If you go to the NCI records, you'll still find that we violated the protocol, which is just a damn lie.

Lesley Brunet, MA
0:21:00.7
But it didn't cause any problems for you?

Emil J Freireich, MD
0:21:03.8
Well, yes, it did. We got fired from the cooperative group, and it had consequence for our grant support. It had all kinds of consequences. We never recovered. To some degree, that's still the case. We're still audited all the time because of that episode.

Lesley Brunet, MA
0:21:23.9
Is it because of that episode, or is there more and more auditing being done because of the problems that have been occurring in the last few years?

Emil J Freireich, MD
0:21:31.0
It's just part of it. Things are complicated.

0:21:55.9 (End of session)

THE UNIVERSITY OF TEXAS
MD Anderson Cancer Center
Making Cancer History®

Emil J Freireich, MD

Interview Session Four: 13 August 2001

Chapter 23
Charles A. LeMaistre's Administrative Success
A: Overview;

Codes
C: Leadership; D: On Leadership;
C: Portraits;
C: Professional Practice; C: The Professional at Work;
B: Critical Perspectives on MD Anderson;
B: MD Anderson History; B: MD Anderson Snapshot;
B: Institutional Politics;
B: Controversy;
C: Understanding the Institution;

Emil J Freireich, MD
0:00:35.4
Dr. LeMaistre, when he came in 1978, was recommended by Dr. Hickey to the executive committee for professorship. In 1978, our Promotion/Tenures Committee was the executive committee of the medical staff and the basic science board for the PhD's. I was on the executive committee as chair of the Department of DT, and Dr. Hickey brought Dr. LeMaistre to the executive committee to be appointed as a professor.

These minutes were distributed beforehand. They came to my desk, and I went over them. The first thing you look at for promotion/tenure is publications. Well, Dr. LeMaistre had a list of 18 publications labeled "Selected Scientific Publications," which we now know is 100 percent of those that exist. Of the 18, there were only 6 that were research publications. The others were all summary-type things, case reports. They were trivial. All 4 papers were published before 1959, and they all existed from his fellowship training at Cornell. So from his fellowship on, he had never done any research.

What did he do? This instructorship is during his training at Cornell. In 1954 he went to Emory. He ran the outpatient clinic, and he was chair of the Department of Preventative and Community

Medicine, which in 1957 was totally trivial. In 1959 he came to Southwestern Medical School. Now, that looks academic. This guy was a professor of preventative medicine. Now he's a professor of internal medicine. Southwestern Medical School is a wonderful medical school.

Lesley Brunet, MA
0:03:03.0
Now it is.

Emil J Freireich, MD
0:03:05.1
It was then.

Lesley Brunet, MA
0:03:06.2
Even in '59?

Emil J Freireich, MD
0:03:06.8
Oh, yes.

Lesley Brunet, MA
0:03:07.6
It was only 10 years old.

Emil J Freireich, MD
0:03:08.4
It was a wonderful medical school. Don Seldin was very good. He was appointed in '59. He ran the outpatient clinics. He ran this Woodlawn Hospital for 6 years. In 1965 he became associate dean for 1 year. That's when what's-his-name picked him up, in 1966.

Lesley Brunet, MA
0:03:37.8
Charles Mullins?

Emil J Freireich, MD
0:03:42.5
No. Frank Erwin. So I looked at this, and I said, "Well, he may be qualified to be president, but he's certainly not qualified for a professorial title." Then there was a hush over the audience. There were about 15 people on the executive committee. Dr. Hickey didn't say anything, bless him. Then we had a secret ballot, and the ballot was 14 to 1. So he became a professor.

Lesley Brunet, MA
0:04:15.3
But it was purely titular, wasn't it?

Emil J Freireich, MD
0:04:18.7
What I said was, "Isn't it sufficient that he's president, unqualified? Why do we have to make him a professor, unqualified?" Later in his career, he made our businessman a professor unilaterally. Dr. LeMaistre insisted on it. I forget what our businessman's name was.

Lesley Brunet, MA
0:04:33.9
Gilley?

Emil J Freireich, MD
0:04:36.3
No. That was Clark's businessman. I'll tell you an anecdote. I don't have the exact year, but shortly after he was appointed here, before I was fired by Krakoff, I was invited to be a visiting professor at Southwestern. You go up there for a week, you make rounds with the students, and you teach them oncology and give a couple of seminars. Dr. Seldin, who is the world's greatest educator—he has received the prize as Outstanding Physician from the Association of American Physicians. Everybody thinks he's wonderful. He walks on water. What he does for the visiting professors is he takes them out to his favorite restaurant, the Warsaw Restaurant, and all the division chiefs in the department come to visit with the visiting professor. He sat next to me at this dinner. After we exchanged niceties, he said, "How's Mickey doing?" I said, "Well, he is certainly different from Dr. Clark. He's got this style and panache and eastern sophistication." He said, "Well, let me tell you about Dr. LeMaistre and how he got to Southwestern. In 1959 the Tuberculosis Hospital was given to the University of Texas because it was abandoned. Because we were treating with streptomycin, it eliminated tuberculosis."

Lesley Brunet, MA
0:06:31.6
The TB hospital here in Houston?

Emil J Freireich, MD
0:06:32.4
No, the hospital in Dallas. The dean called Dr. Seldin and said, "We don't know what to do with this hospital, but we can't reject a gift. We can't look the gift horse in the mouth. So you have to use it for your Department of Internal Medicine somehow." Dr. Seldin told me, "I decided that since it was already a pulmonary hospital, I would make it the Division of Pulmonary Medicine. I had to find someone that was willing to go to this broken-down hospital and build up a program in pulmonary medicine. I looked around the country. It was very hard to find anyone to take this on, but I found LeMaistre. He ostensibly knew about infectious disease. He ostensibly knew about pulmonary. So we hired LeMaistre, and he came to run this hospital. Well, by 1965, after 6 years, he was a total failure at making this work. The students were learning nothing. The patients weren't getting good care. I got a call from the dean, and he said, 'Is there anyone in the Department of Medicine who would like to become the dean of

students?'" Now, that's a job that no one wants. It's a clerical job, but usually it's an MD. He said, "I've got just the man for you," and he gave him LeMaistre. This is Dr. Seldin talking, not Freireich. This is before we've ever had any major disputes.

He was dean of students for 1 year when Frank Erwin called the president of Southwestern and said, "I really need someone to be chancellor at the University of Texas who I can run the university with, someone who looks good, speaks well." He was such a catastrophe as dean of students that the president said, "I've got just the man for you."

Lesley Brunet, MA
0:08:34.2
This is the vice chancellor for Health Affairs?

Emil J Freireich, MD
0:08:36.9
That was his first job. He was the vice chancellor for Health Affairs for 2 years, and then he became chancellor in 1968 and remained there until we were fortunate to get him here in 1978. So Don Seldin said, "My predictions for Dr. LeMaistre at MD Anderson are not very good, but I hope he works out, because he certainly hasn't worked out in any job he's had before."

But he did work out. He was a great success here. That's an anecdote just for the record, to tell you a little bit about him, because you have to understand that LeMaistre is a man who has an enormous talent. He's a theatrical genius, but he has no skills.

Lesley Brunet, MA
0:09:23.9
What made him a success here?

Emil J Freireich, MD
0:09:29.7
He was a regal president. The president of MD Anderson, like John Mendelsohn, becomes a figure in the community. He's appointed to all the business advisory boards. They pay them large sums of money to sit there and nod their heads all day long. He got appointed to the President's Commission on Tobacco, and he nodded his head for 10 years. By contacting all these people, he raised lots of money. He built buildings. He made a good image for MD Anderson in the state. He was very good with the legislators. He was very good at what he did. He was very good at acting, but president of MD Anderson, he was not. Someone else had to do that job. He was the outside guy. But he was very good at what he did. There was no one better than Dr. LeMaistre. If you meet him, you'll realize immediately. With those credentials, with no academic achievements at all, he became chancellor of the third largest university in this country.

Now I want to tell you about 3 important events that will help you understand some of the conflicts that occurred, because it's important to the function of MD Anderson in the international and national program to control cancer.

Chapter 24
Controversies over Use of Drugs in Clinical Trials (1980s)
A: Overview;

Codes
A: The Researcher;
B: Research;
C: Leadership; D: On Leadership;
C: Professional Practice; C: The Professional at Work;
B: Obstacles, Challenges;
B: Institutional Politics;
B: Controversy;
C: Understanding the Institution;
A: Overview;
A: Definitions, Explanations, Translations;
C: Patients; C: Patients, Treatment, Survivors; D: Ethics;
C: Cancer and Disease;
C: Discovery and Success;
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: On Texas and Texans;

Emil J Freireich, MD
0:09:29.7+
The big event was what we call the "Loo caper," on June 11, 1981. I have every piece of paper involved in this whole episode.

Lesley Brunet, MA
0:11:21.8
Is this Ti Li Loo?

Emil J Freireich, MD
0:11:23.5
Ti Li Loo. Dr. Loo is still alive. He's 88 or 89. He's mentally compos. He lives in Washington and is someone that you might want to interview.

Lesley Brunet, MA
0:11:33.9
Oh, okay. For some reason I thought he was dead.

Emil J Freireich, MD
0:11:36.3
He's not dead, far from it. We had a contract with the National Cancer Institute to do pharmacology, and Dr. Loo was the principal investigator. The way this contract operated is that the Cancer Institute developed drugs, and before they went into the clinic, they requested that pharmacology studies be done. We did that on contracts; sent the data back to the project officer. It was a very satisfactory arrangement, and it helped enormously in developing drugs.

We had been given by contract a drug called tetrahydrohomofolic acid to study pharmacologically. The drug was manufactured by a contract with some company to make a radioisotope-labeled drug, and the drug was studied in the clinic in tracer doses to evaluate its pharmacology. This is a radiolabeled drug given to patients to study its disposition, metabolism, half-life, and excretion. The study was conducted professionally, as we do on our contract, and an abstract was written at the AACR. After the abstract book was published, we received a call from the project officer at NCI. He said, "We just realized that this drug did not have an IND filed with the FDA. You have violated the law." Whoa.

Lesley Brunet, MA
0:13:26.4
Is that your responsibility?

Emil J Freireich, MD
0:13:29.4
Of course not.

Lesley Brunet, MA
0:13:30.0
Shouldn't it have been done by the NCI?

Emil J Freireich, MD
0:13:30.7
Of course. That's the point. So the NCI made a mistake. Who's going to get blamed? Well, if you're a bureaucrat, you know how to do that. "Well, obviously, it's their fault." Dr. LeMaistre got a call from Dr. DeVita. "This is a horrible thing. The government is embarrassed that drugs have been given without prior approval by the FDA. Everybody's going to go to jail." Dr. LeMaistre called me. He said, "Freireich, what shall we do?" I said, "Well, we should be proactive. We don't want to just sit around and defend ourselves. We are going to have a thorough investigation of the matter, document it, and send that all to the NCI, because I'm certain it's all done properly." He appointed Bill Fields

Lesley Brunet, MA
0:14:19.0
Yes, I know Dr. Fields. He's still alive. I don't think he's in very good health.

Emil J Freireich, MD
0:14:31.4
Not too compos. Yes, not in good health. He calls me every once in a while. All the old-timers call me because I'm the "old Gomer" here. Bill Fields chaired a committee. It consisted of people who were from various departments— basic science, clinical—and we had an extensive review of the entire thing. We created all the documents. "This is the letter from the NIH. This is the thing. Here's the study. Here's the data."

Now, the first thing that's obvious is there was no harm to anybody because the patients were terminal; they were far-advanced cancer patients. They agreed to do pharmacology studies. They recognized there was no therapeutic intent.

Lesley Brunet, MA
0:15:11.1
They agreed even though there was no therapeutic intent?

Emil J Freireich, MD
0:15:14.5
Of course.

Lesley Brunet, MA
0:15:16.3
Why do you say "of course"?

Emil J Freireich, MD
0:15:18.7
Well, because we're going to treat them. They come here for treatment, and before the treatment, we ask them if they would participate in a research study of pharmacology, and they are more than happy to cooperate with their doctors. We do the 2-day study, and then they get their treatment. They either get better or they don't. It turned out none of them did get better. They all did very badly, but none of them were harmed by this treatment, that's for sure. When the drug was eventually studied 10 years later, the dose we gave them was a thirtieth of the maximum dose. It was just a tracer dose.

It was obvious that the whole thing was a tempest in a teapot. We did this investigation, documented everything, and sent it to the NCI. The NCI responded by saying they wanted to have their own audit, so they appointed a committee of their people. One of them was a really ugly guy, whose name I forgot, who chaired it. They had a group of 10 people from FDA and NCI and all kinds of people. They came here, and they went through the same exercise again. We provided all the same data again.

What followed was the NCI decided, of course, that it was our fault, that we should not have done it without knowing that the FDA had approved it. In other words, when we got the

directions to do it, we were supposed to have checked that there was an IND. Since we didn't do that, Dr. Loo was disenfranchised, the grant was terminated, and funds were frozen. Dr. Loo was prevented from writing any grants for the next 4 or 5 years. There were 2 other physicians involved in the study. They were suspended from any research, and this was a terrible crime.

When we got that report, I was very upset about it, because Dr. DeVita was one of my students. I made him famous. He was director of the NCI, because of the work he did when we were there with MOPP, and I knew him very well. He's a very intelligent, competent guy. I said, "Dr. LeMaistre, I'm going to Washington. I want to straighten this out." So I went to his office, and I sat and talked to him like I'm talking to you. "Vince, why are you doing this? You know Dr. Loo is totally innocent. There's nothing good that can come of this kind of activity."

I'll never forget it, because my relationship with DeVita now changed entirely. He got up, irritated. When you hit people with the truth, they can't handle it. Dr. LeMaistre is the coolest guy in the world, but he'd been furious at me at least 3 times. I mean furious, totally out of control. DeVita got up, pranced up and down his office, and made a speech: "J, you know I support clinical research. You know I've learned from you. I'm one of your students. I think you're the greatest scientist in the world. I wouldn't do anything to harm research." What was the bottom line? Here's the bottom line. "I had to do it." He had to do it to protect his position. This is killing hostages. He had to sacrifice Dr. Loo, someone he knew was innocent, because if he didn't, Congress was going to execute him as director; and they would have. What followed was a Congressional hearing. I had to go. Dr. Loo went. They made a big whoop-de-do about how terrible NCI is in controlling their studies. They gave this drug to people without IND. It was a big whoop-de-do. There were headlines in the papers for weeks. It was a horrible event.

Lesley Brunet, MA
0:19:33.1
Was it blown out of proportion?

Emil J Freireich, MD
0:19:34.7
Of course.

Lesley Brunet, MA
0:19:35.8
It's not like recent events.

Emil J Freireich, MD
0:19:37.0
It was totally silly. You know how politicians do.

Lesley Brunet, MA
0:19:37.7
They had a Congressional hearing?

Emil J Freireich, MD
0:19:41.3
Yes, Congress had a hearing. I went down there and testified. So that was the Loo caper, and the reason that was important is because it established something under LeMaistre that never occurred under Dr. Clark. You see, Dr. Clark was from Texas, and Texans don't let the federal government run the state of Texas. That's what's great about Texas. So if the NCI gives us money, we husband the money. If they don't like the way we do it, all they can do is not give us the money. But just try that.

So Dr. Clark always defended us when we did what we thought was right, but Dr. LeMaistre wasn't there. During this whole episode, it was clear that DeVita established that his power as NCI director meant that he made policy for MD Anderson Cancer Center. I told you about the episode with the chancellor of the university—Dr. LeMaistre in the room, the chancellor, the vice chancellor; Freireich says things are bad. At that meeting, Dr. LeMaistre said we were threatened with withdrawal of all federal funds. Dr. DeVita said, "If you don't rein in Freireich so he does what I say, we're going to withdraw federal funds." And Dr. LeMaistre, of course, caved.

When Mickey said that in the chancellor's office, I looked at the chancellor, and I said, "Dr. Mark, NCI cut off all funds to MD Anderson, the biggest and best cancer center in the world? Who are you kidding? DeVita couldn't do that." But he could threaten, and he succeeded. That's the reason that DT was eliminated.

I told you about the Krakoff hiring. I haven't found the document, but I have a copy of the letter of reference to Dr. LeMaistre supporting Krakoff, and it was signed by DeVita as NCI director. DeVita chose him to come in here. You see, he threatened to eliminate federal funding if they didn't contain Freireich. Now he had to figure out how to get rid of Freireich. We had a number of grants that were terminated on review because we were getting too powerful and too famous in the world, and he picked Krakoff as the hatchet man. He was the perfect pick. DeVita's a genius. He really is a genius, but he's a despot. He's brilliant, but not intellectual. He's motivated, but motivated to power.

By putting Krakoff in here and eliminating DT, his problem was eliminated. We were constantly confronting the FDA on the INDs. We were doing new drugs all the time, and they were delaying us, and we were in the press. We were constantly pressing NCI for more money to press for cancer research. He had to get rid of MD Anderson, and he did, very effectively.

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Chapter 25
Multi-Disciplinary Clinics in a Politicized Environment and a Review of Key MD Anderson People
B: Building the Institution;

Codes
C: Leadership; D: On Leadership;
C: Portraits;
C: Professional Practice;
B: Growth and/or Change;
B: Obstacles, Challenges;
B: Institutional Politics;
B: Controversy;
C: Understanding the Institution;
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;

Emil J Freireich, MD
0:19:41.3+
I told you the reason DT was eliminated. We were Balkanized. I was head of Hematology. Hematology boomed for 2½ years. We were the best in the place. During that time, between '83 and '85, Mickey got a brilliant idea. There was another physician here, Cliff Mountain. Cliff Mountain is somewhere in California with a gold chain and a young wife. He's gone into that phase of his life. Cliff Mountain was a thoracic surgeon who had a national reputation because he worked with the cooperative groups and developed staging systems for lung cancer. He had a big reputation. He got a big grant. He went to LeMaistre, and he said, "The problem with our thoracic program here is it's not integrated." Dr. LeMaistre agreed. So Dr. Mountain created the first multidisciplinary clinic at MD Anderson.

It was thoracic oncology, and the internist came from DT. It was Dr. Manuel Valdivieso, who's now at Southwestern as head of Oncology. The surgeon was Cliff Mountain. The radiologist was probably Sid Wallace [oral history interview] or someone like that. The radiotherapist, I can't recall who it was. But they created a multidisciplinary clinic for lung cancer. Oh, they had an epidemiologist. They had a statistician. They got a big grant.

I really liked that. So on June 10, 1985, I wrote a letter to Dr. LeMaistre. Now, you can tell right away that I'm stupid, because here you are in 1985. He's been here 7 years. I know what's between the ears now. It's not like I'm dealing with a rookie. Here's stupid Freireich, writing a 2-page memo to Dr. LeMaistre. Now, that was the dumbest thing I ever did.

What the memo says is, "I like multidisciplinary clinics, and I think that since you've created one in thoracic, the second one should be in leukemia. I'm head of the Hematology Department, and we have multidisciplinary programs. We have surgeons involved in the pathology staging. We have the lymphoma people involved in the pathology review. We had a lady in Surgery, who is working on immunotherapy. We had radiotherapists involved in lymphoma treatment. We should have a multidisciplinary program in hematology, because that is the biggest program at MD Anderson, dollar-wise, if you put it all together."

Lesley Brunet, MA
0:26:52.3
What did he say?

Emil J Freireich, MD
0:26:52.8
Well, you know what he said.

Lesley Brunet, MA
0:26:54.8
I assume he said no. I don't remember seeing this.

Emil J Freireich, MD
0:27:01.8
Well, what's the date?

Lesley Brunet, MA
0:27:02.6
This is June 7, 1985.

Emil J Freireich, MD
0:27:05.5
On June 14, Dr. Krakoff called me, I came to his office, and he said, "You're fired."

Lesley Brunet, MA
0:27:13.2
You're fired from your program, or you're fired from the whole place?

Emil J Freireich, MD
0:27:16.7
Head of the Department of Hematology and replaced by one of my students. It took me about a year to figure out that what happened was—and I'm certain of it—Mickey looked at this. It's 2 pages and has a lot of words. He can read, but only a limited amount of reading, and he can understand things, but only limited, so he had no idea what this memo was about. So he told his

girl, Judy Johnson, “Show this to Krakoff and ask him what he thinks.” Krakoff took one look at this, and he said, “Insubordinate.” He called me to his office and fired me.

Lesley Brunet, MA
0:27:49.9
Because you wrote to LeMaistre directly?

Emil J Freireich, MD
0:27:52.0
Correct. I went around him. This was not a Medicine proposal. This was multidisciplinary. It had no business being in Krakoff’s hands. But it made him angry, and that’s why he fired me.

Lesley Brunet, MA
0:28:02.8
I’d like to read the whole thing.

Emil J Freireich, MD
0:28:05.6
It’s a good memo, but not for LeMaistre. He never spoke about it. After I was fired, and I went through the entire thing with the dean, he never mentioned this, ever. He had no perception of it. I don’t think he ever read it. I swear he never read it.

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Now, there’s another thing I think you need to understand. I told you about this prescription business. The prescription business is quite important. Do you have the date that Fred Conrad was shot?

Lesley Brunet, MA
0:28:52.6
I think was December.

Emil J Freireich, MD
0:28:55.8
It was December 17, 1982, and in ’83 I got fired, DT was eliminated, Krakoff came in, and all that stuff. The prescription thing is very important, because when it occurred, LeMaistre acted in his usual way. I told you I had my heart attack. The heart attack was clearly over the prescription thing. I was just insanely angry. Believe it or not, when I was in the hospital, almost dead, he called a meeting of Dr. Kenneth McCredie and Dr. Michael Keating.

Lesley Brunet, MA
0:00:04.6
March 10, 1987?

Emil J Freireich, MD
0:00:09.0

Yes. My heart attack occurred March 1. I had a heart attack with 80 percent mortality in the absence of tissue plasminogen activator, which I got a shot of, fortunately, because I was in the trial at Baylor. I recovered, and I had a bypass, so I'm postop. I had my surgery on the third, so it's one week after an open-heart surgery. Dr. LeMaistre resolves the issue that he and I had confronted for months, and the memo says, "All taken care of." It shows you the character of the person. He had no concern about me. He was just hoping I would die. I'm sure of it. But anyhow, even if I didn't die, he wanted to get the issue settled while I was gone. So he settled it with a 3-page memo: "All settled." When I recovered, after about a week, I came back. I immediately responded with a memo that said, "Fine. You decided it's settled, but I'm not going to agree to it because physicians' privileges were suspended. Again, you knuckled under to the Cancer Institute, which is unacceptable. There was no crime committed, and this has to be abolished from our records," which he did. It was put into the personnel files that their suspensions were as physicians. That was eliminated. Dr. Ainsworth sent me a memo, and he said, "We've taken it out of their personnel files. No privileges were suspended. Nothing of the sort." But he never had the guts to write to NCI to say the mistake was not in DT, the mistake was in Krakoff's office. He never had the guts to do that. The thing ended with my memo saying that, "The issue is not closed until you close the loop and tell the NCI we're innocent." Here are the 7 patients who got the AMSA. These were the controls. They certainly weren't harmed. These 7 people caused this whole whoop-de-do.

I have a good friend named Robert Mosbacher. [Redacted] I called him up, and I said, "You know, I'm really upset about this. I think Mickey is hurting MD Anderson. What should I do?" He said, "Well, I have a good friend named Jack Trotter." He was either on the Board of Regents, or he knew everybody on the regents. "Why don't you go over it with him?"

I developed a psychiatric dialogue with Jack Trotter. Jack Trotter owned some oil companies. He had a fancy office on top of a building downtown. I think I would have died of my heart attack if I had continued the tension I was under. But I went to Jack Trotter, and he's a lovely guy. For no reason, just friendship, he set aside a half hour to an hour. He'd talk to me, and I'd tell him what I thought, and he wouldn't do anything. Then a week would go by, and I'd call him again and say, "There's something new," and he'd say, "Fine. Come by." I'd tell him what was going on. But during one of our Mickey conversations, he became insanely angry, because he said I was contacting politicians, which was true.

Lesley Brunet, MA
0:04:22.7
Trotter became angry?

Emil J Freireich, MD
0:04:24.1
No.

Lesley Brunet, MA
0:04:25.6

LeMaistre became angry?

Emil J Freireich, MD
0:04:26.1
Yes, LeMaistre. I told you what Fred Becker said. Dr. LeMaistre is like a government employee. All he cared about was his position. He loved power. He loved money. He loved prestige. Everybody does. Everyone loves power, prestige, and money. Have you seen the *Wizard of Oz*?

Lesley Brunet, MA
0:04:39.9
Sure.

Emil J Freireich, MD
0:04:41.4
The rock musical *Wizard of Oz*?

Lesley Brunet, MA
0:04:42.7
Oh, no.

Emil J Freireich, MD
0:04:43.9
Well, the rock musical *Wizard of Oz*, when the Wiz comes out and he's revealed as a fake, they say, "Why would you do this horrible thing?" He says, "I'm like everybody else." Power, fame, and money—Mickey loves those things, so he had to protect them. I don't blame him. If I loved those things, that's what I would protect, too. The things that made him insanely angry were any politicians, because his base was political. I was talking to Jack Trotter, and Jack Trotter, obviously, talked to regents, and regents asked questions. LeMaistre had to answer them, and he always had the appropriate answers. Of course, I lost, but I just wanted you to know that that happened.

The people who built DT were Dr. Emil "Tom" Frei III, who's alive; he's got Parkinson's, but his brain's okay. He was here '65 to '72. He knew Dr. Clark probably better than I did, because he was closer to Clark than I. I was his deputy. After '72, I dealt with Clark largely through Hickey, and I became close to Clark after he got fired in '79 or '80. Dr. Frei and I, when we came here in '65, we hired Dr. Loo, so he was here in '65. He retired in'94? So he was here 29 years. That's the person who really did a lot for this place.

Lesley Brunet, MA
0:07:09.8
Oh, yes, he did.

Emil J Freireich, MD

0:07:12.6
Dr. David Farquhar, who's still here, was his postdoc. Dah Hsi Ho was recruited from Roswell Park, and she's still here. She retired, but she still comes to work every day, without cost, and she's right down the hall here. Grady Saunders is also retired. He was recruited in 1966. He's still very compos. He still has a grant, and he works, not every day, but pretty close to every day. He came in '66 or '67. His wife, Priscilla Saunders, was a member of our department. She's still alive and well. She was fired by Krakoff when he hired Bob Newman. I personally recruited Bill Plunkett [oral history interview] as a post doc, and he's still here. I took him to a hockey game, as I recall. That's Pharmacology.

The first person we recruited was Gerald Bodey, Sr [oral history interview]. Dr. Bodey is very alive and very compos. He retired maybe 4 or 5 years ago and lives in The Woodlands. He's now coming back part-time to help us out. Dr. Bodey is an enormously intelligent, distinguished, accomplished person. He's world famous for his work on infectious diseases. He was the first director of the Office of Protocol Research. When we had the festouche over the Loo caper, he was given the responsibility of running the review protocols. He was head of our Chemotherapy. He's a very good man.

Evan Hersh was here from '66 until, again, the Krakoff business. Before Krakoff came, I had gone to LeMaistre. He wanted the departments to come together and all that bull, and I convinced him that Hersh had built up a program that was so large and so effective that it deserved departmental status. We had an external review of the pharmacology program, and I was convinced we needed a pharmacology department, separate from DT. I convinced him that we needed a chemotherapy department separate from DT.

So when DT was Balkanized, Hersh became the department chairman. In fact, in 1981 Hersh was made a head of a department. Dr. Hersh is, again, enormously intelligent, accomplished, intellectual, just a fine, fine person. Hersh is now head of Oncology at the University of Arizona in Tucson. He comes back regularly, about once a year, for a Gottlieb program. His heart's at MD Anderson. He came here as a rookie and built up a tremendous program. He recruited Jordan Gutterman [oral history interview], did all the interferon work. He recruited Giora Mavligit, who is still here, working in the GI department. He recruited Yehuda Patt, who is still here, working on liver cancer. He trained a lot of very famous tumor immunology people, including Jules Harris, who's at Rush Medical College. So when DT was at its peak, my executive committee was Hersh, Bodey, Loo, and I. We met and made all of the personnel decisions. I kept rosters like this so people were treated fairly.

Ken McCredie came here in 1969, and he died in 1991. McCredie was very, very important in our program. McCredie had a professorship at the University of Sydney. He's from New Zealand and trained in Australia. I went to Australia to talk about curing leukemia, and he liked it. He's a person who goes for what he believes he should be doing. It's not power, fame, and glory. He likes to get things done. He's a person of action. He called me up, and he said, "You know, I want to work with you because I like the way you think." I said, "Fine." I had a rule that no one became a professor in DT unless they did training, because they had to learn how I

did things. I said, "You have to become a fellow." He said, "Fine." For $12,000 a year, he came over from Australia with his 3 daughters and his wife. He came as a fellow for 1 year, and then in '70 was appointed assistant professor at $18,000. He did really well. In '83 he had $100,000. He was an enormously accomplished person. He died, of course, tragically. It was reported to be suicide. [redacted]

Emil J Freireich, MD
0:13:19.5
Yes. He was here working. He was on our faculty. He was fantastically effective. But McCredie was very big. One of the reasons he was big is because he recruited Michael Keating. Michael Keating was an Australian. He was born in Australia and trained in Melbourne. Ken McCredie had returned to Australia several times to give talks. Keating came here as a fellow in 1974. It's now '01, so he's been here 27 years. He came primarily to work with McCredie. He had a faculty appointment at Melbourne, and we went through the same thing.

I said, "You can't become staff unless you're a fellow," so he came as a fellow for $15,000 in 1974. Michael Keating picked up his 2 sons, 2 daughters, and wife, and came to the United States. It takes tough people to do that kind of thing. He rapidly rose to professor. In '77, after he'd been here 3 years, they offered him a staff job in Australia, so he went back to Australia. He stayed there for exactly 1 year, and he came back because he said he couldn't do what he did with DT.

Michael Keating is the world's greatest physician-scientist. He is infinitely smarter than I am. He's a terrific person. He's developed the treatment for chronic lymphocytic leukemia. He is an absolute superstar. There is no one better than Michael Keating. So from '74 on, Michael Keating is someone that should be on your list. He directed the Office of Protocol Research for a while. When James Cox [oral history interview] came in as vice president for Patient Affairs, Michael Keating was assistant. Incidentally, Cox is someone you definitely ought to have on your list, because LeMaistre hired Cox to be physician-in-chief and then fired him. Cox will have a lot to tell you about Mickey. Keating has played a big role in this institution. Again, he came primarily as a DT guy.

Lesley Brunet, MA
0:16:43.2
Where is he now?

Emil J Freireich, MD
0:16:44.6
He's in Leukemia.

Lesley Brunet, MA
0:16:45.7
He's still there?

Emil J Freireich, MD
0:16:47.0
He's still in the Department of Leukemia, where he's been for his whole career. He's still working very actively, and he's a dynamo and an absolutely wonderful person.

I mentioned Jordan Gutterman. He came in '71. He's still here. I mentioned Dr. Mavligit. He came in '71. I mentioned Dr. Valdivieso, who came in '72. He was the first thoracic oncology multidisciplinary guy. Karel Dicke is still alive. I don't know if you want to talk to him. Raymond Alexanian you know about. Bob Benjamin came here in 1974, so he's been here 27 years, also. Bob came here after training at the Cancer Institute in Washington. He came to work primarily with Jeff Gottlieb, who died of cancer. Dr. Benjamin was a clinical pharmacologist, and he's been a very important factor in the growth of MD Anderson. He's now chairman of the sarcoma department, and he's going to give grand rounds on Friday. He's a very, very good man. A peculiar person, but someone who's interesting, is Dr. Michael Burgess. Dr. Burgess came here in 1970, also from Australia. He knew McCredie and Keating.

Lesley Brunet, MA
0:18:25.2
Why do you say he's peculiar?

Emil J Freireich, MD
0:18:29.3
Well, because he took a sabbatical. He sailed around the world for 3 or 4 years. He became a sail bum. Then he came back, and he works in Sarcoma with Benjamin. But he was very important in DT. He ran the outpatient clinic, and the fellows just adored him. He was a wonderful professor. He's still here, working at MD Anderson.

Jeane Hester came in 1971. I mentioned her before. She's still alive and in the community. She's a tremendous person. She came to us from University of Oklahoma, just having finished her residency. She did her fellowship here. She's the one who developed the blood cell separator, almost single-handedly, to where it is today. She also got fired.

There's an episode we haven't covered, which is the hiring of Albert Deisseroth. When I got fired as head of Hematology, they had this search committee, and they hired Albert Deisseroth. Deisseroth came in '86, and he made a mess of the Department of Hematology. We can get into that. That's more modern history. But Deisseroth is still alive. He just took a job in San Diego as the director of the Sidney Kimmel Cancer Center. He left here to go to Yale to work with DeVita, and then he went to San Diego. He was very bad for DT, for the leukemia program, but that's what his job was. He was hired to replace me.

Bill Murphy is still alive. He came in '72. He worked in thoracic oncology for many years. He retired. He was one of the first DT people to retire. I went to the retirement party, and I said to my wife, "It's a bad thing when your students retire." He came here from San Antonio Medical School and did his entire training here. He's a very good guy.

Michael Keating I mentioned. Bart Barlogie I mentioned. He came here in ’74. He came out of just house-staff training in Germany. He’s a very colorful figure. He built the first flow machine in the United States. He’s a fantastically energetic investigator. He was the one that Krakoff convinced to replace me in ’85. He and Deisseroth, of course, had nothing in common, so he left to go to Arkansas, where he’s established the country’s best program in myeloma. He’s alive and very compos. He was born in ’44, so he’s only 57 years old, I guess.

Fernando Cabanillas came in 1974 from Puerto Rico as a fellow. After his training, he advanced to where he is now. He’s chairman of the Lymphoma Department. As I told you, when I had my first confrontation with Krakoff over the Hematology Department in 1983, I insisted Cabanillas be head of Lymphoma instead of Alexanian. He’s internationally famous in the lymphoma field. He’s very competent, very intelligent, and well worth talking to.

Yehuda Patt came in 1975. He still runs the liver program in GI. Axel Zander came in ’77. He’s in Switzerland. Sewa Legha came in ’76. He’s in practice at Saint Luke’s. He got fired by the Board of Regents over a trivial issue. He’s still very important in the world of melanoma. He authored some very innovative stuff. He’s someone you can talk to. He’s very intelligent. He’s across the street.

Dr. Miloslav Beran came to us in 1983. He’s still here in the Department of Leukemia. He is a staff member. Lynn Feun is in Florida. We’re getting down to the more modern ones that probably are less interesting. Gabriel Lopez-Bernstein is a big power around here. He came to DT in ’79. He’s one of our fellows. John Kavanagh was in Medicine. Dr. Adan Rios, who’s going to get the Alumnus of the Year Award, came in ’81. Ron Walters, who runs our thing, was a DT fellow in ’79. He runs the outreach program for MD Anderson. Hagop Kantarjian, of course, is a superstar. He’s chairman of the Leukemia Department. He’s internationally famous. He came as a DT fellow in 1981. I mentioned Ho and Potu Rao, Farquhar, Priscilla Saunders, Bill Plunkett. Michael Rosenblum, who’s still here, came in ’79. Jim Reuben came in ’79.

Lesley Brunet, MA
0:23:55.3
That’s quite a list of physician-scientists that were here.

Emil J Freireich, MD
0:24:05.0
The reason I went over this list is because these people are still compos, they are still working, and they are accomplished people. They’re people who, after I die, you can find out more from.

Chapter 26
Reflections on Leadership Style, Intellectual Freedom, and MD Anderson
A: Overview;

Codes
A: Character, Values, Beliefs, Talents;
A: Professional Values, Ethics, Purpose;
A: Critical Perspectives;
C: Critical Perspectives;
A: Personal Background;
C: Leadership; D: On Leadership;
C: Portraits;
C: Professional Practice; C: The Professional at Work;
B: Obstacles, Challenges;
B: Institutional Politics;
B: Controversy;
C: Understanding the Institution;
B: MD Anderson Culture;
B: Working Environment;
B: Institutional Mission and Values;
A: The Researcher;
B: Critical Perspectives on MD Anderson;
B: MD Anderson History; B: MD Anderson Snapshot;
D: On Texas and Texans;

Emil J Freireich, MD
0:24:05.0+
I love MD Anderson. Dr. LeMaistre loved LeMaistre. That's not to be critical. People have different goals in life. Dr. Clark would have died for MD Anderson. This was his creation. It's what he cared about. Dr. Mendelsohn cares about science. He's got LeMaistre's qualities. He's a LeMaistre figure all the way.

Lesley Brunet, MA
0:25:16.1
There is that resemblance. Is it just they're tall and white-haired?

Emil J Freireich, MD
0:25:20.6
The regents like people who look presidential. Mendelsohn has that air, and his wife is just absolutely charming. But he cares about cancer. He cares about science. He cares about research; he really does. LeMaistre didn't care a hoot about medicine or science or anything.

Lesley Brunet, MA
0:25:44.1
One thing we haven't talked about is LeMaistre's departure. Was it his choice?

Emil J Freireich, MD
0:25:59.0
Fortunately, I wasn't involved in that. He told the public that it was because of his wife's illness. She had allergies, and he was recommended to take her to the drier climates of Colorado. That's what he says. I don't believe that for a minute.

Lesley Brunet, MA
0:26:23.9
He had been in office a long time. Some would say it was time.

Emil J Freireich, MD
0:26:30.6
He was here 18 years, and he was old. When was he born? Do you know?

Lesley Brunet, MA
0:26:37.5
No, I don't know offhand.

Emil J Freireich, MD
0:26:39.1
I'd say '24.

Lesley Brunet, MA
0:26:44.1
By '95 he'd be about 75.

Emil J Freireich, MD
0:26:48.3
Yes. He was 74, for sure, maybe 75. The regents don't like presidents over the age of 70. They didn't like Dr. Clark at that age. They like young, vigorous leadership. He was still doing very well, but I think he was asked to leave. There was a change in the university administration, and Roger Bulger had left. They had a new president over here, and I just think he was asked to leave. But he had accumulated an enormous fortune as president of this institution. He's really bled the citizens of this state. He continues on some of these advisory boards. I think he's been fired from most of them and replaced by Mendelsohn. But I'm sure he was asked to leave.

I was involved in the search for the new president, because I was chairman of the faculty senate at one point, and we had a subcommittee in the senate that advised the regents on his

replacement. We interviewed Mendelsohn, and we interviewed other candidates. We had some input, but I don't think it was significant. It was trivial. It was already decided, I'm sure.

Lesley Brunet, MA
0:28:46.4
You were also head of the executive committee for a while.

Emil J Freireich, MD
0:28:48.0
Oh, I've been head of everything. I was chairman of the Research Committee and chairman of the Safety Committee.

Lesley Brunet, MA
0:28:53.4
It's sort of surprising, because you sometimes seem to be in conflict with people, but you have to be elected, right?

Emil J Freireich, MD
0:28:57.7
Yes. I did an interview for this journal, *Lancet Oncology.*

Lesley Brunet, MA
0:29:15.7
Oh, it just came out.

Emil J Freireich, MD
0:29:16.1
Yes, while I was on vacation. I always start with my background, as I did with you. If you have to fight your way out of a pile of crap, you learn to be energetic and goal oriented. I've always been that. I've always wanted to get done what needed to be done, politics aside, and that's costly.

When Dr. Clark was here, everybody knew that Clark liked what I was doing. We were building. We were making money. We were adding buildings. We were doing research. We were getting famous. Before DT, we were not a scientific, clinical institution. We put MD Anderson on the map, our little gang of warriors. But once we were on the map and accomplished, we outlived our usefulness, and we got cast aside, so that's appropriate. The renewal of the species keeps youth and vigor.

Lesley Brunet, MA
0:30:51.9
How does that explain why you were elected?

Emil J Freireich, MD

0:30:54.4
Because everybody knew that I would do a good job, and I was aggressive about seeking it. I wanted to be head of the executive committee. There were only 7 or 8 department chairmen, and you can only do it for 2 years, so you eventually came up. I've been chair of everything—Research Committee, Executive Committee, Promotions/Tenures. I've done everything here, just because I was energetic and people knew they could rely on me.

Lesley Brunet, MA
0:00:02.1
You say you were never insubordinate. How do they define "insubordinate"?

Emil J Freireich, MD
0:00:04.8
I have never been insubordinate. Insubordinate is that you don't do what you're told and you don't follow proper procedures. I have always followed the rules. I have always been a team player. I always did what I thought was good for MD Anderson, personalities aside, and that's my goal in life.

You see, I'm very taken with Texas. I've always worked for the government. I worked for the federal government, and I worked for state government, but I hate bureaucracy. I'm a very right-wing conservative. I believe in freedom. I believe in freedom of thought. I believe in freedom of action. I believe in decentralization. I think anyone who thinks as DeVita did, that he can control cancer research for 250 million people, is crazy. Those people have to be executed. No matter how brilliant you are, there are more brilliant people around. You have to give people opportunity and motivation. So I believe in freedom. I'm a very strong believer in that. Whenever one imposes on my freedom to think—intellectual freedom, academic freedom, social freedom—I'll get violent.

All the things I told you about that appear to be insubordinate were all done by the rules. I didn't go around Dr. LeMaistre, and I didn't go around Dr. Krakoff. Dr. Krakoff thought I did, but I did it perfectly properly. It was a multidisciplinary clinic, and I went to LeMaistre. LeMaistre initiated the program in thoracic oncology. The prescription thing we did absolutely perfectly. We defended ourselves perfectly. There was no need to kill hostages. That's not honorable. So everything that I've told you, in my opinion, was done by the book.

I went to LeMaistre when I said I wanted to go to the regents. That was recommended to me. I went to Mr. Trotter in good faith. He agreed to try to help me improve MD Anderson, not me. When I talked to the regents, when I talked to the chancellor, and when I talked to Trotter, I said, "Look, don't worry about Freireich. I'm trivial."

This is the greatest cancer center in the world. You've got to care about MD Anderson. If you care about MD Anderson, if you want to cure cancer, this place has to be a leader. MD Anderson is a symbol of intellectual freedom.

That's the trouble with Washington. That's the trouble with liberals. They're megalomaniacs. Liberals know more than any conservative. You're a liberal. You know how that goes. We know how to do healthcare. Hillary Clinton knows how to deliver healthcare better than 50,000 people who've worked in healthcare their entire lives. Some idiot lawyer comes out of law school, and she knows how to do healthcare. She doesn't know anything about healthcare, and she doesn't care. She's just got this arrogance of the liberal.

The liberal arrogance believes that once you figure out what makes reason, then everybody else should follow your reason. I believe in intellectual freedom. What comes out of a productive society depends on taking advantage of the resources you have, the people. So the national cancer program consists of 30 to 50 cancer centers, each independent, following their own train of thought, not organized into some atom bomb project, but doing what they think they ought to do.

MD Anderson has a unique opportunity in Texas. Roswell Park has an opportunity. University of California has an opportunity. They're all different. They all have different leaders. They all have Freireichs. They all have Hershes. They all have Bodeys. They all have Gradys. They all have Keatings. They all have people who want to cure cancer. Give them a chance. Give them freedom. Get the FDA off their back. Get the NCI off their back. Provide plenty of resources.

The federal government wants to stimulate research. They don't know how to do it, so they give it to NCI. That's the only way they know how to do it. What NCI should do is what Bush did. They should send the money to Texas and let them decide what to do with it. Texas should just give it to the university, and they decide what to do with it. The university should give it to the institute, so they decide. The people making decisions have to be the people closest to the problem because they're the ones who know what's going on. People who know how to do healthcare are doctors and nurses and patients, not bureaucrats.

I've always been a team player. I've always been proper. I never cheat, I never steal, and I don't lie. I always do what's good for MD Anderson, which is what I believe is good for the cancer effort in the United States and what I believe is good for the cancer effort in the world. But I work hard at it.

Lesley Brunet, MA
0:05:36.3
Yes, I know you do.

Emil J Freireich, MD
0:05:41.0
When I was tense, I used to keep these elaborate calendars, so everything is documented with memos and notes.

Lesley Brunet, MA
0:05:49.0

Of events that occurred?

Emil J Freireich, MD
0:05:52.2
Every event.

Lesley Brunet, MA
0:05:52.8
And you've saved all those.

Emil J Freireich, MD
0:05:53.7
On a day-to-day basis. See, "March 16, new Advisory Committee." "March 17, first discussion."

Lesley Brunet, MA
0:06:00.3
You created these at the time. This isn't something you went back and did?

Emil J Freireich, MD
0:06:03.5
No. This is not retrospective. This is live.

Lesley Brunet, MA
0:06:07.3
That's good.

Emil J Freireich, MD
0:06:08.8
Yes, I keep everything. When an event occurs that I think is signal, I write it down. That's what you do if you're a scientist. You record everything, because you can't think about everything while it's going on. I have these elaborate notes, day to day, and I wrote them down every day.

Lesley Brunet, MA
0:07:06.2
That is a great historical record.

Emil J Freireich, MD
0:07:09.5
They are, but not really important.

Lesley Brunet, MA
0:07:12.5
But they are important.

Emil J Freireich, MD
0:07:15.2
I'm a compulsive record keeper. Here's the letter Dr. Loo wrote that's very important. When he retired, he got a job at the Cancer Institute, believe it or not.

Lesley Brunet, MA
0:08:03.7
Yes, I know. I was surprised.

Emil J Freireich, MD
0:08:06.0
He was blackballed. I wrote a very strong letter to Bruce Chabner. Here's the whole document about the methyltetrahydrofolate.

Lesley Brunet, MA
0:08:15.8
He was fired from the head of Pharmacology, but he continued to work here?

Emil J Freireich, MD
0:08:21.0
No, he left. After he got fired, he resigned. This is a good letter. It documents the whole thing I've been telling you about. This was written to Bruce Chabner, who was then deputy head of the Cancer Center, director of the Division of Cancer Treatment. "Worst price to pay is the fact that monies collected and allocated by Congress are being used for review. No potential for contributing to the discovery of anything."

Lesley Brunet, MA
0:09:00.2
Did you ever get discouraged about the bureaucracy?

Emil J Freireich, MD
0:09:03.6
No. You can't be discouraged about bureaucracy. The world is made up of people, and people are distributed normally on the curve of brilliance and stupidity, motivation and no motivation, lethargy and energy and indolence. We've got 30,000 genes. There are all kinds of people.

There is a great majority of people who you can make humor out of it, but they rise to their level of incompetence. You can't tell how a person's going to behave with responsibility until he gets it. You can't tell how you're going to be as a mother until you have a child. You can't tell how you're going to behave as a president until you get the job. Once you get it, then you're in a situation where evaluating your performance creates difficulties for everybody above.

My wife is a great philosopher. She's kept me going all these years. I would've died of a heart attack at the age of 40 if it weren't for her. She always said, "You know, Dr. LeMaistre doesn't care about you. He's trying to do the best job he can do at what he thinks his job is." And that's true of everybody in a bureaucracy. They're all doing what they think their job is.

If you're hired to be a regulator by the FDA, you're going to regulate, and regulation requires that if something is done that is bad, you're at fault. If something is done that is good, you had nothing to do with it. So you immediately become a regulator; you do nothing. That's bureaucracy. A bureaucrat is a person whose main concern is to keep his own job, and that's true of all of us. I want to keep my job right now. Dr. Mendelsohn can fire me tomorrow, and probably will, because I'm not much use anymore.

Lesley Brunet, MA
0:11:13.0
Something we haven't talked about?

Emil J Freireich, MD
0:11:16.5
But I did get the LeMaistre award, so I have Mickey with me forever.

Lesley Brunet, MA
0:11:21.9
That's ironic.

Emil J Freireich, MD
0:11:27.5
He didn't hand it to me. The first 6 or 7, he came personally to hand them out. But when I got mine, he didn't hand it to me. Dr. Mendelsohn did.

Lesley Brunet, MA
0:11:41.3
You had a 15-year anniversary of DT.

Emil J Freireich, MD
0:11:43.5
Yes.

Lesley Brunet, MA
0:11:44.2
It was a big deal.

Emil J Freireich, MD
0:11:45.2
It was a big deal. We did that deliberately.

Lesley Brunet, MA
0:11:47.7
LeMaistre didn't come.

Emil J Freireich, MD
0:11:51.4
No, he didn't come. We did that to demonstrate to the institution how we benefited everybody. In other words, rather than worry about what we were doing, they should benefit from it. So we tried to show how the programs we had initiated benefited the hospital—the pumps, the vessels, nurses, the IV team, the Pheresis Center, and the platelet transfusion. All the things we were doing were good for everybody at MD Anderson. That was the purpose of our 15-year anniversary.

Chapter 27
The Development of the ALZA Infusion Pump
A: The Researcher;

Codes
A: The Researcher;
B: Research;
A: Overview;
A: Definitions, Explanations, Translations;
C: Professional Practice; C: The Professional at Work;
C: Patients; C: Patients, Treatment, Survivors;
D: Ethics;
C: Cancer and Disease;
C: Discovery and Success;
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: On Texas and Texans;
A: Personal Background;

Lesley Brunet, MA
0:12:22.1
Let's talk a little more about the infusion pump. I saw some correspondence about ALZA.

Emil J Freireich, MD
0:12:28.6
Yes. It's a wonderful story. Everybody who gets outpatient chemotherapy now uses the ALZA pump. ALZA stands for Alex Zaffaroni. Alex Zaffaroni was an entrepreneur who discovered some kind of a steroid cream that was good for the skin, and it made a lot of money. So he got this money, and he was one of those guys who didn't like doing nothing. He decided he wanted to do something good for medicine, so he founded a company, and it was named after him. I'm not sure what his overall mission was, but one of the goals of the company was to develop devices for pharmacology. Alex Zaffaroni was a pharmacologist. He said, "Look, if we have medicines like insulin and hormones and things, we don't want this kind of stuff. We ought to have a device that could deliver pharmaceuticals on a continuous basis." So he started this company, and he hired a young engineer, Dick Buckles. Dick Buckles must have been 42 or 43 when he started to work on this problem. If you were going to approach this problem, you'd approach it the way anyone would. You would get some kind of a syringe, you'd develop some kind of a clock motor that would push the syringe, and you'd have an infusion device. It may have been just a year or 2 ago that I threw it out, but I have the first one we did.

Lesley Brunet, MA

0:15:57.7
The first device?

Emil J Freireich, MD
0:15:59.2
Yes. I kept it for many years. I keep little trinkets like that, like my written records. The company was based in California. He went around the country. He started working with people. He went to scientific meetings. He knew DT. I had a reputation by that time. I can't recall the year. He appeared in my office with a young lady who was a nurse, who is still a good friend. We wanted to do this thing. We had a fellow who worked on this, and he's the senior author of the paper, Joe Bottino. He's in practice in New York now. But McCredie was the big mover on this project.

He said he wanted to test this syringe device for continuous infusion. We had been working on ARA-C pharmacology with Bill Plunkett, and we knew that ARA-C worked best if you maintained the level, because if you did this kind of thing, it wasn't as effective. So we were very interested. So we said, "Okay. This is fine. But the first problem is if we want to give a continuous infusion over 10 days, how do you get access to the vein?" He said he had discovered a company in Dallas, I think, that had made catheters that were plastic and could remain permanently implanted for long periods of time, but they couldn't find anyone to study it. Would we do it? Absolutely. This company came out with these catheters, and we began to study them. Initially they were long lines that were put in the antecubital vein and threaded up, and then you took an x-ray. We still do it. The material in the catheters has improved, and the size of the catheters has improved. A lot has improved. The early, primitive ones had lots of problems. We worked with these catheters and then with IVs. Then we learned how to do subclavians. So the catheters seemed to be working reasonably well. They were clotting, and we had some problems. Now we had to evaluate his pump. So we started with these little pumps. You used to strap them on your arm. I even have a picture of me holding one of these on my arm. You strapped it on your arm, and you wore it under your suit coat, and you had a continuous infusion.

Finally, Buckles discovered a guy who had the most brilliant idea. That was the balloon, and that's what we use today. Instead of a mechanical device, what we're going to do is we're going to put in a piece of plastic, a balloon, and we're going to load the balloon with kinetic energy. So you have no energy source. It doesn't require a motor or an engine. It has no heat sensitivity. Everything works. That was the first ALZA pump, which was commercially done. Then it was bought by Abbott Laboratories, and now everybody uses one. You load the pump by blowing up the balloon. Then you hook it up, and the balloon collapses and shoots the stuff in. You get a new balloon every day, and that's how we treat our outpatients with their catheters.
Tragically, before the project ended, Dick Buckles developed lymphoma and died. We treated him for lymphoma. He had an excellent response, but it was short-lived, and he died; he transformed. He was a young guy. He was only 45.

Lesley Brunet, MA

0:20:16.6
What did you do then?

Emil J Freireich, MD
0:20:18.3
By then it was commercial. You'd just buy them. We did it all in DT. The nurses hated us, but eventually they realized they had to do it, and everybody did it.

Lesley Brunet, MA
0:20:36.8
They hated you because it was creating more work or changing the system?

Emil J Freireich, MD
0:20:43.6
It's hard for people to learn new things.

Lesley Brunet, MA
0:20:47.3
Was there ever opposition to development of the whole device and program?

Emil J Freireich, MD
0:20:55.5
Not really. People are always concerned about innovation. There's always the potential for harm. Catheters are going to clot. You get a pulmonary embolism. These people are going to die. You have to flush them with heparin. Sometimes they get occluded. Sometimes they come out and go up here, and they get strokes. So there's always resistance to innovation.

Lesley Brunet, MA
0:21:24.4
But this was a big innovation in terms of the outpatient.

Emil J Freireich, MD
0:21:27.0
It was terrific innovation. It revolutionized care in the country.

Lesley Brunet, MA
0:21:29.6
Didn't outpatient care really boom here?

Emil J Freireich, MD
0:21:37.1
It boomed all over the country. It was Dick Buckles. All we did was the clinical stuff. The gal who worked for ALZA is still doing this as a career. Her name is Suzanne Herbst. She's the president of the Association for Vascular Access. She recently visited me. Her father developed

leukemia, and I helped treat him. He did very well for about 10 years, and then he died of a stroke. So the world is full of challenges.

Lesley Brunet, MA
0:22:12.9
Does it seem like a lot of people associated with MD Anderson also get cancer? Is that just an external kind of view?

Emil J Freireich, MD
0:22:18.2
You have to think of it in probabilities. Cancer is the leading cause of death over the age of 60. So since lifespan is getting longer, the probability that we'll all die of cancer goes up every year.

Lesley Brunet, MA
0:22:34.2
That's a little frightening.

Emil J Freireich, MD
0:22:36.8
Well, I always look at the full and empty part. It's really gratifying that we live long enough to get cancer. We used to all die of cardiovascular disease. There's a 30 percent reduction in cardiovascular mortality every 10 years. Look at me. In '87, I was dead. This is '01, fourteen years later.

Lesley Brunet, MA
0:23:00.8
That's pretty good.

Emil J Freireich, MD
0:23:04.4
I've had open-heart surgery and a bypass and a fake vessel. It's amazing what they can do.

Lesley Brunet, MA
0:23:11.5
I think you came from hearty stock.

Emil J Freireich, MD
0:23:13.1
My mother lived a long time, but my father didn't live very long. My wife is hearty.

Lesley Brunet, MA
0:23:20.2
She keeps you going?

Emil J Freireich, MD
0:23:22.6
Oh, she's hearty. She is a tough thing. She keeps me going.

Lesley Brunet, MA
0:23:26.9
That's good.

Emil J Freireich, MD
0:23:27.8
Yes, I'm very lucky to have her.

Lesley Brunet, MA
0:23:29.0
That's always nice to hear.

Emil J Freireich, MD
0:23:31.2
Yes. I'm square. That's the thing. I have no vices. I have no hobbies. I don't play bridge.

Lesley Brunet, MA
0:23:40.4
You have no hobbies at all?

Emil J Freireich, MD
0:23:42.3
No hobbies at all.

Lesley Brunet, MA
0:23:43.5
Well, you can't retire.

Emil J Freireich, MD
0:23:44.9
I tried. I've been on vacation for 3 days, and when I went to work today Deanie said, "See? You can't even stay home for 3 days." No, I can't retire. Well, I could retire, but it would be difficult for me.

Lesley Brunet, MA
0:24:05.6
No hobbies? No golf?

Emil J Freireich, MD
0:24:06.8

No golf, no nothing. No tennis. No jogging.

Lesley Brunet, MA
0:24:10.3
Your wife wants you to keep going to work, too, I bet.

Emil J Freireich, MD
0:24:13.3
Yes, she loves me out of the house. She gets sick and tired of me. She runs the house, and I run my office. I'm totally square. As I told you, I always follow the rules. I never cheat. I never steal. I'm just one of those unusual people that don't know what to do.

Chapter 28
The Physician-Scientist Training Program and Other Activities and Some Thoughts on Retirement
A: Overview;

Codes
B: Education;
D: On Education; C: Education at MD Anderson;
A: Character, Values, Beliefs, Talents;
A: Professional Values, Ethics, Purpose;
A: Personal Background;
C: Leadership; D: On Leadership;
C: Professional Practice; C: The Professional at Work;
B: Obstacles, Challenges;
B: Institutional Politics;
B: Controversy;
C: Understanding the Institution;
B: MD Anderson Culture;
B: Working Environment;
B: Institutional Mission and Values;
A: The Researcher;
B: Critical Perspectives on MD Anderson;
B: MD Anderson History; B: MD Anderson Snapshot;
D: On Texas and Texans;

Lesley Brunet, MA
0:24:37.9
But your program, the Physician-Scientist Training Program, is a big program.

Emil J Freireich, MD
0:24:43.9
It's fantastic. This is the best time of my life, because what I really care about is people. I think that the opportunity to teach young people is really the highest calling. The highest calling for any man is to be a physician. Some people think it's the ministry and things like that. But all theological systems are based in humanity, and the cornerstone of humanity is medicine, to relieve suffering. That's the highest calling. The second highest calling is teaching. The founder of medicine, Hippocrates, pointed out that the most important part of medicine is teaching; that is, you have to accumulate knowledge, and you have to transmit it.

I've had a chance to be a physician for 50 years, and now I've got a chance to be a teacher. I'm really enjoying it. It's a great thing. Dr. Mendelsohn has given me 70 percent of my time to

teach, and I'm doing that. I run the physician-scientist program. It's a grant-supported activity by the Cancer Institute. It's designed to provide, in 2 years, 1 hour a week, some of the basic principles of clinical research.

I also run the Oncology Core Curriculum, which was Andy von Eschenbach's idea when he was our first executive vice president. I'm sorry he's not still. The idea there is that everybody that comes to MD Anderson should leave with a core of basic science knowledge about oncology, regardless of their discipline. We have our basic science people cover the major areas of tumor biology. That's 1 hour on Monday. Then I use our institutional grand rounds to expose our faculty to ourselves. We have 900 faculty members. I try hard to get every faculty member to present his major research/patient care/educational interest in 20 minutes before an audience of his peers, and that's been enormously successful.

So those are 3 activities. Then I participate fully in the leukemia program, because that's where my scientific and clinical roots are. So up until this year, I have attended on one of our leukemia inpatient services 2 months a year. I have outpatient clinic 2 days a week, and I see new patients and follow my old patients. I also participate fully in all the Leukemia Department's teaching activities, which are quite extensive. We have about 6 hours a week of formal teaching for the fellows who rotate through, and for the faculty, to do our research. So I'm pretty busy.

Lesley Brunet, MA
0:28:32.1
It sounds like it.

Emil J Freireich, MD
0:28:36.0
But I'm having a great time. I'm compulsive about being on time; I'm compulsive on doing what I promise to do; and I expect others to behave the same way, which they rarely do, but sometimes they do. It's a real pleasure for me to be in a circumstance where I have very little responsibility. I still have responsibility for a limited number of patients, but I have an outstanding physician's assistant who helps me, and I still have responsibility for the teaching programs. But I have the intellectual freedom now to do pretty much what I want, the kind of thing we're doing, which I love.

I'm going to do for oncology what Max Wintrobe did for hematology. Dr. Wintrobe, who was the founder of American hematology, wrote *Hematology: The Blossoming of a Science,* which is still being published. When he retired and moved to California, he wrote this book about the history of hematology in America.

Lesley Brunet, MA
0:30:16.2
So that's what you're going to do?

Emil J Freireich, MD

0:30:17.8
Well, if I get fired. See, I love what I'm doing now. I have 2 idols: Sidney Farber and Joseph Kirsner. Sidney Farber, to a large extent, was responsible for oncology as a discipline. Dr. Farber was a pathologist at Harvard, and he discovered methotrexate, inducing remission in children.

Emil J Freireich, MD
0:00:00.3
The tragedy of the world is that he died without ever getting the Nobel Prize. Methotrexate started the whole field of molecular biology. It was the first antimetabolite, and it was intellectually conceived as an antimetabolite, because Dr. Farber thought that folic acid would be important for the treatment of leukemia and he found out it made leukemia worse.

He went to these people and said, "I need something that will inhibit folic acid." They synthesized antimetabolites. But what antimetabolites accomplished was dissection of the entire molecular biology of creating the macromolecules, because now you could make an antimetabolite to every metabolite you identified. That's why we have cloned the entire human genome.

Dr. Farber died without getting the Nobel Prize. It was the most fundamental discovery in the history of medicine, in my opinion. Dr. Farber was not a quiet, retiring type. He was a very assertive, proud figure. To a very large degree, the Clinical Center at the National Institutes of Health was his doing. He worked with Mary Lasker and the Lasker Foundation. He went to Congress. He got the money, and they built the building.

When I made my first discovery—a trivial one now, but then it was important—it related to the height of the white count in leukemia and the occurrence of central nervous system hemorrhage. Dr. Farber was on our external advisory board, and I remember that with tremendous trepidation we presented our great finding. Dr. Farber got up, and he was a very elegant person. He said, "That's what I love about the National Cancer Institute. It's so wonderful to give these young people an opportunity to work and be creative and to create hypotheses. Of course, this is all wrong, because we have proven that the white count has no prognostic value."

He came regularly to NIH, and I went regularly to the Children's Cancer Research Foundation. He was very important. And when I decided to move, he helped me decide where to go. Dr. Farber is my idol because he worked at his job, at his office, every day, and he died at his desk, writing a paper. They found him slumped over, pen in hand, the paper half finished.

Lesley Brunet, MA
0:02:58.3
Not a bad way to go.

Emil J Freireich, MD
0:03:01.2

That's what's going to happen to me. I'm going to die working.

My other idol is Joe Kirsner , because I have a daughter who has inflammatory bowel disease. She was diagnosed at the age of 18. Inflammatory bowel disease is, like all chronic illness, a lifetime illness. You never get rid of it. She came within millimeters of dying of this disease. Through a friend of a patient that I'd cared for here, Ernest Deal, we were able to get a private jet, and we actually took her to Chicago, to Joe Kirsner. Joe Kirsner is to inflammatory bowel disease what Freireich is to leukemia. He was the first full-time inflammatory bowel disease person in the world. I was the first full-time leukemia doctor in the world. Joe Kirsner created a miracle. He turned my daughter around. He educated her. She still returns to the University of Chicago once a year to see Dr. Kirsner's student. His name is Stephen Hanauer. Every time she goes, she has to have an adult with her because they use conscious sedation. You don't stay in the hospital. So I have to go up with her.

Lesley Brunet, MA
0:04:26.5
This is your daughter, but she's an adult.

Emil J Freireich, MD
0:04:28.1
She's 42.

Lesley Brunet, MA
0:04:29.7
She has to bring another adult along?

Emil J Freireich, MD
0:04:31.2
Yes, because she's going to be sedated, and they don't want to put her in the hospital, so she has to have someone with her to drive her and go to a hotel and stay with her to be sure she doesn't die. So while she's getting her colonoscopy, I go visit Dr. Kirsner.

Dr. Kirsner is about 92. I take my daughter, and I go to his office. He's still in the same office. He's still working. Every time I go, he gives me some references. He gives me some publications on the latest in inflammatory bowel disease. He's lost his wife. He's got macular degeneration, so he's having difficulty seeing, but he's at work every day, writing a book. Every year I go visit Joe Kirsner, and he makes me feel young. He's a terrific guy, and he's still in his office. He's got 2 people in the outer office who love him. The people in GI adore him. They do anything he recommends. He goes to the clinic every once in a while to see a patient, but he's a senior citizen. He's an emeritus professor, but he's full-time. He's there every day. Still has personality, and he's still publishing. He still meets with the young people. He still makes rounds. He does it at 92.

Lesley Brunet, MA

0:05:54.9
That's pretty good. I don't know that I want to do that at 92.

Emil J Freireich, MD
0:05:58.2
I would want to do it. It's terrific. Things are changing. We may be doing that at 100.

Lesley Brunet, MA
0:06:03.9
I'd like some hobbies.

Emil J Freireich, MD
0:06:06.4
The idea of retiring is such a tremendous waste of talent. When you take people who have reached the level of achievement that justifies retirement you have to benefit from their knowledge and their expertise. It's really tragic that Dr. Loo's in Washington. He should be here teaching young people. The Chinese respect the elderly. I think we have to learn that. People have wisdom and experience. Retiring, playing golf and tennis, who benefits from that?

Lesley Brunet, MA
0:06:43.4
Some people retire and do volunteer work.

Emil J Freireich, MD
0:06:47.9
It's a terrible thing to retire. I don't want to retire. There are many stories that inspire me of people who, after they finish one career, move into another career. I think a lot of people retire, get a motor home, and they travel around to see the sights. I'm going to do that. I'm going with my wife to New Brunswick for a week. But I'll go crazy in a week. I'll call the office every 3 days. There's only so much sightseeing and relaxing I can do, and that's it. I swim 20 minutes; that's it. Thirty minutes, maybe.

Lesley Brunet, MA
0:07:26.6
That's it. You're relaxed. That's all you need.

Emil J Freireich, MD
0:07:28.6
It's boring after that. Working is wonderful. It's wonderful to do things that you think will last forever and will benefit other people. There is no higher calling. The work you're doing—writing, creating knowledge, organizing the world—is terrific. It's a great place we live.

Lesley Brunet, MA
0:07:54.0

It is.

Emil J Freireich, MD
0:07:54.6
It's a great time we live in.

Lesley Brunet, MA
0:07:56.0
I'm enjoying this.

Emil J Freireich, MD
0:08:02.9
I am on vacation today. I'm not going to go to my meeting at 12:00, so I don't have to stop at 11:30. I can go on forever. When I get done with you, I'm going to go home and fight with my wife. I just decided this year that I was going to take vacation. I've never done it before. It's the first time.

Lesley Brunet, MA
0:08:20.9
A vacation?

Emil J Freireich, MD
0:08:21.9
Yes. I've never just stayed home.

Lesley Brunet, MA
0:08:26.4
I can understand that.

Emil J Freireich, MD
0:08:29.3
All of our vacations have been associated with professional travel. If I go to Paris to a meeting, we take a week. We've never just taken a vacation, but we're going to do it next week.

Lesley Brunet, MA
0:08:44.4
I think it's time.

Emil J Freireich, MD
0:08:45.4
I'm taking off August.

Lesley Brunet, MA
0:08:46.3

We're almost halfway through, and you're still here.

Emil J Freireich, MD
0:08:50.7
But I'm on vacation. I'm not going to any of the meetings. I'm not seeing any patients. I'm just relaxing.

Lesley Brunet, MA
0:09:00.2
When did you first become involved with the physician-scientists?

Emil J Freireich, MD
0:09:06.3
It started with this grant. I've always been training physician-scientists. That's the list I'm talking about. That's really an interesting story. In 1976, a long time ago, I gave the Karnofsky Lecture. It's published just recently. It wasn't published initially, because it was too controversial. I called attention to the fact that physician-scientists were being threatened by 2 factors: One was regulation, randomized trials controlled by the government, and the other was lack of funding from the NCI.

Lesley Brunet, MA
0:10:50.6
But this was happening in other disciplines as well, wasn't it?

Emil J Freireich, MD
0:11:02.6
Oh, yes. This was clinical research in general.

Lesley Brunet, MA
0:11:10.7
What do you mean by saying it was threatened by randomized trials?

Emil J Freireich, MD
0:11:17.3
The randomized trial is a device which is very powerful. It allows you to eliminate bias, but the most important part of randomized trial is that it allows total control of the research process. That's what DeVita used to make life miserable for us, because the randomized trial requires that you make a plan, and then you stop thinking. You just follow the protocol. That's not good. I began to have disputes with people over the randomized trials. I'd say, "Well, wait a minute. We do a randomized trial in good conscience, and one group looks a little better than the other group, wouldn't you rather get the treatment that's looking better than the bad one? Why should I continue to randomize people?"

Lesley Brunet, MA
0:12:33.2
Isn't the basic idea that you had to do this for a certain amount of time?

Emil J Freireich, MD
0:12:37.5
You had to do it until the magic number was released. After that speech, I became a pariah. I was "against the randomized trial." I'm not against it. I'm in favor of it. I explained that carefully in that article.

The trend in funding from the NIH for clinicians kept going down. Twenty years ago, Jim Wyngaarden, who was an NIH director, wrote an article. He said they're an endangered species. Tom Frei and I wrote an article, and they were an endangered species. The National Research Council had a panel. They studied it and said it was an endangered species. NIH had a panel; they studied it. It was an endangered species.

In 1997, a dramatic thing happened to clinical research. I have to explain the organization of clinical research. I told you about the Association of American Physicians that was founded around the turn of the century. This was all the professors who chaired departments and were accomplished physician-scientists. When science came into medicine, the younger physicians took up science, and before they got to be AAP members, they needed somewhere to present their stuff. So a young guy named Henry Christian, who was an AAP member, founded a thing called the American Society for Clinical Investigation. That was about 1910 or so, and it still exists. It was also honorific. It was for the associate-professor-level people who were doing good research, who had accomplished something in their lifetime. They elected so many members a year. It was a big honor, and it still is. At the present time, there are only 2 members of this at MD Anderson, Dr. Hong and I, and there are only 4 or 5 of these guys in ASCI.

Henry Christian then realized that there was another category of young assistant professors who hadn't quite made associate professor that were doing excellent research, and they needed a forum, but it was non-honorific. So he formed a society called the American Federation for Clinical Research. This was about 1940. This was the political organization of medicine when I came into it. These people were called the "Young Squirts," ASCI were called the "Young Turks," and AAP were the "Old Turks." When you came into academic medicine, you began to do research as a fellow, and you presented your papers to the AFCR. Membership was open to everybody. They had a national meeting. My first paper was given here. If you did good work here, you were elected honorifically to ASCI. You could give papers at the ASCI. If you were good at the ASCI, you were elected to the AAP. That was the hierarchy.

Medicine became specialized, and there were all kinds of specialty societies—American Society of Clinical Oncology, American Society for Endocrinology, American Society for Heart—and all these societies were booming, but the tri-societies were declining. They were declining because these Young Squirts found they had a better forum in their specialty society than they did in the general medical society. So a dramatic thing happened. The Young Squirts' board of

directors, all 40-year-old guys, decided that, for their survival, they had to change their name to the American Federation for Medical Research. They had a journal that consisted of abstracts, which was called *Clinical Research*, and they decided at the same time to change their journal from *Clinical Research* to *Journal of Investigative Medicine*.

Now, why did they do that? They did that because it had gotten to the point where a physician doing research on patients could not get funding. So the only way the departments of medicine could maintain their academic status was that physicians had to go into the lab and do PhD research in order to get a grant. If they got funded as a PhD, they didn't care about their clinical research. They only cared about their lab research. They had to compete with PhD's to get funded.

So the Young Squirts consisted of guys who only worked in the lab. They didn't like clinical work. They didn't like this clinical stuff. When that happened, a physician named Gordon Williams, a professor of Harvard in endocrinology, was chair of an NIH committee that investigated how clinicians fared in the study sections. They reviewed 2 full grant cycles, and they showed objectively that physicians had a lower probability of success than the laboratory scientists. So Gordon Williams wrote a letter to the editor of this journal, and he said, "I'm very upset about changing the name *'Clinical'* out of *Clinical Research*. This is a symptom of something very bad happening with medicine. Maybe it's time to start a new society."

When I saw that letter, I immediately wrote a letter to Gordon Williams, and I said, "You are dead right. I've been working on this for 20 years. It's time for those of us who are the last survivors of the patient-oriented research community to found a new society." We did just that in 1998. We started with 7 people, including Gordon Williams, myself, Ed Ahrens, who had written a *Crisis in Clinical Research*, and Jules Hirsch at Rockefeller University. There were 7 of us. We met, and we said, "It's time to start a new society." We're now in the fourth year. We have a new society. It's called the Association for Patient-Oriented Research. We struggled a long time about the name. We didn't want to use the words "clinical research," and I'll tell you why.

The reason we don't use the word "clinical" anymore is because the NIH had commissioned a study section to find out if they were allocating their funds properly to clinical research. The chairman of this committee was David Nathan, who was the director of the Dana-Farber Cancer Center, and they ended up doing a study. The Gordon Williams study was commissioned by the Division of Research Grants. This was commissioned by NIH. They found that the clinical research was getting just as money as laboratory research. Well, how did they do that? They did it by defining clinical research as anything involving human tissue. So every lab scientist working on HL60, any cell line, was called "clinical research." Anyone who worked on a sample from the clinic was called "clinical research." And so they proved that clinical research, all encompassing, was getting plenty of money. But there was one thing missing: the doctors. There were MD's working in lab, but there were no scientists working in the clinic. So we abandoned the word "clinical" because David Nathan's committee had taken it away from us. "Clinical" now meant everything.

So we invented this word, "patient-oriented research." To do research, you require 2 ingredients, an MD—PhD can't do it; doesn't have a license—and a patient, someone who's sick. It could be a volunteer, but he's got to be alive. They both have to be alive. And we used the word "association" instead of "society," as we wanted to be egalitarian. We didn't want anything honorific. This is a place where patient-oriented physicians can come together, and we can campaign. Our first meeting was in '99. We've had 3 meetings. The Society is struggling, but it's coming along.

The outcome of our struggles has been that this word is catching on. In *Science*, just 2 weeks ago, there was an editorial written by the scientific directors of the major funding organizations, calling attention to the fact that we need to support young physicians who want to do research. This word is catching on. The first thing that happened was the NIH floated what's called an RFP, Request for Proposal, and it was for physician-scientist training. I wrote an application, and Dr. Leonard Zwelling, who was in the Office of Protocol Research, supported it. Dr. Stephen Tomasovic [oral history interview], in the Office of Education, supported it. Then I got a call from Tomasovic that the medical school is also preparing an application, and Baylor's doing an application.

Lesley Brunet, MA
0:24:31.5
Baylor's been doing it for a while, haven't they?

Emil J Freireich, MD
0:24:36.8
No. We went in the first round. We were in the first group. There were 20 awarded the first round. They said, "Freireich, we'll never get 3, so let's join forces, and we'll get together with the medical school," which we did. Then Dr. Tyson was made PI, for a number of complicated things, and I'm the co-PI. We started this program, and it's going very well.

Did I tell you about my sabbatical in Washington, when I did my study?

Lesley Brunet, MA
0:25:16 .1
Yes.

Emil J Freireich, MD
0:25:19.4
The outcome of that were these K series. So the next thing is the K12 program. The K12 had an RFA called Training in Patient-Oriented Research. The first Request for Proposals came out in '99. Robert Bast was head of Medicine at the time, and he applied for one. Then in '00, Dr. Margaret Kripke asked me to be educator, and I called Dr. Tomasovic and said, "Let's look at all the grants available to support my program." We found this POR thing. He said, "Wow, Dr. Bast has just written one last year, and I think it's going to get funded, so we're competing." I

met with Dr. Bast, and we worked out a thing where our grants would presumably not overlap. His is for faculty development. Mine would be for trainees. In 2 weeks, after I got appointed, I got this grant in for the deadline, and we got a 3.5 priority, which is about as bad as you can get.

Lesley Brunet, MA
0:26:45.4
Out of a total score of?

Emil J Freireich, MD
0:26:48.5
Well, it's 1 to 5. This is terrible. But we got a critique. We put it together in 2 weeks, and this year, we went in again. We met the deadline and sent it in 6 weeks ago. This time we did a good job. I fully expect to get funded. If we do get funded, that's going to fund 7 full-time clinical fellows in patient-oriented research. If it doesn't get funded, I'm going to fund it anyway. I'm working with the drug companies to raise money. I have one nibble. I'm going to create scholars, Bristol-Myers Scholar in Oncology Research. All they have to do is give me $100,000 a year for 5 years, and we'll train a fellow, like the Markle Scholars. So that's one of my big ambitions. I want to have a patient-oriented scholar program.

Bast [oral history interview] is very cooperative. We work together. He has the Faculty Development Program, and it's going very well. Dr. Mendelsohn has a physician-scientist program. When we have our big ball downtown, the money is going to go to the physician-scientists. I think they appoint 2 a year and they keep them going for 3 years. There's more and more interest in patient-oriented research, and that's good.

Lesley Brunet, MA
0:28:18.3
Yes, that is good.

Emil J Freireich, MD
0:28:21.8
We need it.

0:29:06.6 (end of interview session four)