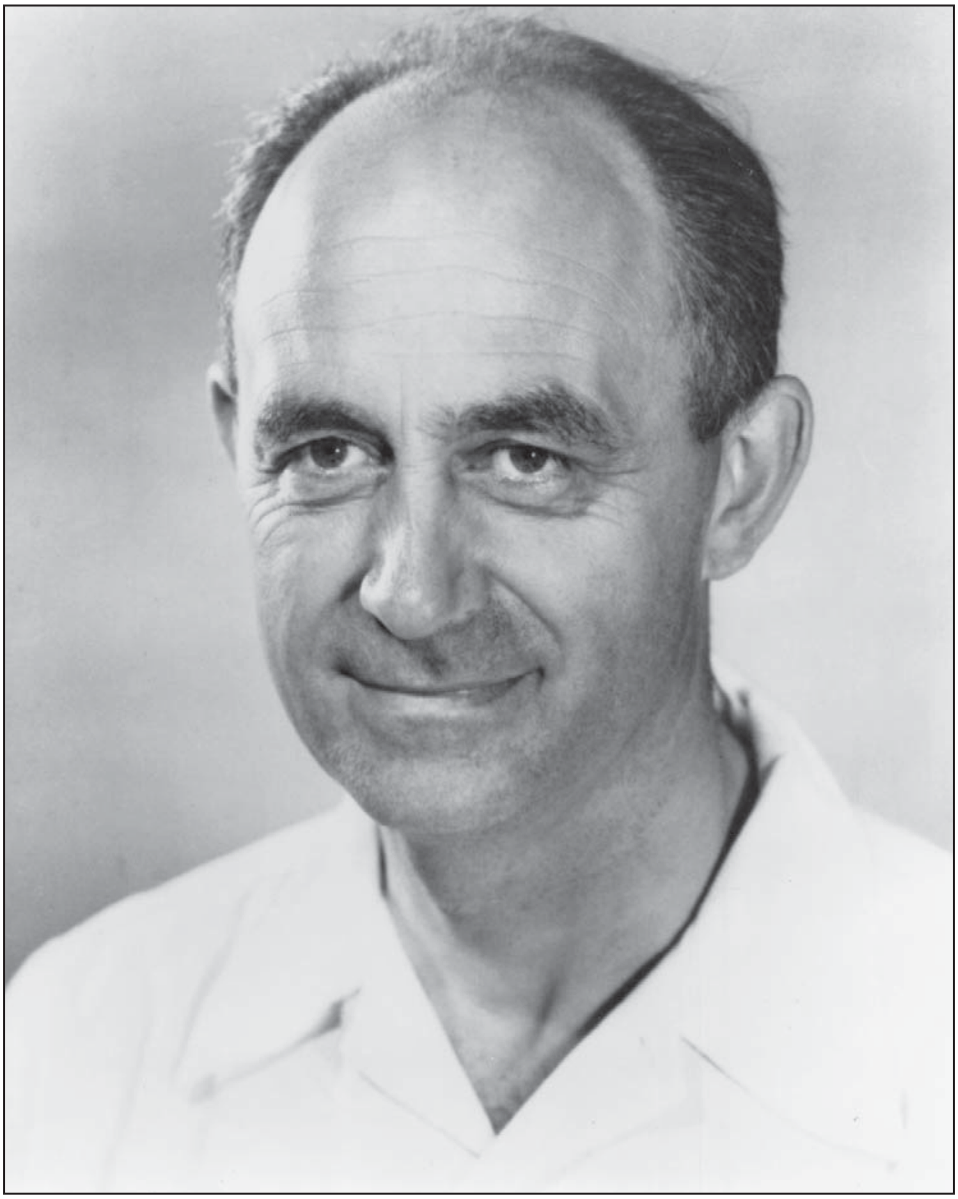
Enrico Fermi:
The Master Scientist



by Jay Orear and others

Enrico Fermi–The Master Scientist

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Enrico Fermi: *The Master Scientist*

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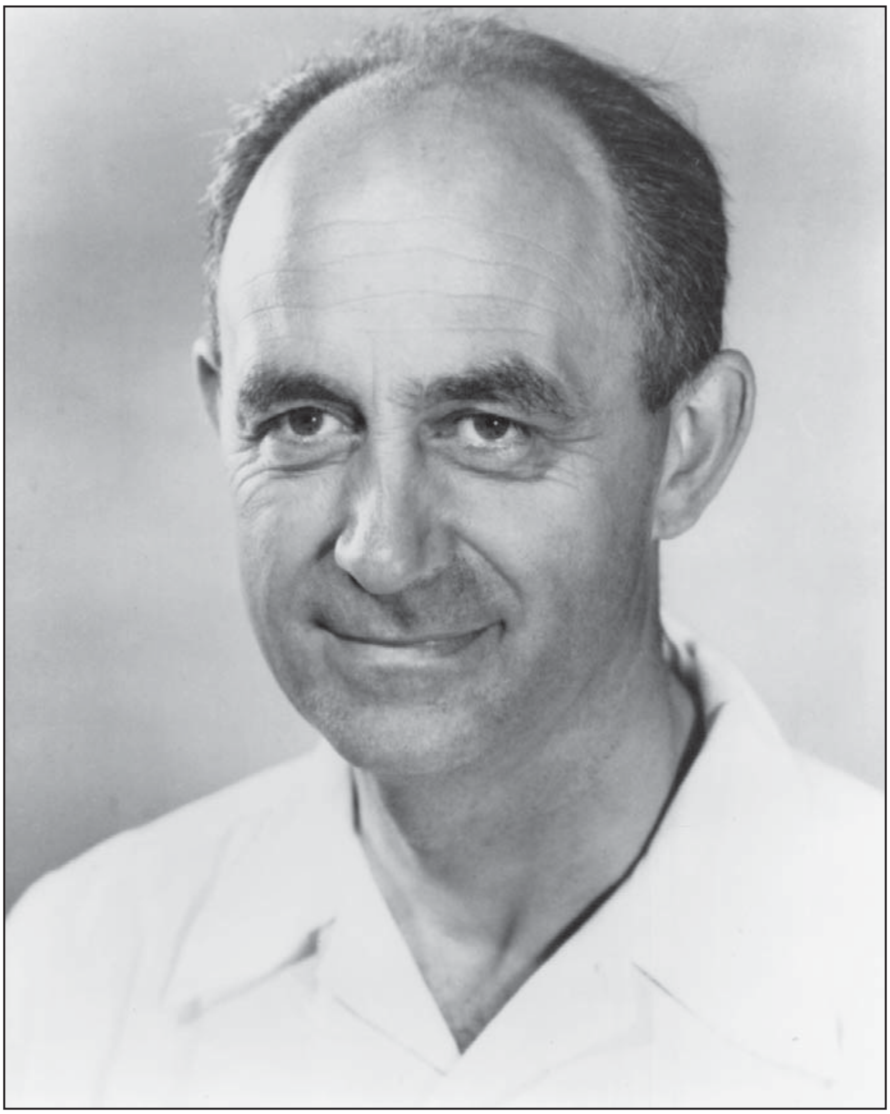


Figure 1.
Photo of Enrico Fermi while at Los Alamos. Note the smile and twinkle in his eyes, which were typical. It is as if we can tell what this man is like on the inside just from this photo. Courtesy Fermilab history of accelerator physics office.

Note: Nearly all of the figures can be seen by clicking on the following link: www.library.cornell.edu/dcaps/orear. Several index frames per page will appear. If you click on one of these mini-frames, it will fill up the entire screen at high resolution. Or if you click on the web address in the list of figures, it should also take you to the corresponding high-resolution image.

Contents
Part A (according to Jay Orear)

Chapter **Page**

1. Introduction ... 1
2. The Cornell Fermi Symposium ... 3
3. The Four Student Reunions ... 9
4. My First Meetings with Fermi ... 11
5. Orear, Rosenfeld, and Schluter ... 13
6. Other Examples of the Fermi Approach ... 19
7. My Ph.D. Thesis ... 25
8. Fermi Intuition ... 31
9. Fermi Humor ... 35
10. The Excited State of the Proton ... 45
11. Fermi and Strong Focusing ... 47
12. Fermi and Politics ... 49
13. Fermi and Religion ... 57
14. The Fermi Family ... 59
15. Fermi and Creativity ... 63

List of Figures

1. Enrico Fermi while at Los Alamos (fig. 1a.jpg) ... i
2. Group photo of the speakers at the Cornell Fermi Symposium (Group Shot 1.jpg) ... 4
3. List of Fermi Nobel Prize winners ... 6
4. Second reunion of Fermi students (Fig. 2a.jpg) ... 9
5. Contents of Orear, Rosenfeld, and Schluter ... 9
6. Cover of Physics by Orear ... 19
7. Contents of Notes on Statistics for Physicists ... 22
8. Dick Feynman talking to Bob Wilson ... 23
9. Fermi's trolley car in Chicago cyclotron ... 25
10. T. D. Lee ... 34
11. Dick Feynman lecturing at Cornell ... 36
12. Announcements of Fermi's last two APS lectures ... 37
13. Energies of the world's highest-energy accelerators ... 39
14. Central laboratory building at Fermilab ... 40
15. Fermi's ultimate accelerator ... 41
16. Fermi's jigsaw model of strong focusing ... 47
17. Fermi letter to Pegram accepting offer of Columbia professorship ... 49
18. Fermi's October 1954 press release ... 53
19. Birthplace and great-granddaughter of Enrico Fermi ... 59
20. Fermi's family tree ... 60
21. Wedding photo of Enrico and Laura ... 61

Part B (according to others)

Chapter

16. Welcome to Cornell by Dale Corson 69
17. The Italian Navigator by Carl Sagan 71
18. Pilgrimages to Rome by Hans Bethe 75
19. Fermi and the Nuclear Age by Al Wattenberg 83
20. Fermi at Chicago by Valentine Telegdi 89
21. Fermi at Columbia, Los Alamos, and Chicago by Harold Agnew 101
22. (1) Working with Fermi by Robert Wilson 107
    (2) Fermi and Politics by Robert Wilson 113
23. The Fermi Family by Jane Wilson 117
24. Glimpses of Fermi in Chicago and Los Alamos by Dick Garwin 121
25. Fermi and Technology by John Peoples 125
26. A Different Perspective by Nella Fermi 129
27. Comments of Some Former Grad Students by Art Rosenfeld 139
28. Glicksman Comment by Maurice Glicksman 141
29. Wolfenstein Comment by Lincoln Wolfenstein 143
30. My Life as a Physicist's Wife by Laura Fermi 145
31. Enrico Fermi by C. N. Yang 155
32. Fermi Centennial Comments by Leon Lederman 159

Index 161

Part B Figures

22. Dale Corson 69
23. Carl Sagan 71
24. Hans Bethe, Boyce MacDaniel, and Bob Wilson in Synchrotron tunnel 75
25. Cowboy Wilson bareback on horse 76
26. Neutron flux as a function of number of layers in the nuclear pile 84
27. The Rome Fermi Museum 105
28. Fermi and Wilson in a Los Alamos group picture 107
29. Bob Wilson, Jane Wilson, and I. Rabi 117
30. Garwin speaking to Bethe 121
31. Laura Fermi presenting her paper at Erice 145
32. Frank Yang 155
33. Leon Lederman and Bob Wilson singing their song 159

Chapter 1
Introduction

One of the purposes of this book is to give the reader a feeling for Enrico Fermi the man—his personality, creativity, intuition, sense of humor, warmth, ebullience, humility, and how he related to students as well as being a great scientist and teacher. The approach is to make use of my firsthand memories and experiences as well as those of others. Some of Fermi's close friends have exchanged anecdotes and have spoken at meetings devoted to his memory. Gatherings have included symposia, dedications, birthday celebrations, and four reunions of his former grad students and associates. Much of such material has gone unpublished. I have had the privilege to be (a) one of his last two grad students and research associates, (b) the primary organizer of a Fermi symposium at Cornell University on October 14, 1991, (c) the organizer of the first two of the four Fermi student reunions, and (d) an invited speaker at six of the one-hundredth-birthday celebrations in 2001. They were (1) Fermi Day at the opening day of the Fermi Summer School at Varenna, Italy, (2) Planetary Emergencies School at Erice, Sicily, (3) Particle Physics School at Erice, Sicily, (4) Press Conference dinner at Rome, Italy, (5) the four-day Fermi Rome Congress, and (6) Fermi student session at the UCLA Fermi Symposium. In addition, I attended, and was a resource person at the Columbia University Fermi Symposium, and I gave university lectures about Fermi at (1) University de Los Andes, Bogota, Colombia, (2) UCLA, (3) Florida International University, Miami, and (4) Cornell University. So I am in a special position to put together quite a bit of new material on Fermi. I hope to show that because these various firsthand contacts come to common conclusions about Fermi's characteristics and history, then it is likely that those conclusions are correct.

After the Cornell symposium, several attendees suggested that I edit these new materials into a book before they were lost and forgotten. Fortunately it was all videotaped by the Cornell Physics Department, and in my retirement I have finally found time to get it transcribed and see how it fits in with other sources of knowledge about Fermi. The two main sources containing some overlapping material are the biographies by Laura Fermi and Emilio Segré (both published by University of Chicago Press). My approach is mainly that as seen by a Fermi grad student and research associate, whereas Laura Fermi's approach is as a wife, and Segré's relates more to Fermi's earlier career and as a co-worker. The technical level of the Segré book is above that of this one. I have attempted to make most of this book useful to nonscientists as well as scientists. The L. Fermi and E. Segré books emphasize his work and life in Italy, whereas I emphasize his career in the United States, especially in Chicago.

I am indebted most of all to Enrico himself for the seven years he spent making me into a physicist whom I hope he would be proud of. I am also indebted to his many colleagues for interviews during the six celebrations of his one-hundredth-birthday year that I attended. And for the help that Dick Garwin, Carl Sagan, and the Cornell Physics Department gave in organizing the 1991 Cornell Fermi Symposium. I wish to thank Nino Zichichi and the Plenum Press for permission to reprint the full text of the excellent talk that Laura Fermi gave at Erice, Sicily, on July 16, 1975, and to Enrico's granddaughter Rachel Fermi for special insights into the family. I am grateful to Dean Robert Cooke and other officials at Cornell for their splendid cooperation. They showed me how to do new things by doing much of them for me.

Chapter 2
The Cornell Fermi Symposium

October 14, 1991
Memories of Enrico Fermi

In the early 1990s, those closest to Enrico Fermi were rapidly dying off—people like Laura Fermi, Emilio Segré, Herb Anderson, and Leona Marshall. By 1991 many of us felt that Fermi's contribution to the world was so exceptional that it should be well documented by available firsthand observers before it was too late. The occasion of the 1991 Bethe lectureship at Cornell University provided a unique gathering of thirteen close firsthand observers. Dick and Lois Garwin, Hans and Rose Bethe, Bob and Jane Wilson, Val and Lia Telegdi, Boyce and Jane McDaniel, Dale and Nelli Corson, and Jay Orear would all be at the same place (Ithaca) at the time of Dick Garwin's Bethe lectureship (October 1991). Garwin agreed with my suggestion that it would be a good idea to invite the surviving Fermi acquaintances and spend one day of Garwin's lectureship sharing our memories of Fermi. Since I was the chairman of the Bethe Lecture Committee it was easy to get approval and some funds. In our early planning we invited Carl Sagan to be the master of ceremonies. Orear, Garwin, and Sagan did most of the planning and organizing. We tried to invite all who had known Fermi personally, and many of them were able to attend. The program was as follows:

Subject	**Speaker**	**Time**
Welcome	Dale Corson, Chancellor	9:00 a.m.
Introduction	Carl Sagan	9:10 a.m.
Pilgrimages to Rome	Hans Bethe	9:30 a.m.
Film and audio clips	Jay Orear	11:00 a.m.
Fountain in Rome	Joe McEvoy	11:20 a.m.
Experiments in the 1940 s	Al Wattenberg	11:35 a.m.
Lunch, movie, and tours		12:00–2:00 p.m.
Columbia – Los Alamos	Harold Agnew	2:00 p.m.
Pre-Chicago Years	Bob Wilson	2:15 p.m.
Laura and Family	Jane Wilson	2:35 p.m.
Fermi at Chicago	Val Telegdi	2:50 p.m.
Chicago, Los Alamos	Dick Garwin	4:10 p.m.
Fermi and Technology	John Peoples	4:30 p.m.
Los Alamos Inventions	Perce King	4:45 p.m.
Reception and dinner break	5:45 p.m.	
A Different Perspective	Nella Fermi	8:00 p.m.
Panel discussion	Rosenfeld, Glickstein, Wolfenstein	8:30 p.m.

Figure 2.
Group photo of the speakers at the Cornell Fermi Symposium (left to right): Back row: L. Wolfenstein, J. Peoples, U. Habersheim, A. Wattenberg, J. McEvoy, R. Martin, J. Friedman, D. Nagle, H. Agnew, M. Glicksman, L. Perce King. Front row: R. Garwin, V. Telegdi, J. Orear, Nella Fermi Weiner, H. Bethe, R. Wilson. D. Corson, A. Rosenfeld, C. Sagan, and J. Wilson are missing. Courtesy Cornell physics department.

Fermi Symposium Out of Town Attendees

Name	**Affiliation**
Jim Chimbidis and crew	film producer for ANL
Adrienne Kolb	archivist for Fermilab
Rachel Fermi	Giulio Fermi's daughter
Nella Fermi	Enrico's daughter
A. DePino	Physics teacher, E. Fermi High School
Principal	E. Fermi High School
Darragh Nagle	Los Alamos
Uri Haberscheim and wife	MIT
Al Wattenberg	University of Illinois
Jerry Friedman	MIT
Harold and Beverly Agnew	Former director, LANL, and spouse
John Peoples	Director, FNAL
Val and Lia Telegdi	CERN and University of Chicago professor emeritus and spouse
Bob Schluter	Northwestern
L. Wolfenstein and wife	CMU
Perce King	Santa Fe, New Mexico

Dick and Lois Garwin	IBM; spouse
Art and Roz Rosenfeld	University of California; spouse
Vicki Weisskopf	MIT
Maurice and Yetta Glicksman	Provost, Brown University, and spouse
Ron Martin	ANL
Joe Lach	Fermilab

I think all the speakers came to the same assessment of Fermi as expressed by Val Telegdi:

> None of the great scientists who worked at Chicago ever had a greater impact on his immediate and worldwide surroundings than did Enrico Fermi. Nobody in the history of modern physics possessed greater versatility than he. He had just as great achievements in pure theory as in concrete experimental work. He could with equal ease solve abstract problems or design and build with his own hands astonishingly useful experimental "tools". . . . To these qualities he added those of an exceptionally lucid lecturer and expositor. As well as an active and patient thesis supervisor. . . . But it defies the bounds of human inspiration to speculate that any other man or woman might have played Fermi's role as a teacher in the broader sense of this term. Through the influence of his students, Fermi effectively revolutionized the training of students in the United States and one hopes in the whole world.

In Chapter 16, I list 18 of the many accomplishments of Fermi, and one of them is what Telegdi calls "Fermi's role as a teacher in the broader sense."

This assessment of Fermi by Telegdi must be correct if so many independently minded firsthand observers would come to the same conclusions as they did at this symposium. I also feel, as does Telegdi, that scientists all over the world have been and still are being exposed to Fermi's way of looking at science and doing science. A similar and perhaps even stronger appraisal of Fermi was expressed by our first speaker, Hans Bethe:

> My conversations with Fermi showed me a completely new approach to physics. I had studied with Sommerfeld, and Sommerfeld's style was to solve problems exactly. You would sit down and write down the differential equation. And then you would solve it, and that would take quite a long time; and then you got an exact solution. And that was very appropriate for electrodynamics, which Sommerfeld was very good at, but it was not appropriate at all for nuclear physics, which very soon entered all of our lives. Fermi did it very differently, and Dale Corson already described it very well, namely he would sit down and say, "Now, well, let us think about that question." And then he would take the problem apart, and then he would use first principles of physics, and very soon by having analyzed the problems and understood the main features, very soon he would get the answer. It changed my scientific life. It would not have been the same without having been with Fermi; *in fact I don't know whether I would have learned this easy approach to physics which Fermi practiced if I hadn't been there.*

Bethe also seems to be rating very highly Fermi's contribution as a teacher.

Another sign of Fermi's strong positive influence on his students and others is the large number who became Nobel Prize winners (Fig. 3). Of course, the Fermi students received their Ph.D. degrees before performing their Nobel Prize research. There were (1) **Lee** and (2) **Yang** for the correct theory of nonconservation of parity, (3) **Owen Chamberlain** and (4) **Emilio Segré** for the discovery of the antiproton, (5) **Jack Steinberger** for the muon-flavored neutrino, and (6) **Jerry Friedman** for measurements of the quark in electroproduction. (Jerry was an active Fermi student, but Fermi died before Jerry earned his Ph.D.) (7) **Dick Garwin** was also a student of Fermi and if the Nobel Prize had been awarded for the experimental discovery of parity violation in pion-muon and muon-electron decay (as it should have been), it would have been shared by him. (8) **Jim Cronin** was formally a grad student of Sam Allison (who was very busy as director of the institute). However, his office was next door to Fermi's office, and Jim frequently visited with his close neighbors and also attended Fermi's courses and Fermi student group meetings. It was agreed that Fermi would help out with Allison's students. Cronin received the Nobel Prize for the discovery of CP violation. (9) **Maria Mayer** was not a Fermi student, but she was a faculty member who consulted closely with Fermi. She gives credit to Enrico in her paper on the nuclear shell model for supplying key ideas such as using a spin-orbit interaction to explain the mysterious nuclear magic numbers that she had discovered (but couldn't explain). I think it is fair to say that the shell model is a joint product of the two of them. One of Fermi's "trainees" from the Italian days was (10) **Hans Bethe** as a postdoc. Hans made many discoveries including the thermonuclear energy source of stars. This is a total of 10 followers of Fermi receiving Nobel Prizes in a short period of time. I don't know of any other physicist who has left such a strong mark on his followers. A possible eleventh is **Murray Gell-Mann,** who joined Fermi on the Chicago faculty as a young instructor. Millie Dresselhaus has told me that while at a party in his house, Fermi had patted Murray on the back and predicted that he would become a Nobel Prize winner. A more recent Nobel Prize winner who also had spent a year or two working with Fermi is (12) **S. Chandrasekhar**. Depending on how we count, Fermi training led to 10, 11, or 12 Nobel Prizes. I estimate the probability that an existing Nobel Prize winner in physics "give birth" to another winner is less than 1/10. So if this is purely random, the probability of one winner giving birth to 10 other winners would be one-tenth to the 10th power or one in 10 billion, which is essentially impossible. The explanation is that Fermi was very creative and the world's best trainer or teacher of physics. Also, his known talent and pleasant personality attracted the best students. According to the talk given by Lincoln Wolfenstein, all the Chicago theory students would have preferred to be Ph.D. students of Fermi.

Figure 3.
Twelve "trainees" of Fermi who received (or should have received) the Nobel Prize in physics:

1. T. D. Lee
2. Frank Yang
3. Owen Chamberlain
4. Emilio Segré
5. Jack Steinberger
6. Jerry Friedman
7. Dick Garwin
8. Jim Cronin
9. Maria Mayer
10. Hans Bethe
11. Murray Gell-Mann
12. S. Chandrasekhar

I was asked at the time of the Cornell symposium to edit a book presenting the dozen or so invited talks. The book that has evolved attempts to reveal the real Enrico Fermi, his personal traits, in addition to his many great contributions to science and to the world. Some of the personal traits are his sense of humor, famous intuition, and creativity. I have chosen those talks that help reveal Fermi's personality, and I have devoted a chapter to each such speaker in Part B. Each talk is verbatim (with minor editing) from the Cornell videotapes. Whenever laughter is apparent, it is noted in the transcript. I have at times added comments of my own. Some other talks occurring after 1991 are quite pertinent and have also been included in Part B. Part A has evolved from my Cornell talk with considerable additional material and analysis consistent with the above goals. My Cornell talk made use of the few recordings that are available of Fermi via audiotape and film. Some of the audio and video sources I used are (1) my 5-minute condensation of the 50-minute film *The World of Enrico Fermi*, (2) the video of the tenth anniversary of the first nuclear chain reaction produced by *See It Now* of CBS news, (3) the audiotape of Fermi's 1954 lecture titled "Physics at Columbia University, the Genesis of the Nuclear Energy Project," (4) Fermi's personal notes and slides on his talk as retiring president of the American Physical Society in January 1954, and (5) "To Fermi—with Love," an audio recording produced by Argonne National Lab, making use of 16 friends of Fermi plus a commentator. The video *See It Now* contains live speeches by Fermi, Arthur Compton, Leo Szilard, and Leona Marshall. Two of the videos show a reenactment of the famous phone call of Arthur Compton to James Conant that gave the good news to Washington using the code: "The Italian Navigator has safely arrived in the New World." I strongly recommend the complete 50-minute film *The World of Enrico Fermi* (now on VHS videotape). It gives insight not only into Enrico's personality but to that of his wife, Laura, and even her details of their courtship. For up-to-date information on how to obtain this video, contact Professor Gerald Holton, Physics Dept., Harvard University, Cambridge, Mass.

I am very grateful to the other speakers who so kindly let me use their papers (Chapters 16 to 32). Eighteen talks are presented here. Fourteen of them were presented at the Cornell symposium and were videotaped. Bob Wilson gave me a second "talk" to use in this book. I have acquired rights to reprint a talk by Laura Fermi titled "My Life as a Physicist's Wife" that was given in Erice in 1975. It was an updating for her book *Atoms in the Family*, written 20 years after her husband's death.The last two presentations were by Frank Yang and Leon Lederman, who were unable to give them at the Fermi Rome Congress. Also I wish to thank Cornell University and its Physics Department for their splendid cooperation in making the 1991 symposium such a great success and to thank members and organizers of other Fermi symposiums who invited me to speak as well as attend.

Chapter 3
The Four Fermi Student Reunions

I. October 13, 1991

Cornell hosted a dinner for all of Fermi's former students who were attending the symposium (of which there were at least a dozen) and their spouses. This first reunion was held in the boardroom of the Statler Inn on October 13, 1991, the night before the Fermi Symposium. We shared the experiences we had with Fermi as well as the excellent food and service of the Cornell Hotel School.

II. December 3, 1992

Figure 4.
Photo taken by Roger Hildebrand at the end of the second Fermi reunion. (left to right): Back row: A. Wattenberg, M. Glicksman, G. Farwell, R. Schluter, V. Telegdi, J. Friedman, O. Chamberlain. Front row: A. Rosenfeld, U. Haberscheim, J. Hinton, G. Yodh, J. Orear.

Art Rosenfeld suggested that we have another student reunion a year later where we could relate what each of us had done since leaving the University of Chicago. A very convenient date that would fit into most of the Fermi students' schedules was the fiftieth anniversary of the first nuclear chain reaction that was celebrated at the University of Chicago on December 2, 1992. Since I had the names and mailing lists, I suggested to Roger Hildebrand that a Fermi student reunion be held the morning after the December 2 symposium. Roger made arrangements at his end that included meeting in a spacious Physics Department lounge. We decided to invite Chicago physics faculty and grad students to sit in. The meeting ended with a group photo and a free lunch. Dick Garwin and Jack Steinberger had to leave early so they missed out on the photo and lunch. The remaining Fermi students and associates are shown in Figure 4.

III. September 29, 2001

The 2001 Chicago reunion was an open meeting organized by Jim Cronin that was scheduled on the exact day of Fermi's one-hundredth birthday. Unfortunately I could not be in two places at the same time and I had accepted a prior invitation. I gave two talks at the Rome Fermi Congress during the four days that straddled Fermi's birthday. The Fermi associates who were able to attend the third reunion were George Farwell, Jerry Friedman, Dick Garwin, Murray Gell-Mann, Maurice Glicksman, Murph Goldberger, Roger Hildebrand, Joan Hinton, Darragh Nagle, Bob Schluter, Jack Steinberger, Al Wattenberg, Lincoln Wolfenstein, Courtenay Wright, and Gaurang Yodh. Jim Cronin is compiling these talks into a book titled *Fermi Remembered*.

IV. December 1, 2001

The fourth reunion took place as part of a West Coast two-day Fermi Symposium held at UCLA on November 30–December 1, 2001, also during the year of Fermi's hundredth birthday. It was titled "The Life and Times of Enrico Fermi" and included a Fermi's student roundtable consisting of Harold Agnew, Richard Garwin, Marvin Goldberger, Nina Byers, Steven Moszkowski, Arthur Rosenfeld, William Slater, Gaurang Yodh, and me.

Chapter 4
My First Meetings with Fermi

By chance I lived just across the Midway from the University of Chicago. While a senior at Hyde Park High School I applied for a full scholarship starting in June 1943 and was a winner in the competition. In those days full tuition was $100 per quarter. After four quarters I was drafted into a navy electronics program where I had the opportunity to continue my college education part-time via home-study courses. After one year of navy electronics training I became an electronics instructor at Navy Pier, Chicago. After the war with Japan had finished I was scheduled for discharge but was asked to volunteer to evaluate shipboard electronic damage in the Bikini atomic bomb tests scheduled for the summer of 1946. I chose to volunteer, and I was back to Chicago in time to start graduate physics and math courses in the fall of 1946, the same year that Fermi joined the faculty at the University of Chicago. My first course with Fermi was Quantum Mechanics taken in the fall quarter of 1947. I was just one face out of almost 100, but I really met him in a more unconventional way. That same quarter I also had registered for a physical education course called Social Dancing. Early in the course one of the coeds in the class invited me to a dance party at a girlfriend's house. As we were walking to the house that night she happened to mention the name of her girlfriend as Nella Fermi, an art major. I asked whether her friend was the daughter of *the* Fermi. Being an art major, my date had never heard of Enrico Fermi, and Nella was not one to brag about her father. But once I entered the door, I was greeted by the warm, smiling face of my quantum mechanics instructor. I was surprised that Fermi recognized my face, and as we entered he asked me what I thought of his quantum mechanics course.

The party was a square dance with Harold Agnew as the caller. Harold was a more senior Fermi student who had worked with Enrico at Los Alamos and who later became director of Los Alamos. Many at the dance were Nella's and Laura's friends (mostly female) and Enrico's co-workers (mostly male). I was an indirect guest of Nella and not Enrico. I was invited as a friend of a friend of Fermi's daughter. These Fermi square dances were held once a month. Fortunately I was better than the average square dancer. From then on I was on the guest list of the Fermi family. The guest list was worked out by Nella, Laura, and Enrico. Harold Agnew did the calling and supplied the dance records. Both he and I have the impression that Nella and her father enjoyed working together in organizing those parties.

I can give an idea of what a good sport Enrico was by relating one experience at those monthly parties. Sometimes between the sets of dancing, there were party games. I once proposed a group version of Twenty Questions. I suggested that the guesser be one of the world's best logical thinkers. So Enrico was chosen and he gladly agreed to step out of the room. Then I proposed to the rest of the crowd that we not choose any object for him to guess, but instead we answer "yes" if his guess ends in a vowel, "no" if his sentence ends in a consonant and "sometimes yes and sometimes no" if the sentence ends in a "y." So we called Enrico back into the room and stood in a circle around him. He could choose anyone in the circle to answer any of his yes or no questions. He rather quickly realized that he should ask some redundant questions and then he remarked, "I think you have made up a story with some built-in contradictions." I replied to him, "How could we all come up with the same crazy story and be in complete agreement with each other?" He never did discover the vowel-consonant code and finally had to give up. I was proud that so early in my career I had gotten the better of our great master.

Not much later, again by pure coincidence, I encountered Enrico ice-skating by himself at a university rink. He greeted me and it seemed only natural to join him. I didn't even give it a second thought. It was clear that he enjoyed young people, and we got better acquainted in this and subsequent ice-skating sessions. It was not beneath him to associate freely with students and to treat them as equals. In fact, I think he enjoyed young physics students more than some of his older colleagues. In the course of writing this book I learned that Fermi did interview some of the outstanding students such as T. D. Lee and Dick Garwin and young instructors such as Murray Gell-Mann.

Another example of Fermi's enjoyment of young people was that he often ate lunch in the large student cafeteria (the Hutchinson's Commons) rather than at the Men's Faculty Club where most of his fellow faculty members ate. The center long table at the student cafeteria became known informally as the Fermi table, but anyone was welcome. Several of those who frequented that table later became Nobel Prize winners. In the Chicago Physics Department of that time the younger grad students felt that some of the older grad students (like Lee, Yang, Chew, Goldberger, Garwin, Wolfenstein, Steinberger, and Rosenbluth) were better teachers on the whole than the faculty at that time (except, of course, for Fermi, who was clearly the best). Fermi was a modest person and liked to be treated as one of the crowd. Just to give one example of his modesty, even though one of his many great achievements was the discovery of Fermi statistics, he *always* referred to it as "Pauli statistics."

Chapter 5
Orear, Rosenfeld, and Schluter

C O N T E N T S

CHAPTER I. PROPERTIES OF NUCLEI Page 1

A. Isotopes, Charts and Tables 1
B. Packing Fraction and Binding Energy 2
C. Liquid Drop Model 5
  1. Semi-empirical mass formula 6
  2. Isobaric behavior 8
  3. β-emission 9
  4. Periodic shell structure 9
D. Spin and Magnetic Dipole Moment 9
E. Electric Quadrupole Moment 15
F. Radioactivity and its Geological Aspects 17
G. Measurement and Biological Aspects of Radioactivity 18
Appendices
1. Magnetic Moment for Closed-shell-plus-one Nuclei 19
2. Electric Quadrupole Moment 21
3. Mass Correction for Neutron Excess 22
Problems 24

CHAPTER II. INTERACTION OF RADIATION WITH MATTER 27

A. Energy Loss by Charged Particles 27
  1. Introduction 27
  2. Bohr formula 27
  3. Electrons 30
  4. Other particles 31
  5. Other absorbers 31
  6. Range 31
  7. Polarization Effects 32
  8. Nature of equation for -dE/dx 32
  9. Ionization of a gas 33
  10. Radiation 34
B. Scattering 34
  1. Classical calculation for single scattering 34
  2. Multiple scattering 36
C. Passage of Electromagnetic Radiation through Matter 38
  1. Photoelectric absorption 38
  2. Compton scattering 40
  (3. Radiation loss by fast electrons) 43
  4. Pair formation 47
  5. Cosmic ray showers 49
  6. Summary 49
Appendices
1, 2, and 3. Multiple scattering 51
4. Momentum and pair creation 54
References 54

CHAPTER III. ALPHA EMISSION

A. Rectangular Barrier 55
B. Barrier of Arbitrary Shape 56
C. Application of Barriers to α-decay 58
D. Virtual Level Theory of α-decay 59
E. α-ray Spectra 66
Appendix 67

vii

Figure 5a.
Table of contents of Fermi's Nuclear Physics edited by Orear, Rosenfeld, and Schluter. Published by the University of Chicago Press.

CHAPTER IV. BETA-DECAY page 69

A. Introduction 69
B. Examples of β-processes 69
C. Energy diagrams 71
D. Theory of β-decay 72
E. Rate of Decay 73
F. Shape of Energy and Momentum Spectra 78
G. Experimental Verification 79
H. Selection Rules 80
J. Fτ Tables 82
K. Remarks on K-capture 84
L. Remarks on the Neutrino Hypothesis 84
M. Neutrinos and Anti-neutrinos 85

CHAPTER V. GAMMA-DECAY 89
A. Spontaneous Emission 89
General emission formula 90
Electric dipole emission 94
Magnetic dipole emission 95
Half lives 96
B. Selection Rules 96
1. Angular Momentum 96
2. Parity 98
3. Improbability of nuclear dipole radiation 99
4. Summary 100
5. Dipole absorptionat high energies 100
C. Internal Conversion 101
1. Theory of internal conversion 101
2. Selection rules 104
3. Other processes 105
4. Experimental determination of conversion coeff. 105
D. Isomeric States 106
Problems 106

CHAPTER VI. NUCLEAR FORCES 111
A. Introduction 111
1. Meson Theory 111
2. Saturation of nuclear forces 111
3. Exchange forces 112
B. The Deuteron 113
1. Non-central and spin-dependent forces 113
2. Ground state of the deuteron 115
C. Neutron-Proton Scattering 117
1. Method of partial waves 117
2. Low-energy solution for σ 119
3. Virtual state of the deuteron 120
4. Evidence for exchange forces 121
D. Proton-Proton Forces 123
1. Pauli principle complications 123
2. Spin functions 123
3. Coulomb scattering 125
E. Neutron-Neutron Forces 129

CHAPTER VII MESONS

A. Experimental Properties 131
B. Theory 133
References for meson theory 137
Problems 138

viii

Figure 5b.
Table of contents of Fermi's Nuclear Physics edited by Orear, Rosenfeld, and Schluter. Published by the University of Chicago Press.

CHAPTER VIII. NUCLEAR REACTIONS

A. Notation	page 141
B. Cross Sections, General	141
C. Inverse Processes	141
D. Compound Nucleus	141
E. Example of an Unstable Nucleus ($_4Be^8$)	149
F. Resonances; Breit-Wigner Formula	152
G. Resonances; Data	157
H. Statistical Nuclear Gas Model	159
J. Fission	164
K. Orbit Model of the Nucleus	167
L. Capture of Slow Neutrons by Hydrogen	171
M. Photonuclear Reactions	175
N. Remarks on Very High Energy Phenomena	177
CHAPTER IX. NEUTRONS	179
A. Neutron Sources	179
1. Radioactive sources	179
2. Photo-sources	179
3. Artificial sources	180
B. Slowing Down of Neutrons	181
1. Inelastic	181
2. Elastic	181
3. Energy distribution of neutrons from a mono-energetic source	183
4. Distance from a point source vs. energy	185
C. Diffusion Theory	187
1. Age Equation	187
2. Distribution of thermal neutrons	191
D. Scattering of Neutrons	194
1. Effect of chemical binding	194
2. Low energy scattering	194
3. Interference phenomena	196
4. Para- and ortho-hydrogen	199
5. Crystalline diffraction	200
6. Index of refraction	201
7. Scattering by microcrystals	203
8. Polarization of neutron beams	204
E. Theory of Chain Reactions	208
CHAPTER X. COSMIC RAYS	215
A. Primary Radiation	215
B. Secondary Radiation	217
1. Protons	220
2. Neutrons	220
3. Mesons	221a
4. Electronic Component	221a
C. Analysis into Hard and Soft Component	221 b
D. Motion in the Earth's Magnetic Field	225
1. Trajectories	225
2. Illustration: Equatorial Plane, Shadow Effect	230
3. Intensity: Liouville theorem	232
4. Charge of Primary Radiation	233
5. Latitude Effect	233
REFERENCES	239
NOTATION	240
PHYSICAL CONSTANTS AND VALUES	242
INDEX	244

ix

Figure 5c.
Table of contents of Fermi's Nuclear Physics edited by Orear, Rosenfeld, and Schluter. Published by the University of Chicago Press.

In the following year Fermi taught nuclear physics for the first time at Chicago. I had been studying and working problems with classmates such as Art Rosenfeld and Bob Schluter. We had a system of refining our classroom notes together, and we reasoned that with a little extra effort we could type them on mimeograph stencil sheet masters and make our class notes on nuclear physics available to the entire department. All three of us had training in touch-typing. The department chairman liked our proposal and offered to pay for the materials and we would in turn provide free labor. During the first few days of my struggles with the messy stencil sheets, my father suggested that we switch from mimeograph to photo-offset. Then it would be especially easier to make the many drawings and mathematical symbols. My father recommended a firm in Michigan that charged almost the same as the mimeograph process (wholesale cost per bound book $1.50). At that time I and my co-editors had not been aware that photo-offset would be comparable in price to mimeograph.

Whenever we got stuck we usually consulted T. D. Lee or Frank Yang. Only when their response was not satisfactory did we consult Fermi. As one might expect, in those rare cases when Lee and Yang could not understand a part of the lecture, then neither did Fermi. Fermi's office door was always wide open and any stranger or friend was always welcome to enter (as long as he or she observed the no smoking sign on his desk). As an example, once when I was in his office, Val Telegdi freely entered to ask Fermi to settle a dispute with the editor of the *Physical Review*. (I was shocked to see that Val didn't bother to extinguish his ever-present cigarette.) Telegdi had used the term "I-spin" in place of the usual expression "isotopic spin." In spite of the cigarette, I quietly agreed with Val's reasoning, but Fermi did not think it was worth the effort. However, Fermi had succeeded in getting the physics community to replace pi meson and mu meson and K-meson with pion and muon and kaon. Fermi had also tried without success to get the community to replace "center-of-mass system" and "laboratory system" with "barsy" and "labsy." I now suspect this is why Fermi felt that Val's proposal would not catch on.

Many have remarked on how simple Fermi made things *seem* in his lectures. But then after the lecture it was not so simple to reconstruct his reasoning. I do not blame this on any over-simplifying on the part of Fermi. It is because understanding of physics requires many successive steps of not too obvious reasoning. For this reason Art, Bob, and I would occupy a nearby empty classroom immediately following each Fermi lecture and try to make sure that we each really understood the lecture we had just heard. It usually took us more than an hour to convince ourselves that we had understood the one-hour lecture.

When we made the choice to switch over to the easier and superior system of photo-offset, we were not aware of another very powerful advantage: now the number of copies could be unlimited rather than restricted to about 500 (the lifetime of a wax stencil). We were still thinking that 500 copies would be more than adequate, but it quickly became clear that, literally, the whole scientific world wanted copies of these Fermi lecture notes. No nuclear physics book of this breadth or talent had yet appeared on the market. Not only was this a book on nuclear physics, it contained indispensable quantum mechanics such as the "Golden Rule Number Two," which I could not find in any introductory quantum mechanics text. Even Fermi was not aware that this rule was not to be found in the quantum mechanics textbooks. At the Cornell symposium Lincoln Wolfenstein told the story that one of the students in this nuclear physics class asked Fermi where he could find a reference. Fermi answered, "any quantum mechanics textbook." The student then asked, "Which one?" Fermi paused for a moment and then said, "Rojansky." Wolfenstein then pointed out to the symposium that it is not in Rojansky. Remember, Fermi was in his early twenties when quantum mechanics was being "invented," and he did some of the inventing himself (such as quantum electrodynamics). I have spent more time on this story to emphasize that for Fermi to understand a new topic he had heard about, he felt he had to independently invent the subject for himself without the help of published derivations. In this sense he may have independently invented nearly all of quantum mechanics by himself including quantum electrodynamics. Quite often Fermi's personal notes contained new topics that he did not realize were new, and often his derivations were clearer and better than the original published ones.

Fermi's contract with the University of Chicago required that any outside money he earned must be given to the university. So we all agreed that the distribution and sales should now be delegated to the University of Chicago Press. They paid me $333.34 and Rosenfeld and Schluter $333.33 each for our services. As Telegdi has pointed out, this way of teaching the whole world is just one of the ways Fermi has left his mark on almost all physicists in the world. Teaching of the Fermi approach was not restricted to the West. The University of Chicago Press edition was copyrighted 1950, but a Russian-language edition appeared in 1951 that was in violation of international copyright agreements. It was in widespread circulation in Eastern bloc nations. In fact, a radical Chicago student who went to a Moscow peace conference in 1951 brought back a copy for me.

Chapter 6
Other Examples of the Fermi Approach

This chapter is not terribly important for the story I am trying to tell and may be skipped by those in a hurry.

I. My Introductory Textbook

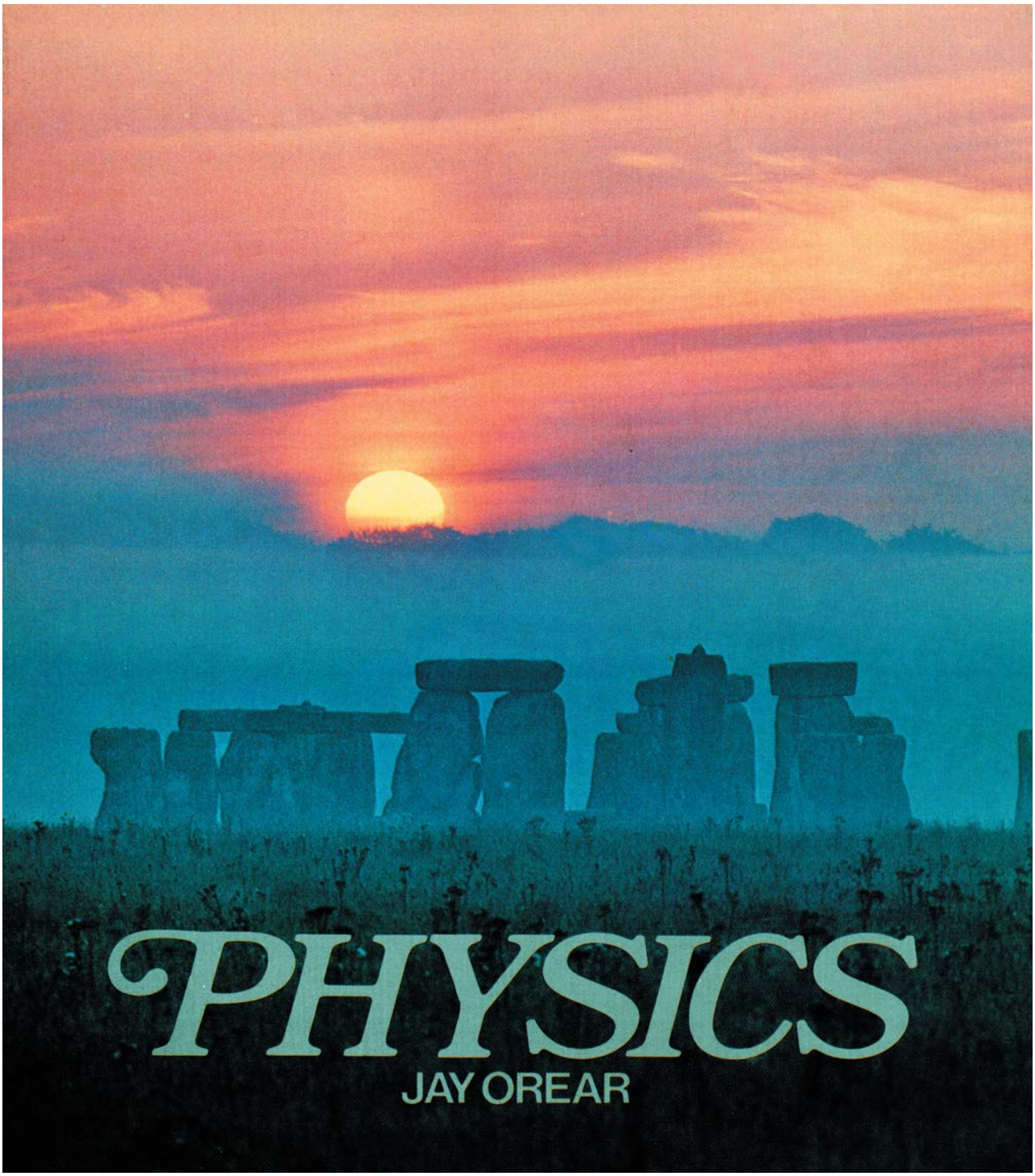


Figure 6.
Cover of Orear's textbook on introductory college physics.

As explained in the previous chapter, the Fermi book by Orear, Rosenfeld, and Schluter brought Fermi's approach to physics to the entire world. In this chapter I will describe two other "books" that I hope had a similar effect on the world audience. A Fermi-style book that reached an even larger audience was an introductory college textbook titled *Fundamental Physics* written by me and published in 1961 by John Wiley & Sons. My attempt to achieve the Fermi style was explained in the preface:

> My greatest debt is to Enrico Fermi, who not only taught me much of the physics I know, but also how to approach it. As a teacher, Fermi was well known for his great ability to make the most difficult topics seem beautifully simple in a clear, direct way with little mathematics, but much physical insight. The goal I have been aiming at is to try to present the spirit and excitement of physics in the way that Fermi might have done.

This time I was paid royalties in roubles for the Russian edition, which numbered 300,000 copies for the first printing. A fellow Cornell professor, Phil Morrison, had warned me that they would take out 25 percent income tax and then pay me with roubles wrapped in roubles. Phil was right. Taking the advice of a person in the U.S. embassy, I deposited this pile of roubles in a secret Moscow bank account (in this respect they were more capitalist than American banks). This bank on Gorky Street only had record of my bankbook number. They did not know my name or physical appearance. I could lend my bankbook to friends and they could and did make legal withdrawals. This first college physics textbook used no calculus, but eight years later I wrote a second, more advanced version that did teach calculus along with the physics.

II. Statistics for Physicists and the Tau Meson

I have one more personal example of how Fermi left his mark on the entire international physics community. In the early 1950s most physicists, including me, were not very knowledgeable about statistical inference. Let me take time to give one of many examples. I was at an American Physical Society meeting in the late 1950s and the speaker had fit a curve to more than 100 data points. I think he was trying to determine the mass of a new particle. So he tried different values of the mass until the least squares sum of the deviations of the experimental points from the curve was minimized. (This is called the method of least squares.) According to statistical theory if this experiment were repeated the average least squares sum for this situation would be 100, the number of data points. I don't remember the precise value of the least squares sum for this experiment—let us say it was 110. Now to find the error of that mass determination, the speaker had changed the value of the mass until the least squares sum increased by 1 (from 110 to 111). Wolfgang Panofsky from the audience objected and said because of the large number of data points the speaker should have used an increase considerably larger than one. But Panofsky was wrong and the young speaker was correct. Panofsky was a brilliant physicist, but his knowledge of statistics was typical for the times. I have no reason to pick on Panofsky. I could have just as well told a similar story about my brilliant friend Jack Steinberger. Fermi is the only physicist I knew who in the early 1950s would have been smart enough to increase the least squares sum by just one. The young physicist at the APS meeting who knew the correct method had learned it from me and I had learned it from Fermi.

In my thesis I had to find the best 3-parameter fit to my data and also the errors of those parameters to get the contribution of these new data to the pion-proton phase shifts and their errors. Fermi showed me a simple analytical method. At the same time, other physicists were using and publishing other cumbersome methods such as Monte Carlo on how to obtain combined statistical errors. I suggested to Fermi that he teach statistics not just to me but also to a few other students who had similar interests. So he met several times with Art Rosenfeld, Frank Solmitz, George Backus, and me. Backus was a math grad student. Fermi taught us a general method, which he called Bayes Theorem, where one could easily derive the best-fit parameters and their errors as a special case of the maximum likelihood method (which was derived from his Bayes Theorem). I remember asking Fermi how and where he learned this. I expected him to answer R. A. Fischer or some textbook on mathematical statistics. Instead he said, "perhaps it was Gauss." I suspect he was embarrassed to admit that he had derived it all from his Bayes Theorem. One piece of evidence that he derived much of it on his own is that at a later time Fermi suggested a "hybrid" method to use for the analysis in my paper on the signs of the pion-proton phase shifts. Fermi had suggested treating each small-angle event separately and lumping the more abundant larger-angle events into bins. I was not able to find this method in any statistics textbook. The Bayes Theorem of Fermi was "The ratio of the probabilities of two hypotheses is the ratio of the probability of a given experiment turning out the way it did assuming the first hypothesis to the probability of it turning out the way it did assuming the other hypothesis." (This is, of course, assuming that the starting ratio is 1 to 1.) It is my opinion that Fermi's statement of Bayes Theorem is not the same as that of the professional mathematicians but that Fermi's version is nonetheless simple and powerful. Just as Fermi would invent much of physics independent of others, so would he invent mathematics.

Frank Solmitz and I felt we should get down on paper the statistics we were learning from Fermi. So with help from the notes I took at those Fermi meetings on statistics and a Frank Solmitz report on the least squares method, I was able to pull all this together a few years later in a 1958 UCRL report, "Notes on Statistics for Physicists." It was of comparable popularity (in terms of copies printed) to the "Nuclear Physics" of Orear, Rosenfeld, and Schluter and was distributed at no cost to requests from any country regardless of its politics. This work was done during the summer of 1958 while I was a guest of the University of California Radiation Lab at the invitation of Luis Alvarez and Art Rosenfeld. One of the reasons for my invitation was to get me to teach a summer course on Fermi's approach to mathematical statistics.

Even in 1956 while I was at Columbia University, an MIT paper claimed incorrectly that the 3 pion final state of the kaon had spin 1 (at that time called the tau meson). The theorist Richard Dalitz had worked out how the experimental pion decay energy and angle distributions should depend on the spin of the tau meson. But using the very same MIT events and the maximum likelihood method (which automatically assigned the correct statistical weight event by event) I obtained spin zero. And by adding in more plentiful data of my own I got a likelihood ratio of $10^{12}/1$ favoring spin zero to spin 1. It became a friendly MIT versus Columbia battle, but within a few months the entire physics community (including MIT) understood and endorsed the maximum likelihood method as had been taught to us by Fermi. For example, when I presented the 10^{12} figure in a colloquium at Princeton, Robert Oppenheimer from the audience made the comment "Young man, I recommend that you never play the horses." In spite of the audience laughter, I took this as an endorsement of my methodology. (I am still not sure—I sometimes have trouble fully understanding Oppenheimer's remarks.) The friendly MIT professor was Dave Ritson. We had exchanged our data and resolved the disagreement as all good scientists should in such a situation. This was a very important finding because it was the first good experimental evidence for nonconservation of parity and in my opinion was of Nobel Prize caliber.

Later in the 1990s when the SSC was under construction, my group was approved to measure the total proton-proton cross section using a method of our design. A year or so later, Dave Ritson had designed a new and better way, so we joined forces and worked together on it. It would have been one of the first experiments to take data. Unfortunately construction of the SSC was stopped by Congress.

Now back to the problem of the tau meson. I (and then others) also made measurements to show that both this 3 pion decay mode and the 2 pion decay mode had the same lifetime as well as mass. So now the world was faced with a serious problem: the 3 pion decay mode of the kaon was odd parity and the 2 pion decay mode was even parity. We called this the tau-theta puzzle. It was the first solid evidence for the nonconservation of parity. At that time it hinged on convincing the physics community to use the more powerful statistical methods of Fermi.

My statistics notes based on Fermi were revised in 1982 as a Cornell preprint. Counting both editions, thousands of copies were distributed all over the world at no cost to scientists living on either side of the Iron Curtain.

NOTES ON STATISTICS FOR PHYSICISTS, REVISED

Contents

Preface 1

1. Direct Probability 2
2. Inverse Probability 2
3. Likelihood Ratios 3
4. Maximum-Likelihood Method 5
5. Gaussian Distributions 7
6. Maximum-Likelihood Error, One Parameter 8
7. Maximum-Likelihood Errors, M-Parameters, Correlated Errors 11
8. Propagation of Errors, the Error Matrix 17
9. Systematic Errors 19
10. Uniqueness of Maximum-Likelihood Solution 20
11. Confidence Intervals and Their Arbitrariness 22
12. Binomial Distribution 23
13. Poisson Distribution 25
14. Generalized Maximum-Likelihood Method 27
15. Least-Squares Method 30
16. Goodness of Fit, the χ^2-Distribution 37

Appendix I: Prediction of Likelihood Ratios 41

Appendix II: Distribution of the Least-Squares Sum 42

Appendix III: Least Squares with Errors in Both Variables 44

Appendix IV: Maximum Likelihood and Least Squares Solutions by Numerical Methods 45

Appendix V: Cumulative Gaussian and Chi-Squared Distributions 49

Figure 7.
Table of contents of Notes on Statistics for Physicists

Figure 8.
Cornell photo of Dick Feynman and Bob Wilson. Dick had received the Nobel Prize in physics for the correct theory of the Electro-weak Interaction, and Bob had become designer, builder, and first director of what is still the world's highest-energy accelerator at Fermilab.

The initial discovery of the tau meson around 1950 was a lucky break. It was a cosmic ray particle that had entered and stopped in a nuclear emulsion. This was several years before there existed accelerators of high enough energy to produce it. It was as if nature had given us a free head start of several years. By measuring grain density and multiple scattering versus residual range its rest mass was determined to be about 500 MeV. But it was unstable and decayed into three identical particles, one of which also stopped in the emulsion, and that particle was a foolproof pion because it had a telltale pi-mu-e decay. This was a very lucky break and the entire event was vastly overdetermined to an accuracy in mass of about 1 percent. Can just one event like this be foolproof evidence of a whole family of new unstable particles? Even though many physicists said "you can't trust statistics of just one event," Fermi and I agreed that this was foolproof evidence of a new particle, later called the 3 pion decay mode of the K^+ meson. Many other phenomena in physics are of absolute certainty, such as the earth is "round" and not flat; the earth goes around the sun and not the sun around the earth; each hydrogen atom contains a proton and an electron; mirror symmetry is violated by the weak interaction. These are facts true and certain. But many scientists and philosophers claim that no physics statement can be certain. For example, the *Encyclopedia Americana* states, "it is characteristic of science, however, that no hypothesis is presumed to be so certain as never to be subject to possible revision or rejection. In this sense, *the whole of science may be regarded as hypothetical*."

Also, Dick Feynman states on page 27 of his book, *The Meaning of It All*:

> So what we call scientific knowledge today is a body of statements of varying degrees of certainty. Some of them are most unsure; some of them are nearly sure; but *none is absolutely sure*.

Dick is too good to make such a mistake. I call it a mistake because it has misled other thinkers. Perhaps Dick was making a fine distinction between "scientific statement" and "scientific fact." If so, he should have said so. It is true that a true hypothesis cannot be proven true and that theoretical physics deals with hypotheses (and other things like scientific facts). Experimental physics on the other hand deals with experiments that determine facts of nature and that exact repetitions of an experiment always give the same results, i.e., a certainty. Fermi did not ignore the *certainty* of experimental facts such as the existence of a meson that decays into three pions even if it was based on just one foolproof event.

Chapter 7
My Ph.D. Thesis

The Chicago Physics Department required a lengthy written exam for Ph.D. candidacy. It lasted eight hours a day for four days in a row. Even one of Fermi's future Nobel Prize–winning students failed it on his first try. As soon as I learned that I had passed, I asked Fermi to take me on. He agreed but assigned me one more task: to work for a month or so under the direction of Dick Garwin on the fast coincidence circuit he was designing. To me, Garwin was another Fermi: they had both been friendly and helpful to me and to my classmates. So I was thrilled with Fermi's "requirement" that I first work for Garwin before working for Fermi. Fermi didn't realize this because he then said to me, "Even though Garwin is younger than you, he is the only true genius I have ever met." As a check that Fermi really did say this, I have found an interview with Marvin Goldberger in the May 15, 1981, issue of *Science* in which Goldberger said, "Fermi declared Garwin to be 'the only true genius I have ever met.' " And that, says Goldberger, was an accolade "since Fermi was not one to praise others very freely." (Garwin is shown in Figures 3 and 30.)

Garwin really was quicker at solving "Fermi problems" in his head than was Fermi. My guess is that neither really thought of himself as a scientific genius. Both are capable of making occasional mistakes. We all liked to play with trick questions. It was not often that I could trap Fermi. Once I succeeded by asking him would the direction of motion of his famous "trolley car" reverse if the cyclotron field were reversed? After a pause he said, "yes, because of symmetry." Of course, once I told him he was wrong, he gave it a minute or so more thought until he was in agreement with me.

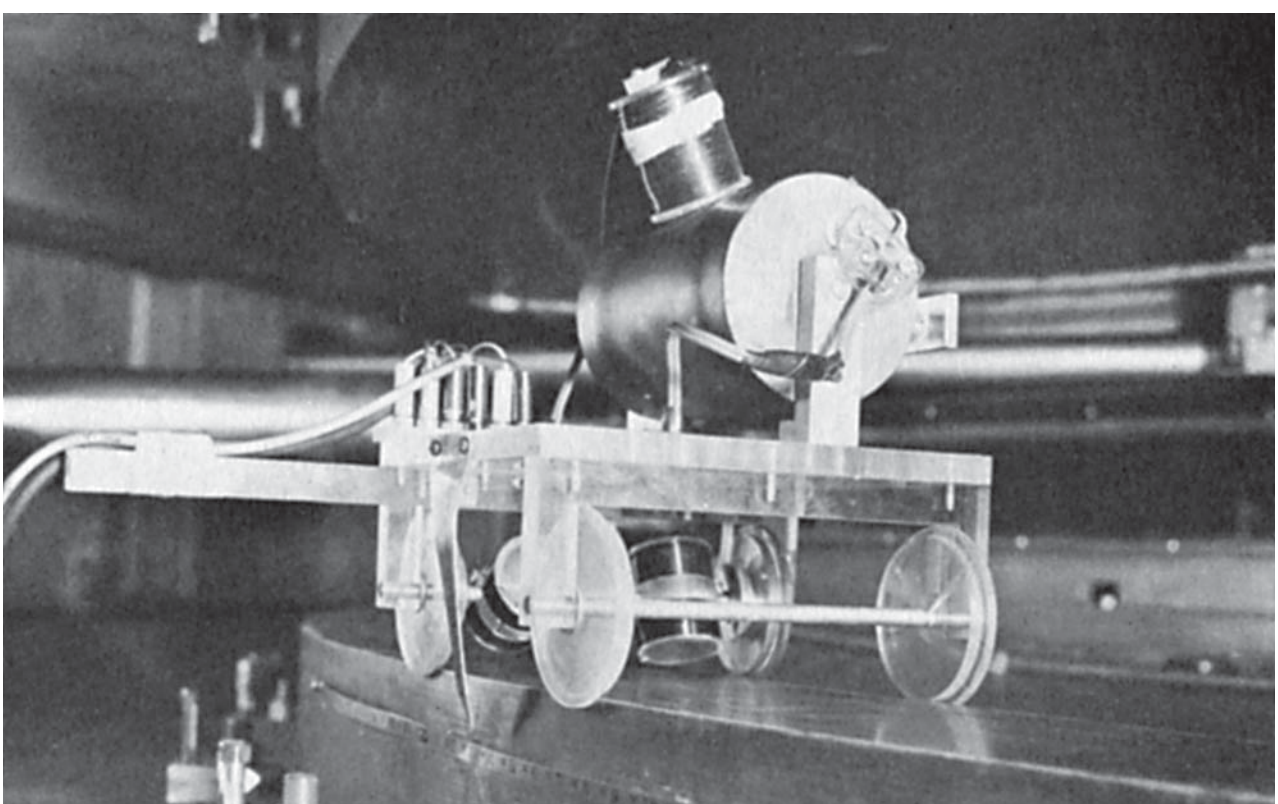


Figure 9.
Fermi's famous trolley car. It held the target of the Chicago cyclotron. The two-dimensional position and energy dissipation were measured remotely. It was designed and constructed by Fermi himself.

I remember at one of the Thursday afternoon INS seminars Fermi made a not-too-obvious blunder. He must have sensed it from the looks on some of our faces. He then quickly saw his mistake and seriously scolded all of us for not immediately correcting him. (I was one of those who had made a face and as his grad student I felt extra responsibility to alert Fermi of said blunder.) Another responsibility he gave me was to read the *Physical Review* and tell him whenever there was a paper I thought he should see. He told me that usually he did not read the literature but that he did learn of most discoveries before they were published by means of telephone, mail, personal visits, and preprints. He knew I was always at my desk next to his office when he arrived by bicycle at 8:27 each morning. (He never did ask how much earlier I actually arrived—and it was not much.) We would "check base" as he came in to see whether either of us had learned anything "new." Fermi also told me where and when he did most of his work. He told me he had a kind of insomnia where he had no trouble getting to sleep; he usually went to bed by 9:00 or 10:00 p.m. But he nearly always awakened by 4:00 a.m. and would be unable to get back to sleep. Then he would get up and go to his desk at home and there is where he did most of his creative work. (This famous desk is now in the home of his granddaughter Rachel Fermi in Los Angeles.) Then he would have breakfast with his family about three hours later. At the university he spent most of his time consulting with others, teaching, doing administration, etc. One fringe benefit of this mode of operation was that I was sometimes the first one in the world to learn of his latest discovery—like when he used the known spin-orbit interaction of the outer shell nucleon to explain the left-right asymmetry in proton-nuclear elastic scattering that had been recently measured.

The first thesis topic he had for me was to irradiate some separated isotopes on the Chicago 100 MeV Betatron to discover possible new daughter isotopes. They were in powder form and the induced radioactivity could most efficiently be measured by putting the same up against an end-window Geiger counter. Fermi warned me that end-window counters could misbehave as compared to cylindrical Geiger counters. So I was careful to use brand new end-window counters that did not misbehave. I reasoned to myself that in the 20 years since Fermi did his neutron irradiations, the manufacturers had learned how to make more reliable end-window counters. I expressed this feeling to Fermi but he did not seem completely convinced.

Occasionally Fermi would go into my counting room and stare at my scalers that were tracking new long-lived isotopes. Finally, after a few weeks he found that I was getting some nonrandom counts and he gave me further warnings. I then looked at the pulses on a scope and found that some of them were now giving after-pulses and thus counting too high. (Previously there had been no after-pulses.) So my long lifetime results could not be relied upon. I certainly had not expected a detector to change its characteristics so quickly and dramatically. One lesson I learned was that when Fermi takes the trouble to give a new grad student some advice, the student should pay special attention. Another mistake I had made in some Betatron work involved a stockroom sample of graphite. In my calculations I used the Handbook value of density. I had repeated a measurement by Leona Marshall and gotten an answer different from her. I guess Enrico expected me to recheck my work and resolve the discrepancy. So I measured the density of my graphite sample and was shocked to find that its density was 10 percent less than the book value. In this case he let me learn by doing. But in the case of the end-window counters he gave additional help.

During that period I was also working with Art Rosenfeld on how to process thick nuclear emulsions that were sensitive to minimum ionization tracks. Nuclear emulsions had become a very powerful new tool by that time, and it was important that our institute have a first-rate processing and scanning facility. So now I proposed a new project to Fermi: namely, to redo his group's pion-proton elastic scattering experiment by a new technique of area scanning that I had worked out. Fermi and Anderson had used scintillation counters in their experiments. Fermi, as usual, gave full support to Art and me. This experiment worked and I was able to confirm the Fermi-Anderson result. Then after my Ph.D. I was able to extend my techniques further and go to lower energies and smaller angles than could be reached by scintillation counters. The smaller angles permitted me to see smaller-angle pion-proton elastic scatterings than could be seen using scintillation counters. I was able to do a better job on the s-wave phase shifts than the counter experiments.

I was even able to see events in the coulomb-nuclear interference region and thus obtain the signs of the pion-proton phase shifts. Those like Herb Anderson who were trying to do the same experiment with scintillation counters had to give up. In July 1954 I sent a first draft of my paper to Fermi, who was teaching at the Varenna summer school at Lake Como, Italy. He liked my paper and especially the use of the powerful event-by-event statistical analysis we learned from him. (I strongly suspect he had invented this new method just for my experiment. It would have been typical of him to make such an important contribution without claiming credit for it.) In the return mail he urged me to send it in for publication. In the last paragraph he mentioned he was having trouble swallowing. This was the first symptom of the cancer that cost him his life four months later. This letter from Fermi may have been our first indication that Enrico was having a serious health problem.

One of the duties of a thesis adviser is to teach the student how to do good scientific writing. Fermi took this responsibility seriously. He made me go through four or five different drafts. He took special delight each time he found a spelling or grammar mistake. One thing he taught me was to be overly generous about giving references to other people in the same field. In an early draft I had intentionally left out reference to a worker in a nearby university. I knew that person's work well enough to judge that it was not worth reporting. Fermi taught me that what I had done was a no-no. But also Fermi had taught me the correct methods for statistical combination of measurements of the same quantity even when that quantity was one of several in different kinds of experiments. One could get wrong results if one included results when the errors had been underestimated by one of several experiments. Actually in those days (the early 1950s) one of Herb Anderson's students, Hank Stadler, had commented to me that in his opinion most papers had overestimated their errors. Then the experimental points lay closer to the predicted curve than they should and that almost all the points included the curve within their error brackets. In today's language we would say that chi-squared per degree of freedom was much smaller than the expected value of 1. We now know that it should not be much smaller than 1-sqr(2/n) where n is the number of degrees of freedom. In 1950 the old-timers (even Fermi) joked that one should multiply one's errors by pi. Then one's curves looked less jagged or more "beautiful." However, in my *Notes on Statistics for Physicists*, I strongly urged that everyone should publish the statistically best estimates for the errors without any alterations in either direction contrary to the suggestions of the old-timers. Then one could safely combine results of all relevant experiments. By 1960 this advice had caught on.

Fermi took his rule seriously that one should not leave out any reference. I had showed him a case where a postdoc of Hans Bethe had published a solution for the low-energy phase shifts in pion-proton elastic scattering and ignored my more comprehensive solution that had been published earlier in an Italian journal. I rarely observed anger from Fermi, but this time he uttered something along the lines that the president of the American Physical Society (Bethe) should know better than this. I suggested that Bethe probably wasn't even aware of the obscure publication in question, but Fermi insisted he was going to complain to Bethe anyway. There may be no connection, but four years later I received an offer of associate professor with tenure from Bethe and Wilson at Cornell.

Fermi's nuclear emulsion group was used mainly to support the Ph.D. theses of Orear and Rosenfeld. But also the facility was used by Fermi to study two problems of interest close to him. We had arranged a thesis project for Horace Taft that would repeat a recent experiment from Columbia University led by Leon Lederman that tended to rule out the p-wave resonance in pion-proton elastic scattering. This is discussed in more detail in Chapter 9. The other project designed by Fermi probably would have been a thesis for Bob Swanson or Bill Slater. It was to be a study of secondaries produced in high-energy pion-nuclear collisions with nuclear emulsion nuclei using a new high-energy pion beam at the Brookhaven Cosmotron. Fermi was anxious to see how well his new statistical model of secondary production would work and whether some of the newly discovered cosmic ray particles could be produced at the Cosmotron. This experiment required exposing a "solid" nuclear emulsion stack in a high-energy external pion beam at the Cosmotron and building a special microscope with a motor-driven, track-following stage. Normally he would have sent Art or me to Brookhaven to make the exposure, but the in-house nuclear emulsion group at Brookhaven was not very cooperative in sharing the new beam they had worked so hard to create. (On the other hand, part of their job description was to provide service to visiting scientists.) Fermi reasoned that they could not turn him down if he asked to make an exposure himself. (In this instance he was knowingly taking advantage of his high position in the physics community.) To eliminate background tracks it was necessary to assemble the stack in the Brookhaven darkroom just before the exposure and then disassemble it just after the exposure. Art and I trained Fermi how to do this in our Chicago darkroom and, as expected, he did a good job as an experimental physicist in making the Brookhaven exposure.

Fermi also did most of the design of the special scanning microscope. The x and z coordinates were operated by the left and right hand, while a variable-speed motor connected to the y-coordinate (beam direction) was foot controlled. It took about 30 cm of track following per inelastic interaction. Hundreds of interactions were needed; however, data collection by this method turned out to be slow. Shortly after Fermi's death, newly developed bubble chambers turned out to be the best method to collect this kind of data.

Here is one last anecdote of the grad student days where Fermi was treated as one of the gang. In those days the University of Chicago neighborhood was not as safe as one would like. Even Fermi's son, Giulio, had been attacked by ruffians. And so had my brother. Art Rosenfeld and I had done some "research" on tear-gas guns that were disguised as fountain pens. We discussed things like that with Fermi, and he agreed that a surprise object that could incapacitate the attacker would be of some advantage. So we ordered three such kits by mail for us. As soon as they arrived, Fermi took his into his personal machine shop and he modified the trigger mechanism so it would have a quicker "hair-trigger" response. Art and I were satisfied with the original design and we had some worries that Fermi's modification might some day backfire in his pocket. (To my knowledge it never did.)

At this point I should comment on Fermi's private machine shop. I believe just after the war he had competing offers from Chicago and other universities. But Chicago showed him blueprints of the new Institute for Nuclear Studies with a personal machine shop adjoining his office. The people at Chicago must have known that one of Fermi's dearest wishes was to have his own private machine shop. Next door was a darkroom and lab rooms for him and his students. Even though Fermi had always wanted his own private power tools, he freely let his grad students use them. Dick Garwin, like Fermi, wanted full access to a machine shop, and this shop was of advantage to him also. He did object that Fermi did not have a milling machine, but by the time I was using Fermi's power tools, there was a nice one. Also the machine shop had one wall lined with glass cabinets that were filled with a good collection of preprints and reprints. He also shared these with his grad students. His system was to alphabetize them according to author. In the case of multiple authors he would underline the person he knew the best and use that name for the alphabetization. Not too many years later, that system would not work very well because most high-energy experimental papers listed a large number of co-authors. Some of the present-day experiments list 400 co-authors! Fermi kept his famous notebooks at home. These were 6″x 9″ with bound and numbered pages. Each notebook had an index at the end. He had one set for the courses he had taught and another set as sort of a daily scientific diary.

In that same period I had found a mail order company that sold inexpensive dosimeters complete with battery chargers. I told Fermi that I felt a personal dosimeter was more important than a personal fallout shelter. The shelter could do harm if it contained some undiscovered radiation after an attack. And I had lived through the Bikini atom bomb tests where I returned to a target ship with some remnant radiation zones that could be found with the help of my personal survey meter. I offered to include an extra kit for Fermi in my order and he agreed with my reasoning and gladly joined in on the order. At times like that we thought of him as a fellow grad student. Not only was he fascinated with new "gadgets" just as we were, but he really treated us as equals.

Fermi held a meeting in his office every Tuesday night to which his students and others were invited. He shared with us in these meetings his latest news received via phone calls and other communications. I remember one warm early spring evening when he showed up with Laura to cancel our meeting so that he and Laura could go for a walk in the warm spring air. None of us resented this—we felt they had made a wise choice. It really was a remarkably beautiful evening. My guess is that it was Laura's idea.

Every few weeks our nuclear emulsion group held meetings. Fermi came to about half of them, and members of the Marcel Schein cosmic ray group and Jim Cronin attended some of them. One of our running debates was just how many different strange particles there were. Many of the particles lighter than the proton had different charges, decay modes, and masses. Fermi and I independently felt that there was only one such strange meson and that some of the mass measurements must have been in error. It did turn out that most of the cloud chamber mass measurements had larger errors than originally quoted.

Chapter 8
Fermi Intuition

To me, intuition is a kind of mental telepathy, and mental telepathy is supernatural, i.e., by definition it is "outside of nature" and does not exist. So now let me give you some examples of Fermi's famous "intuition" (or whatever). About one or so months after the Berkeley Bevatron had been running on both electronic and nuclear emulsion antiproton searches, not one antiproton had been seen. Also no antiprotons had been seen in cosmic rays. Murray Gell-Mann had just returned from Berkeley with these negative results, which he was relating to Fermi and me in the hall just outside our office doors. Murray said, "Now we know there is no antiproton." Then Fermi said in a very definitive and loud manner, "There *is* an antiproton." We grad students used to joke, "Fermi had an inside track to God." And sure enough, within a month of that definitive pronouncement, the first antiprotons were discovered at the Bevatron. The group leaders of the electronic experiment at Berkeley (Chamberlain and Segré) were former grad students of Fermi.

Another example was his explanation of the large number of newly discovered V-particles in cloud chambers and nuclear emulsions. Fermi used to joke that that there are more new names than there are new particles or letters of the alphabet. But this intuitive conclusion was based on some knowledge. The measured masses of a group of these particles differed by just a few standard deviations. Both Fermi and I independently felt that God would not have created so many new bosons of almost the same mass. The simpler explanation was that these observations were different decay modes of the same particle and that some of the mass measurements must have had larger errors than claimed. Fermi supervised the Chicago nuclear emulsion group, and he and his students knew that nuclear emulsions could determine masses more accurately than the cloud chambers. We guessed that the cloud chambers must have had larger errors than those that were quoted. As it finally turned out, all these different bosons were just the three different charge states and antiparticle states and different decay modes of the same particle, the K-meson or kaon.

In 1953 Fermi taught a particle physics course. I sat in on the course and took detailed notes. My notes reveal two more examples of what might be called intuition. On my pages dated April 11, 1953, Fermi explains the odd intrinsic parity of the pion as two spin 1/2 sub-particles in an L=1 orbit around each other and with the intrinsic spins and orbital angular momenta opposed to give a total spin zero for the composite particle. A spatial inversion would give a change in sign or odd parity for the composite. This is the present quark model of the pion years ahead of its time. On my pages of the same date Fermi gets even intrinsic parity for the neutrino in one reaction and odd intrinsic parity in another reaction, so two different parities for the same neutrino. (Actually this is now known to be true!) I asked Fermi in class, "Suppose there is an antineutrino in the other reaction?" He said, "Let me think about that." Later that day he called me into his office and said that he still had the problem that he got both parities for the neutrino. He admitted that he still did not understand the neutrino. I like to speculate that if he had known about the two-component neutrino in the *Pauli Notes*, he might have beaten Lee and Yang by three years.

The most famous story about Fermi's so-called intuition has to do with his Nobel Prize–winning discovery of how slow neutrons can produce much larger amounts of artificial radioactivity. It is undisputed that he was the first to slow down a beam of neutrons with a slab of paraffin. But there is at this time a dispute whether he first tried a lead filter with no result and then followed it with paraffin resulting in a hundredfold increase in the induced radioactivity. On one side of the dispute is a famous quotation by Chandrasekhar as presented in Segré's book. Chandrasekhar had told Segré that Fermi told him in a conversation about the scientific method: "When finally, with some reluctance, I was going to put it [the lead filter] in its place, I said to myself; 'No, I do not want this piece of lead here. What I want is a piece of paraffin.' It was just like that with no advance warning, no conscious prior reasoning." This is one of the reasons why we students would joke about Fermi having an inside track to God. Segré, who was in another room at that time, doesn't seem to remember that detail, but he cannot trust his memory. Chandrasekhar admits he did not write down verbatim what Fermi said to him but he feels he can trust his memory.

On the other hand, Laura Fermi in her *Atoms in the Family* tells a different story. On page 98 she writes, "They placed the neutron source outside the cylinder and interposed objects between them. A plate of lead made the activity increase slightly. Lead is a heavy substance. Fermi said, 'Let's try a light one *next*, for instance, paraffin.' " Laura Fermi's book was proofread by her husband. Too bad that Enrico Fermi, Laura Fermi, Segré, Pontecorvo, or Chandrasekhar are no longer available to help settle this dispute. Even so it would be risky to trust detailed recollections of anyone going back two decades in time. At the end of this paragraph Gerry Holton, a science historian, points out it is safer to make use of the written data taken on the same day as the event itself. I can think of several reasons that support Laura Fermi's version: (1) the Chandrasekhar version is admittedly not verbatim, (2) the Laura Fermi version is verbatim (she was writing a book while interviewing her husband and her husband did proofread her entire book), (3) if Fermi at the last minute had changed their agreed-upon logical plan without any warning to Segré, Segré might have been annoyed and have a reason for remembering something so out of Fermi's character. At the Rome congress honoring Fermi's one hundredth birthday it seemed most of the audience was on the side of Chandrasekhar. However, a compromise theory was proposed that the lead experiment was done the day before the paraffin was "impulsively" selected. This seems to be supported by the original data book that lists one page for the lead data and the *following* page for the paraffin data. Gerald Holton at the 2001 Rome Congress showed the two consecutive pages of Fermi's data book. To further complicate the matter, the dates at the top of the two pages ran backward in time (a handwriting error?). Whether or not the lead was used, the fact that paraffin was selected either first or second is a good example of remarkable intuition on the part of Fermi. This dispute may be over a minor detail, but it strengthens our case if we all are in complete agreement. I can think of only one other dispute and that is raised in the talk by Nella Fermi. I think it is adequately handled in the appendix to her talk. So I conclude that the entire science community has come to common conclusions about the life, the personality, and the role played by Enrico Fermi.

Rather than joking about an inside track to God, I prefer to say that Fermi was highly skilled at making educated guesses and reasoning by analogy. In the above example, Fermi knew from experience that light elements behaved differently from heavier elements and that uranium behavior was in a class by itself.

My last example is a case where Fermi's remarkable "intuition" failed him. He himself later referred to it as his "great mistake." When his Rome group irradiated uranium with neutrons in 1934 they observed more than the usual amount of radioactivity. They had expected to get radioactivity from transuranic elements. But their chemistry and halflives didn't fit in a pattern that would enable them to prove that is what they saw. A German chemist, Ida Nodnick, actually published in 1934 that what they saw was nuclear fission, which seemed too wild an idea at that time. Apparently her idea was too wild to be taken seriously. According to Segré, Fermi's knowledge of nuclear energy states was such as to make him think fission was not possible. He knew that the penetration barrier was too high for a fission product to escape from a spherical nucleus. The liquid drop model of the nucleus would permit nonspherical nuclei and thus smaller penetration barriers, but it had not yet been invented. Fermi never wanted to publish an experimental result unless he was sure of it. If Fermi had published that he had seen fission, the half-sized pieces would have an excess of neutrons and these neutrons could give rise to more fissions most likely in a chain reaction. Then both Germany and the United States might have had atom bombs in time for World War II. The world should be grateful for this one mistake by Fermi!

I like to make the following analogy between the two great Italian navigators, each of whom had made an earthshaking discovery. The first in 1492 found an enormous new world, but he thought it was China; the second navigator in 1934 found fission, but he thought perhaps it was just transuranic isotopes. Fermi did feel bad about making such a historically great mistake. T. D. Lee tells the story that when he came from China to America to select the best grad school in physics, he interviewed with Fermi during his Chicago visit. Fermi showed him the blueprints of the Institute of Nuclear Studies then under construction. In the blueprint was a figure of a man standing by the front door. Fermi pointed to the man and said to Lee, "That is the man who made the great mistake." At the time of that interview Lee was a teenage scholarship winner from China asking to be excused from most of the lower-level graduate courses. I would guess that Fermi could sense the latent talents of these two teenagers, Lee and Garwin. Within a mere 10 years they and co-workers would deliver the theoretical and experimental discoveries of the nonconservation of parity.

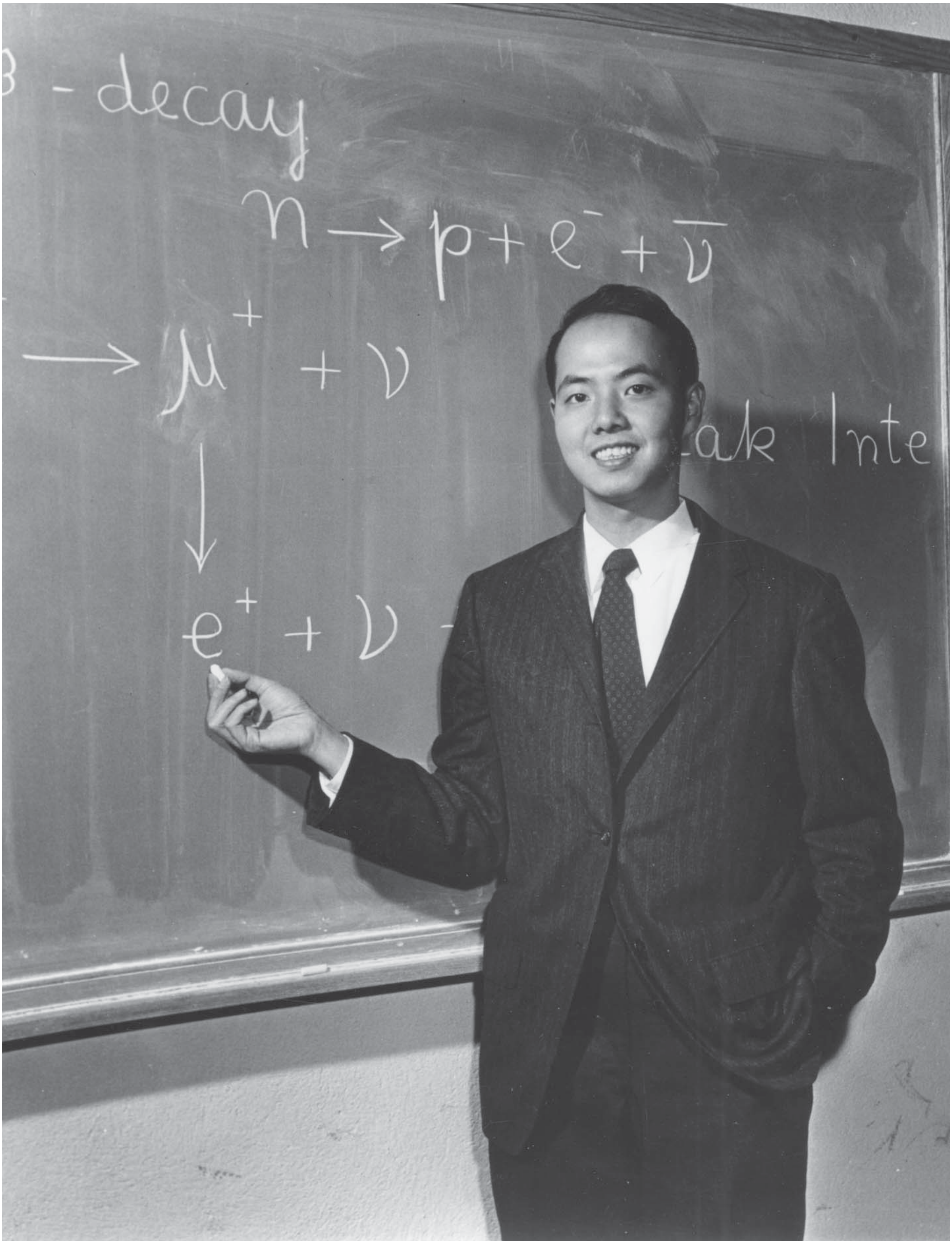


Figure 10.
Photo of T. D. Lee. He and a fellow student, Frank Yang, were the first to show that parity conservation was not needed to explain beta decay. This was an "earthshaking" prediction that proved to be true and earned them a Nobel Prize in physics. I found it peculiar that the Nobel committee never rewarded a prize for the experimental confirmation of the theoretical speculation about nonconservation of parity.

Chapter 9
Fermi Humor

Hans Bethe in his talk at the Cornell Symposium gave an example of Fermi's humor when Fermi was age 29. I consider this example comparable to Feynman's prank of opening up top-secret safes at Los Alamos and leaving cryptic notes for the security personnel. Not only was Fermi a full professor at such a young age, he was also the youngest member of the Royal Academy with the title of "His Excellency Fermi." The driveway to the Physics Institute also led to an important governmental department that sometimes had "classified" meetings, and on such occasions the driveway was closed to the physics people. On one of those days Fermi came driving and when the guards stopped him he said, "I am the driver to His Excellency Fermi and His Excellency would be very annoyed if you didn't let me in." And as he told the story later, Fermi emphasized that he had told the whole truth: he *was* the driver to the Excellency Fermi, and indeed His Excellency *would* have been very annoyed. (And the guards did let him in.)

Feynman was a more accomplished comic and artist than was Fermi. As an amateur musician Feynman even won a bongo drum contest competing against professional Brazilians in Rio. He was interested in and spent some time exploring art, music, and philosophy. Fermi felt he had little talents in the artistic world, but he did spend time in physical activities such as tennis, skiing, swimming, mountain climbing, ice-skating, and square dancing. The Feynman jokes may have been more polished and clever, but the Fermi jokes had a freshness about them. Usually there was more laughter in a Feynman lecture than a Fermi lecture. As we shall see later in this chapter, the Fermi lectures also had good laughter and no one could ever accuse Fermi of being dull. To illustrate their differences in style I will compare a famous Fermi quotation with one of Feynman. The invitation to the UCLA Fermi birthday symposium leads off with the Fermi quote: "Whatever nature has in store for mankind, unpleasant as it may be, men must accept, for ignorance is never better than knowledge." Fermi is pointing out the advantages of truth.

On this same subject, Feynman adds the idea of beauty contained in truth:

> We have been led to imagine all sorts of things infinitely more marvelous than the imaginings of poets and dreamers of the past. It shows that the imagination of nature is far, far greater than the imagination of man. For instance, how much more remarkable it is for us all to be stuck—half of us upside down—by a mysterious attraction, to a spinning ball that has been swinging in space for billions of years, than to be carried on the back of an elephant supported on a tortoise swimming in a bottomless sea. The same thrill, the same awe and mystery, come again and again when we look at any problem deeply enough. With more knowledge comes deeper, more wonderful mystery, luring on one to penetrate deeper still. Never concerned that the answer may prove disappointing, but with pleasure and confidence we turn over each new stone to find unimagined strangeness leading on to more wonderful questions and mysteries—certainly a grand adventure!

Feynman is more lyrical and perhaps doesn't fully realize that *he* is a great poet and dreamer of the present age. To me the lectures of both Feynman and Fermi contain a deep, inner beauty as well as humor. We are fortunate to have been exposed to the lectures of both of these great teachers. It is to Fermi's credit that his goal was to teach every physics course offered at Chicago starting with freshman physics.

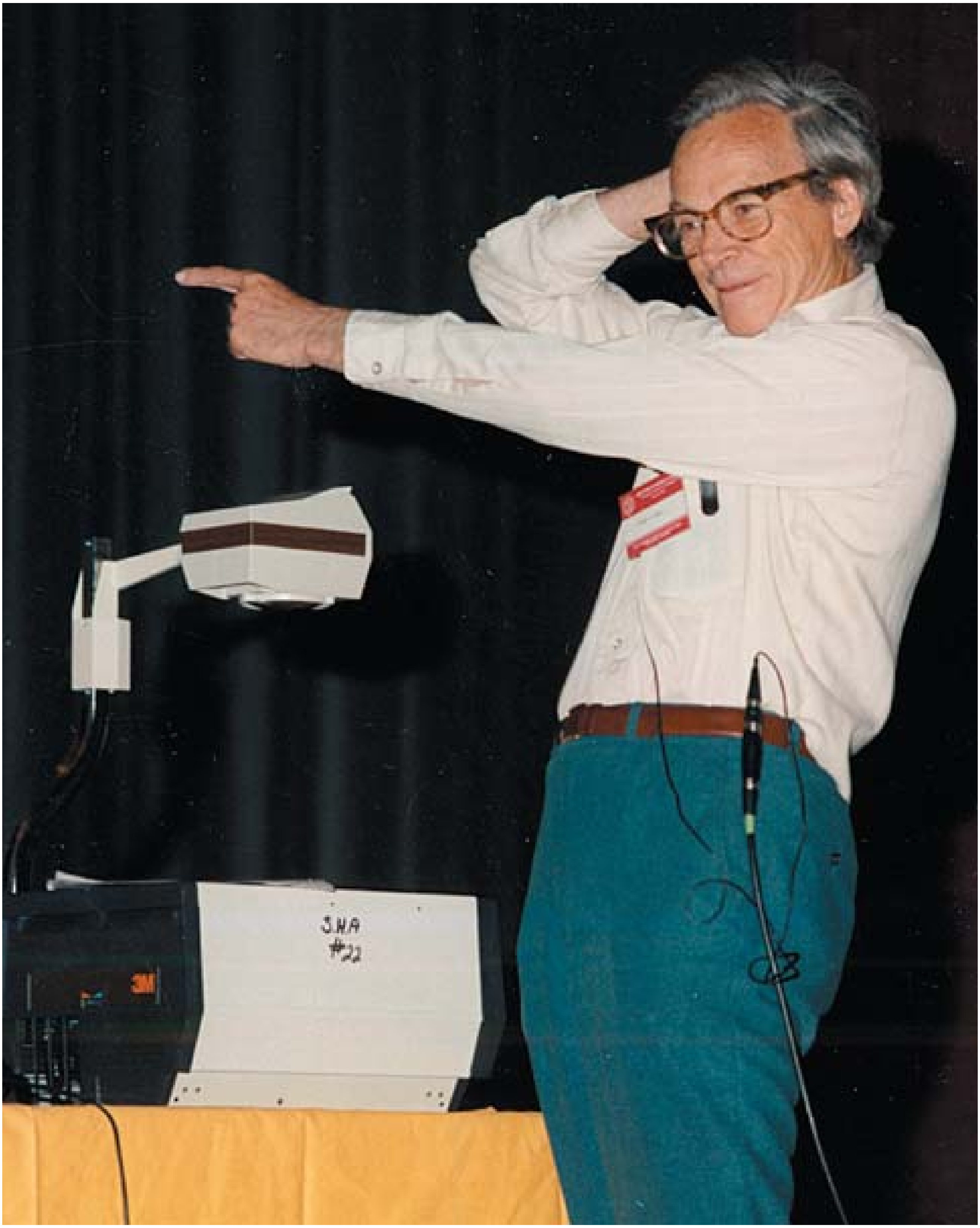


Figure 11.
Dick Feyman lecturing at Cornell.

JAN. 29, 1954

FRIDAY AFTERNOON AT 2:00

McMillin Theatre

(H. A. BETHE AND P. E. KLOPSTEG presiding)

Joint Ceremonial Session of the APS and the AAPT

Retiring Presidential Address of the American Physical Society

P1. What Can We Learn with High-Energy Accelerators? ENRICO FERMI, *University of Chicago.*

Presentation of the Oersted Medal of the AAPT

Response of the Oersted Medallist

P2. The Metaphysics of a Physics Teacher. C. N. WALL, *University of Minnesota.*

Twelfth Richtmyer Memorial Lecture of the AAPT

P3. Fields and Particles. J. A. WHEELER, *Princeton University.*

JAN. 30, 1954

SATURDAY MORNING AT 10:00

McMillin Theatre

(H. A. BETHE presiding)

Physics at Columbia University: a Programme in Celebration of the Bicentennial of the University

Q1. Physics at Columbia University: the Early Years. G. B. PEGRAM, *Columbia University.* (30 min.)
Q2. Physics at Columbia University: the Genesis of the Nuclear-Energy Project. ENRICO FERMI, *University of Chicago.* (40 min.)

Figure 12.
Official announcements in the January 1954 Bulletin of the American Physical Society of Fermi's last two APS lectures.

In his later life Fermi chose to inject quite a bit of humor into his retirement lecture as president of the American Physical Society (APS) on January 29, 1954. The announcement and title are shown in Figure 12. On the next morning Fermi gave a second lecture in honor of the two hundredth anniversary of Columbia University that he also sprinkled with humor. Both of these lectures give a good idea of his personality and style of humor. Unfortunately no audio or visual recording exists for the first, but the entire second lecture exists on audiotape and is transcribed both in *Physics Today* and Segré's book. Neither transcription indicates audience laughter. Fortunately I was given a copy of the tape by the audiovisual department of Argonne National Lab, and I discovered much laughter of both Fermi and his audience on the tape.

(1) The First Lecture: *The Ultimate Accelerator*

This is the unofficial title we physicists gave to the retiring presidential lecture. Fortunately Fermi typed out one page of notes for it with his own hands (he did know how to type). Parts of this one page are discussed below. We shall see that he does plan jokes days in advance, and from the second lecture where we can hear both Fermi laughter and audience laughter we note that he laughs heartily at his own jokes. As far as I can tell, the style of humor and delivery shown in these documents are just as I remember and to me they give some feeling of his humble, friendly, and cheerful personality. My comments are in italics. Words in boldface are from his typewritten notes. The gaps of several dots each are the same as on his typed page.

The first sentence of Fermi's page of notes reads, **"Congratulate Society on Loosing mediocre President and getting eccellent one."** *Spelling has not been corrected—the two spelling errors are typical of Fermi. This first joke is one of self-deprecation.*

Next sentence: **"Counting number of papers. . .most active branches . . .solid state physics in which, perhaps mistakenly, we believe . . .nuclear Physics in which we cannot make that mistake. Since Yukawa. . .first suspected and then known. . . ."** *As the father of both solid-state physics and meson physics Fermi can get away with criticizing them.*

Now he explains his criticism of particle physics: **"But, to our dismay we got a lot more. . . .many so called elementary particles. . .and because in addition. . .each. . .many names. . .number of names. . .stupendously great. . .even more than the number. . .which large enough."** *He finds it humorous that there are even more names than there are particles.* **"But to solve the mysteries higher energy data are needed. But cosmic rays above 25 BeV only one per cm^2 at an inconvenient location. For these reasons. . .clamoring for higher and higher. . . ."**

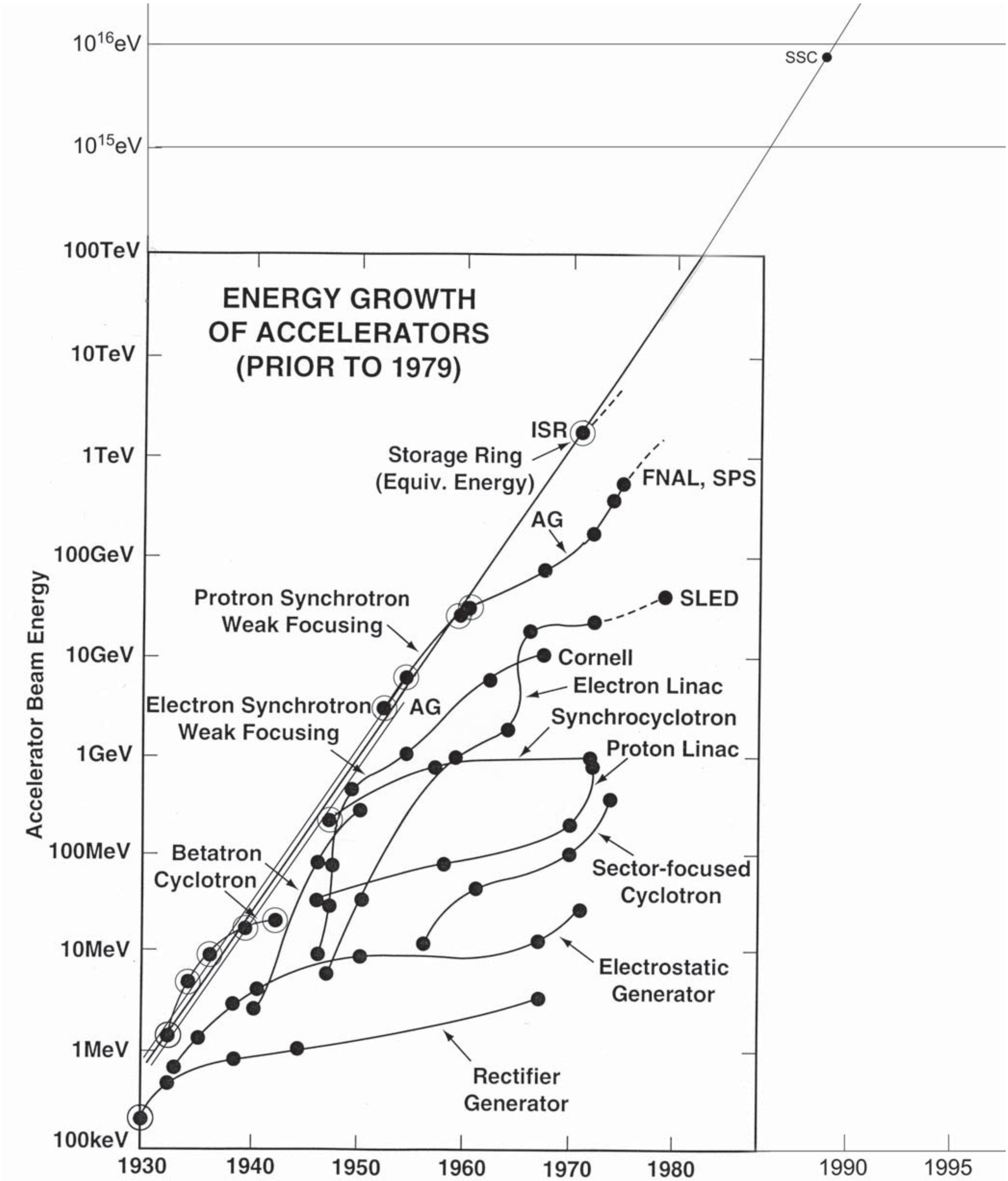


Figure 13.
Semi-log plot of beam energy versus t where t is the first year of accelerator operation at that energy. Each of the highest energy points is circled. A triple line is drawn through the highest proton energies obtained up to 1954. The accelerators after 1954 are also shown and they still tend to lie on the same line as determined by the pre-1954 accelerators. How did Fermi know that this exponential relationship would maintain itself for at least six more decades? Such remarkable intuition! Courtesy Cornell accelerator group.

Our Fig. 13 is similar to Fermi's Fig. 4, which was a similar plot of energy versus time (of the existing accelerators in 1954 showing extrapolation to 1994). Fermi's predicted value at 1994 is an energy of 5 x 10^9 BeV at a price of \$170 B. (Remarkably, this energy could have been built in 1994 and at a lower price of about \$11 B by using colliding beams. So far the highest-energy colliding beams were achieved at Fermi National Accelerator Laboratory in 1988 at an equivalent beam energy of 2x10^6 BeV.)

Next Fermi makes a preliminary design for a single ring proton accelerator of energy 5 x 10^9 BeV:

Figure 14.
Central laboratory building at Fermilab where the world's highest equivalent beam energy of ~2 x10^6 BeV was achieved in 1988.

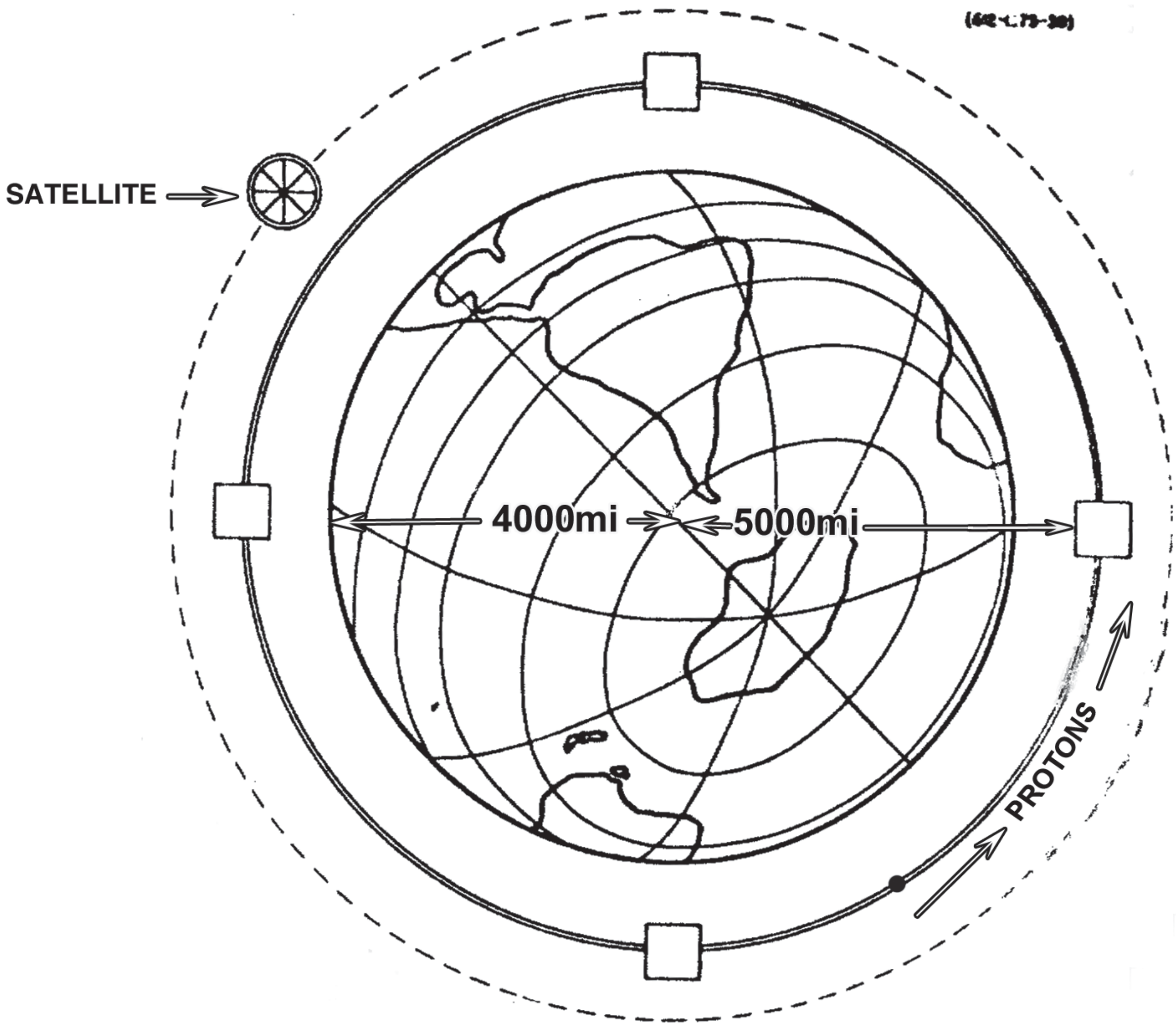


Figure 15.
Slide 3 of Fermi's talk on the ultimate accelerator. Strong focusing magnets are in earth orbit to confine a proton beam to a circle 1,000 miles above the surface of the earth.

"Preliminary design…8000 km, 20,000 gauss" *Such a single ring would give the desired energy, but the radius of 8,000 km or 5,000 mi would put the orbit 1,000 mi above the surface of the earth! This is shown in Fermi's Slide 3 as our Figure 15. The entire ring magnet would be in orbit around the earth. By now the audience must have been in hysterics. They were still talking about it when they came back to Chicago.* **"What we can learn impossible to guess. . .main element surprise. . .some things look for but see others** *(this is the same as what Feynman was saying but Feynman was more poetical about it)*. . . .**Look for multiple production. . .antinucleons. . .strange particles. . .puzzle of long lifetimes. . .large angular momentum?. . .double formation?** *(now called associated production)* **At present more probable… ."**

Fermi's intuition was working well: this energy was close to being achieved in 1994. It is incredible that he could make such an accurate prediction 40 years into the future! A colliding beam version could have been built well under his estimated cost, but Congress ruled that the cost of approximately $10 B was too much. Fermi was correct in predicting that the main element would be fantastic surprises (like strangeness, charm, and bottom and top quantum numbers, charged and neutral intermediate bosons of high mass, heavy leptons, electro-weak unification, the six quarks and three different kind of leptons and neutrinos, nonzero mass of neutrinos, QCD, the fantastic success of the standard model, nonconservation of parity, etc). So many new surprises in 40 years! However, preference for "double formation," which is now called conservation of strangeness, was also correct. I feel that Fermi was very close to solving the puzzle of long lifetimes for strongly produced particles.

". . . .tried to photograph what I saw in the ball. . .and made slide. Slide 5—Strange particles in pion nucleon collisions. . .should realize this picture retouched. . . ." (End of Fermi's one page of lecture notes.)

His Slide 5 (a cloudy crystal ball) must be Fermi's last joke in this talk. Unfortunately I was not able to find it among his papers. But I seem to remember him showing a slide in Chicago that might meet this description. It was a sphere containing strange-looking little animals.

(2) The Second Lecture: *Physics at Columbia in the 1940s*

The following excerpts contain eight of the 20 or so jokes in this talk that are on tape. One can hear Fermi as well as the audience laughing while telling the joke. Sometimes he starts laughing before reaching the end of the joke. Fermi's words are in boldface.

(1) **I don't know how many of you know Szilard; no doubt many of you do. He is certainly a very peculiar man, extremely intelligent.** (*laughter*)

(2) **I see that this is an understatement.** (*laughter*) **He is extremely brilliant and he seems somewhat to enjoy, at least that is the impression he gives to me, he seems to enjoy startling people.**

(3) **And in fact help came along to the tune of $6,000 a few months after and the $6,000 were used in order to buy huge amounts—or what seemed at that time when the eye of physicists had not yet been distorted—**(*laughter*) **what seemed at that time a huge amount of graphite.**

(4) **So physicists on the 7th floor of Pupin Laboratories started looking like coal miners** (*laughter*) **and the wives to whom these physicists came back tired at night were wondering what was happening.**

(5) **We know that there is smoke in the air, but after all . . .** (*laughter*)

(6) **It was the first time when apparatus in physics, and these graphite columns were apparatus, was so big that you could climb on top of it—and you had to climb on top of it. Well cyclotrons were the same way too, but anyway that was the first time when I started climbing on top of my equipment because it was just too tall—I'm not a tall man.** *(laughter)*

(7) **Now graphite is a black substance, as you probably know. So is uranium oxide. And to handle many tons of both makes people very black. In fact it requires even strong people. And so, well we were reasonably strong, but I mean we were, after all, thinkers** *(laughter).*

(8) **So Dean Pegram again looked around and said that seems to be a job a little bit beyond your feeble strength, but there is a football squad at Columbia** *(laughter)* **that contains a dozen or so of very husky boys who take jobs by the hour just to carry them through college. Why don't you hire them?**

The film *The World of Enrico Fermi* illustrates how the workers by the end of the day turned from white to black. There is a scene of Fermi wearing goggles and stripped to the waist machining a block of graphite and creating a black cloud that rises up and hits him in the face. It was typical of Fermi to participate in all phases of an experiment—even the dirty parts. It is easy to understand why his machinists especially praised him.

Chapter 10
The Excited State of the Proton

One last example of good intuition is whether Fermi believed in the Fermi-Metropolis phase shifts as defined in paper 260 of the *Collected Papers of E. Fermi*, Vol. II, U. of Chicago Press, 1965. In the paper delivered by Val Telegdi at the Cornell symposium, Telegdi says the Fermi-Metropolis fit "favored by Fermi did not correspond to the proposed resonance." What Telegdi should have said is that "the world data at that time favored the Fermi-Metropolis phase shifts but Fermi instead favored the resonance fit." It is true that the full set of world data at that time taken together gave a better goodness-of-fit to the Fermi-Metropolis solution than to the solution where the p-wave phase shift went through a resonance. But it was this resonance fit that Fermi personally *always* favored. In an earlier talk I remember Herb Anderson making a statement similar to Telegdi's. These statements might cause readers to rule out Fermi as the discoverer of the first excited state of the nucleon (and equally important, that protons and neutrons could have excited states which suggests subnuclear particles like quarks). Telegdi and Anderson should have said that in the Fermi-Metropolis paper the Fermi-Metropolis solution gives a better goodness-of-fit value than the resonance solution. One must keep in mind that Fermi and Metropolis were doing a fit to the *combined* world data. At that time the resonance solution fit every combination of world data until the first "measurement" of the pi plus-proton total cross section was claimed by Columbia University. They reported a total cross section considerably smaller than required by a p-wave resonance. They had exposed nuclear emulsion to positive pions close to the resonance energy at a position near the center of the Nevis cyclotron. It was a difficult experiment because of the heavy background and the scanning efficiency for finding all the elastic scatterings is expected to be low. Fermi and I felt all along that the scanning efficiency must have been lower than what the Columbia scanners had estimated. If the Columbia data could have been properly corrected for this, then the Fermi-Metropolis fit would be ruled out. (Later experiments at the Cosmotron using external pi plus beams at and beyond the resonance energy proved that the Columbia cross section was way too low.) Fermi was so confident that there was a resonance that he tried to repeat the Columbia experiment using the Chicago cyclotron with Horace Taft as the grad student in charge and with Fermi in charge of experimental design. In this kind of configuration Fermi's philosophy was that "no shielding is the best shielding." This involved mounting some nuclear emulsions and their supporting structures near the center of the vacuum tank where residual radiation levels were significant. Members of our nuclear emulsion group took turns working short shifts inside the tank. Of course we wore film badges and dosimeters and made sure no one was exposed to more than the approved limit of 300 mr per week. Fermi as a member of the group insisted on taking this same dosage as did Taft, Orear, Rosenfeld, and others. (The others may have been Bob Swanson, Bill Slater, Elliot Silverstein, and Jerry Friedman.) Several times I pointed out to Fermi that he already had accumulated much more lifetime dosage than we and that we preferred that he not crawl inside the cyclotron as we were doing. But he was an egalitarian and he felt very strongly about this and he was our boss. (Nobody had any hint that he would die from cancer in the following year. In Chapter 26, Nella Fermi reports that his physician claimed that Fermi's cancer was not produced by radiation.) We did find some elastic scatterings in our exposures, but we also found heavy background that would swamp out the signal at the needed exposure levels. So we were unable to disprove the Columbia experiment as the Cosmotron did shortly after Fermi died.

As a check on my memory of Fermi's beliefs, I sent the above opinion as an e-mail to Nick Metropolis in September 1991 and received the following reply: ". . .I have read the now ancient documents and they are consistent with what you plan to say. Trust you'll have a most successful conference, Yours sincerely, N. Metropolis." I feel that one can safely conclude that Fermi never did actually believe in what is known as the Fermi-Metropolis solution.

In making exposures inside the cyclotron that was underneath the control room, many transits of the long staircase were involved. Fermi always ran up and down the stairs and so did his students. I believe, true to his competitive reputation, that Fermi was the fastest. But I suspect that some of us were holding back to make Fermi feel that he was the winner.

I mentioned in Chapter 8 that the only way Fermi could explain odd parity for the pion was to speculate that the pion had a structure of two sub-particles. And the only way to explain excited states of the proton would be to invoke sub-particles in the proton. If Fermi had lived one or two more years my guess is that he would have solved the numerology of associated production of the strange particles (the law of conservation of strangeness). And then as soon as a few of the new bosons and hadrons were discovered, he could solve their quark numerology. I think he would have taken the 1/3 charge seriously, just as he had taken electron and neutrino production seriously in his theory of beta decay.

Chapter 11
Fermi and Strong Focusing

The following anecdote is a perfect example of Fermi's true greatness. Usually when new discoveries were made, Fermi was told about them before publication. But (*Phys. Rev.* 88: 1190, 1952) the theoretical discovery of strong focusing was described in the Letters section of the *Physical Review* delivered to our mailboxes that morning. I was the first one Fermi saw after he had read the letter of Courant, Livingston, and Snyder about using an alternate gradient principle for future proton and electron synchrotrons. Fermi told me we should organize a special high-energy physics seminar for that afternoon, and he asked me to give a talk on the letter and lead the discussion. I reminded him that I was not an expert on accelerator theory and I might not be the best choice for the job. He may have thought I was an accelerator expert because I organized and gave a lecture (based on preprints) to our graduate student physics club on the Cosmotron and Bevatron while they were still under construction. Both Fermi and Teller came to my talk "disguised" as students. They sat together. It was the only time any faculty had attended our student club. An hour or so later the morning of the special strong focusing seminar, Fermi told me that Courtenay Wright would be glad to take "my place" and I was relieved to be relieved.

A fairly good crowd showed up on such short notice, and when Fermi entered he was carrying a closed cardboard box. Wright gave a better talk than I could have done at that time and when he finished there was discussion, with Enrico rather than Courtenay leading the discussion and answering most of the questions. If the principle would really work, Fermi foresaw that all new high-energy accelerators would have much higher intensity at lower cost. It would make the proton synchrotrons at Princeton, Cambridge, and the proposed zero gradient accelerator at Argonne obsolete. Construction of the new 30 GeV proton accelerators at Brookhaven and CERN had not yet started and their design could be changed in time to incorporate strong focusing.

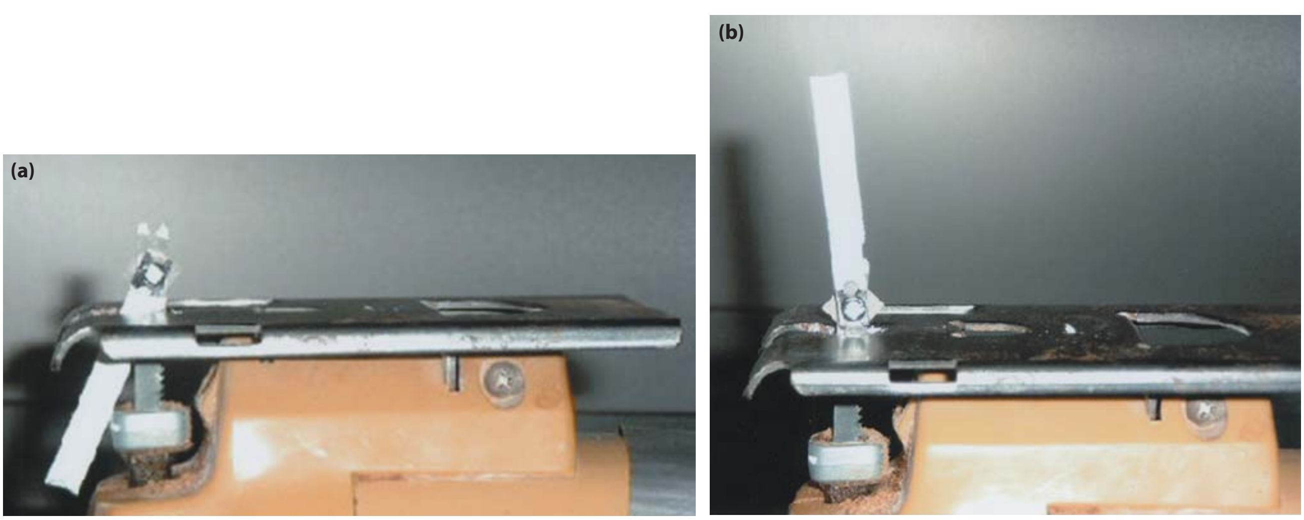


Figure 16.
A home hand-held jigsaw is mounted upside down. The blade painted white is fastened loosely with one screw. (a) Before turning on the motor the blade hangs down. (b) After turning on the motor the blade rises and the loose end stays on top as if the direction of gravity were reversed.

The problem now was to quickly build a prototype or physical model to see if the theory reported in the *Physical Review* would really work. *And in Fermi's cardboard box was the world's first prototype!* Within an hour or so he had secretly designed and constructed such a physical model in his own machine shop. He mounted an electric hand jigsaw upside down with the blade pointing up. Then the blade was loosened and held loosely by just one screw. Now the blade was hanging almost upside down. When he turned on the motor the blade quickly rotated about the loose screw until it settled upright above the screw as if gravity had been turned upside down! What seemed to be a weaker upward force was dominating over the stronger downward force! If the blade spent equal times in the regions of stronger and weaker force, it would end up pointing in the direction of the stronger force, but if it spent less time in the direction of stronger force, it could end up pointing in the direction of weaker force. Figure 16a shows my home jigsaw with the motor off and 16b is with the motor on. I consider this a good example to illustrate Fermi's true greatness. As soon as he read the abstract of that paper he must have figured the theory out for himself and in one hour built a working model that would help confirm the theory. He did both theory and experiment at the same time! This was his trademark. He could do top-quality theory, experiment, engineering, and machining all at the same time. No one else in that lecture room would have thought of using a jigsaw demonstration. And no one could have done a better job of teaching. And this in spite of the fact that there were several future Nobel Prize winners in that audience. After living through such experiences firsthand I am inclined to rate Fermi the best of the twentieth-century physicists. It took several months for Bob Wilson to build his prototype strong focusing model that was a complete 1 GeV working electron synchrotron. The accelerator people at Brookhaven were afraid to take the risk of changing their design until they had built a strong focusing electrostatic prototype.

But how did Fermi know that the jigsaw blade would point in the "wrong" direction? I never did ask him, but I can guess and my guesses can give the reader further insight into Fermi's approach to physics problems. I recently asked my colleagues at Cornell. One of them, Dick Talman, both an experimental and a theoretical accelerator physics expert, knew the answer (he had seen the same "jigsaw" problem solved in a mechanics textbook by Lev Landau). In his solution Landau, had give earlier Russian references. It may have been discussed by the Russian Chwolson in his 5,000-page textbook on "all" of physics, published in 1915. Fermi had mastered all 5,000 pages as a teenager and thus ended up knowing more physics than any of his contemporaries. Another explanation may be that he did not know how the jigsaw would behave, but his intuition told him the answer and during the time he calculated the result for alternating gradient magnets, he also calculated how a loose jigsaw blade would behave. He had a Marchant calculator on his desk and he was expert at doing numerical calculations.

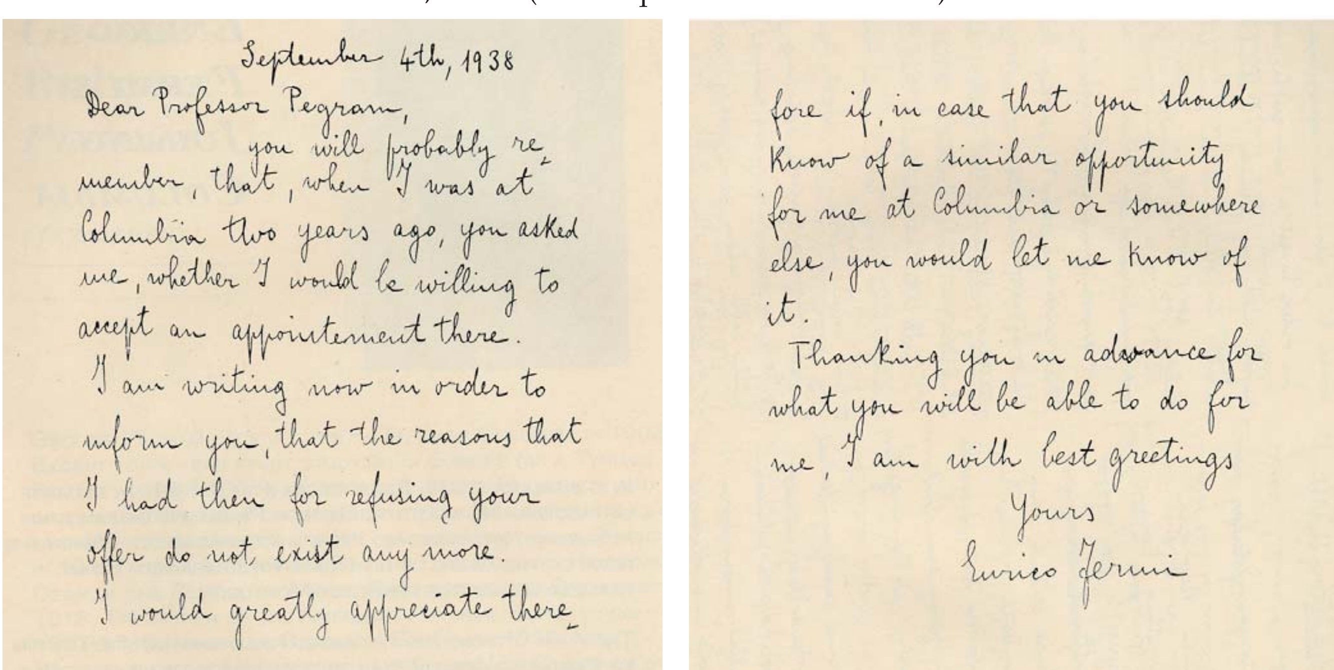
It turned out that there was an additional complication that even Fermi was not aware of. This had to do with resonance effects where each time a particle passed the same point in its orbit it could experience an accumulation of the same perturbation until it hit a magnet poleface. This problem was solved a few years later with careful tuning of the magnet lattice. The lesson is that any new theory should not be taken for granted without proper experimental confirmation.

Chapter 12
Fermi and Politics

Fermi's Response to Fascism

Fermi was very dedicated to science, but because of his high positions and his high intelligence and common sense, he encountered situations where he was virtually forced to take political stances. He feared that the more time he spent on political questions, the less time would be available for science. He was forced to deal with the evils of Italian fascism while he was still in Italy. Not to do so might result (and *did* result) in loss of life of some of his close relatives and friends. As you shall see, he did take definitive actions that may have saved some of these friends and relatives. T. D. Lee, in the Fermi talk he gave in August 2001 at Erice, dealt with this racism question and presented some material that was new to me. A photo of Lee is given in Figure 10. He suggested that some persons in the know tipped off Fermi well in advance that he would be receiving the 1939 Nobel Prize in physics. I do not know the truth of that statement. This was not the usual situation, and it may have been done surreptitiously to give Fermi an easy opportunity to escape the growing anti-Semitism in Italy at that time.

Lee has researched the files at Columbia University with the help of I. Tramm and found letters dealing with Fermi's Columbia appointment and his effort to find positions for his talented Jewish friends. Lee showed the relevant letters in his August 2001 talks at Erice. They are now printed in a booklet (with no date or copyright) titled *The Columbia Physics Department—A Brief History.* This booklet was distributed in October 2001 at the Columbia Fermi birthday celebration and at the department office. I would assume that anyone can obtain copies from the department office. Page 21 reads: "Enrico Fermi's Joining Columbia—In the fall of 1938, Fermi decided to leave Italy because of fascism. He wrote to George Pegram *(dean of the Graduate School and former Physics chairman)* regarding this possibility, and received every encouragement. When the news of the award of his Nobel Prize arrived, Fermi realized he had the perfect opportunity. The two letters (on the pages following) written by him to Pegram chronicle this critical period of his life. It is particularly touching to read of his concern for his fellow physicists in Italy, as expressed in the letter of Oct. 22, 1938." (End of quotation from booklet.)



September 4th, 1938

Dear Professor Pegram,

you will probably remember, that, when I was at Columbia two years ago, you asked me, whether I would be willing to accept an appointement there.

I am writing now in order to inform you, that the reasons that I had then for refusing your offer do not exist any more.

I would greatly appreciate therefore if, in case that you should know of a similar opportunity for me at Columbia or somewhere else, you would let me know of it.

Thanking you in advance for what you will be able to do for me, I am with best greetings

Yours

Enrico Fermi

Figure 17.
Handwritten letter dated September 4, 1938, of Fermi to George Pegram accepting an appointment in the Columbia University Physics Department.

The first letter, dated September 4, 1938, is shown in Figure 17. In this letter Fermi has changed his mind about accepting an offer from Pegram, but he doesn't give the reasons (at least not in writing). The letter of October 22 in which he is seeking positions for Segré, Rossi, Racah, Fano, Pincherle, and Rasetti is reproduced below:

> Dear Professor Pegram,
>
> I cabled to you yesterday as follows: <L.C. Pegram Columbia New York Accept Professorship writing Fermi>. I should like to express to you again my really very sincere thanks for your generous offer; and please extend my thanks also to Professor Butler (Columbia's president). I should like to come to New York, if possible, for the beginning of the spring term which starts, so far as I remember, at the end of February.
>
> For reasons that you can easily understand however, I should like to leave Italy, without giving the feeling that this is due to political reasons. I could manage this much more easily if you could write me officially to teach at Columbia through the Italian Embassy in the U.S. Of course you need no mention, or stress, in this request, that it would be a permanent appointment.
>
> In order to get a non-quota visa for myself and my family, I should need besides an official letter from Columbia stating that I am appointed as professor and mentioning the salary. In case that you cannot write me through the Embassy, please send me only this second letter. And in any case please *do not give unnecessary publicity to this matter* until the situation in Italy is finally settled.
>
> I shall take the opportunity that I am writing to you from Belgium, in order to give to you some information about the situation of the Italian physicists, that have lost their positions on account of racial reasons.
>
> They are Emilio Segré, whom you already know. He is now at Berkeley and has, so far as I know, a small research fellowship for one year from the University of California. I don't think that I need to inform you about his scientific work.
>
> Bruno Rossi, formerly professor at the University of Padova (married with no children; age about 32). He is one of our best young physicists, his work on the cosmic radiation is probably known to you. He has lately acquired some experience on high tension work, since he had built in Padova a one million volt Cockroft Walton outfit, that was just now being tested.
>
> Giulio Racah, formerly professor at Pisa (not married; age about 30). He has a very extensive knowledge of theoretical physics. Has published many papers on atomic physics and quantum theory; in particular he has obtained independently and published only a few days after Heitler and Bethe equivalent results on the theory of the emission of high energy gamma rays from cosmic ray electrons colliding against nuclei.
>
> Ugo Fano (age about 26; not married) was my assistant for theoretical physics. Good knowledge of theory; very great enthusiasm for research. Has been lately very much interested for theoretical problems in connection with biology. Had several discussions on these topics with Timofeeff-Ressowshi of Berlin and with P. Jordan of Rostock.
>
> Leo Pincherle, formerly lecturer of theoretical Physics at Padova (age about 30; married with 1 or 2 children). Has published rather interesting papers on intensity problems of x-ray lines. I might finally mention that Rassetti

> too, though not for racial reasons, is trying to find a situation abroad. He would also like to be invited for some course next summer.
>
> Please write to me to my home address Via L. Magalotti 15 - Roma, Italy. Looking forward to seeing you next winter, I am, with best greetings
>
> Enrico Fermi

In addition to the letters to Pegram, Fermi wrote three other letters inquiring about jobs for himself at other institutions. Laura Fermi in her 1975 paper delivered in Erice, Sicily, titled "My Life as a Physicist's Wife" (see Chapter 30 of this book), says, "Fermi wrote four letters to four American universities, in veiled terms, fearing censorship. But the Americans are smart. They took the hint, and Fermi got five invitations. He accepted the offer of Columbia University."

The Cobalt Bomb

My first political discussion with Fermi was in the form of a question asked by Art Rosenfeld and me. After the first H-bomb test the possibility of a cobalt bomb producing widespread radioactive contamination was rather obvious. We asked Fermi for his opinion on this and he spoke freely to us. He gave a response I did not expect. He said the military leaders would not rely on a weapon whose effects had never been tested and that the long-range air patterns are too unpredictable. Now that I am older and perhaps wiser, I agree with Fermi on this.

Fermi, Oppenheimer, and Teller

In his Cornell talk (see Chapter 22) Bob Wilson criticized the common opinion that Oppenheimer was more liberal than Fermi. (This is also in agreement with a statement in Segré's book.) Wilson gave the pending May-Johnson Bill on government control of atomic energy and research as an example. He, Fermi, and others felt that the May-Johnson Bill would permit too much government secrecy in fundamental research. Wilson said that Oppenheimer was for the May-Johnson Bill, but ultimately Fermi strongly opposed it and supported an alternate civilian control bill (the McMahon Bill). Please see Chapter 22 for the details. Fortunately there were enough liberals in Congress to defeat the May-Johnson Bill. And when Oppenheimer's security clearance was revoked, Fermi testified on his behalf before Congress. Behind the scenes, Fermi privately tried without success to persuade Edward Teller not to testify against Oppenheimer.

Fermi and the H-Bomb

Carl Sagan in his Cornell talk quoted a strong warning by Fermi not to make an H-bomb. Carl said,

> In the October 1949 report of the General Advisory Committee to the U.S. Atomic Energy Commission, there was an addendum by Enrico Fermi and Isadore Rabi. This was a report on whether it was a good idea to build the first thermonuclear weapon, and the main report, signed by Robert Oppenheimer and others said, "The extreme danger to mankind, inherent in the proposal by Edward Teller and others, to develop a thermonuclear weapon, wholly outweighs any military advantage" and the addendum, by Fermi and Rabi, made that point even more strongly. It said, "The fact that no limits exist to the destructiveness of this weapon makes its very existence, and the knowledge of its construction, a danger to humanity. It is an evil thing." Which is, to my mind, a very strong statement.

The complete majority and minority reports were considerably longer than what is quoted by Sagan. The minority report by Fermi and Rabi did add the condition that if the Soviet Union starts construction of an H-bomb, then so should the United States.

The Fermi-Rabi statement is advocating that the "*knowledge* of its construction is a danger to humanity." And even the knowledge that an H-bomb *could* be built is also a danger to humanity. Here Fermi is advocating suppression of scientific knowledge. But before the bomb project Fermi had advocated the exact opposite when he said, "whatever nature has in store for mankind, unpleasant as it may be, men must accept, for ignorance is never better than knowledge." I think Fermi would now still say that knowledge and truth is the ultimate goal, but that to achieve that goal one might have to temporarily suppress knowledge that could destroy the human race. How can the truth be discovered if there are no humans left to discover it?

Did Fermi contribute to the "invention" of the H-bomb? Or did he leave that "immoral" job to others at Los Alamos? Harold Agnew (see Chapter 21) has some comments about the contributions of Fermi and Garwin to the H-bomb project. Agnew says, "Enrico would come in the summertime, and he brought Dick Garwin. *(They shared the same office.)* Dick had been with us in graduate school, and between the two of them, they made *tremendous* contributions toward Los Alamos in those days." Fermi and Garwin were there at the very same time that the first H-bomb was designed and constructed. Agnew seems to be implying that they were the main inventors of the H-bomb. In a later chapter I present quotations from Edward Teller giving the main credit to Garwin for the invention of the H-bomb. If we combine the Agnew and Teller statements, they seem to be saying that Fermi, second to Garwin, made a "tremendous" contribution toward the H-bomb. I have observed that when Fermi and Garwin are together, sparks fly, and what comes out is greater than the sum of the parts. On the other hand I don't think either of them wants to be known as "the co-inventor of the H-bomb."

As a part of the Jay Orear retirement celebration at Cornell on November 8, 1993, Garwin gave a two-hour talk, "Learning from Experience in Defense Development and Procurement—My 43 Years and Still Counting." One could draw different conclusions about Fermi's involvement from this talk. Garwin said in the written version of this talk, "At the beginning of my second summer, 1951, I learned of the invention by Edward Teller and Stan Ulam of the concept now used in building two-stage thermonuclear weapons. As Edward Teller relates it, he explained to me the idea and expressed the desire to have a test explosion. But, as he puts it, I came back in a short time with a sketch of a first thermonuclear explosion (Mike) which was fired 16 months later (Nov. 1, 1952) with an overall explosive yield of 10 megatons. . . .Indeed, the design of the first hydrogen bomb was very much driven by theorists. As I recall, Teller after having conceived the new approach with Stan Ulam played only a small part, while the burden of the work was carried by Hans Bethe, Conrad Longmire, Marshall Rosenbluth, Harris Mayer and others." Note that there is no mention of Fermi in this Garwin quote.

On October 4, 1954, Fermi held a press conference at which he said that the H-bomb "achievements are the result of a remarkable group endeavor and the devoted and skillful effort of the individuals of the staff of the (Los Alamos) Laboratory." See Figure 18 for the text of the handout.

The quote from Agnew gives a different impression than the Garwin quote. I don't know which is closer to the truth. Perhaps the true story lies between the two. On September 14, 2003 (a few days after the death of Edward Teller) the *New York Times* ran an editorial titled "Teller's World." The second paragraph states in part, "The American bomb project (the A bomb) was a great technical success, but Dr. Teller's role in designing that weapon was minor. His mind was already on something bigger: the hydrogen bomb....The idea for such a bomb **originated with Enrico Fermi.**" This seems to be giving more credit to Fermi than to Garwin or Teller.

The film *The World of Enrico Fermi* closes with a shot of a press conference called by Fermi (with the help of Art Rosenfeld) on October 4, 1954 (less than two months before he died), that criticized a newly released right-wing book praising Teller and accusing Los Alamos of negligence.

October 4, 1954

It is my conviction that the Los Alamos Laboratory has deserved the gratitude of this nation through its development of both A and H weapons. This outstanding success is due to the intelligent and self-sacrificing work of its staff and to the sound and farseeing direction of Norris Bradbury. For this reason I have been deeply perturbed by the implications of the recent book The Hydrogen Bomb by Schepley and Blair, that the Laboratory dragged its feet and went only half-heartedly into the H-bomb development. Statements of this kind are bound to produce dissention and to set back the atomic program. It is true, of course, that Edward Teller is the hero of the H-development. But it is equally true that a single man cannot alone carry a job of that kind. A genius needs the support of many other men and organizations. The Los Alamos Laboratory developed and added to his ideas and brought them into practice. Quoting from President Eisenhower's citation of July 8, 1954:

> "The Laboratory's momentous success in the field of fission weapons has been followed by equal accomplishments in the fusion field. These achievements are the result of a remarkable group endeavor and the devoted and skillful effort of the individuals of the staff of the Laboratory."

ENRICO FERMI

Figure 18.
Press release issued by E. Fermi on October 4, 1954.

Fermi and the A-Bomb

Harold Agnew (see Chapter 21) suggests that Fermi was the first to point out that the plutonium A-bomb program as planned by Los Alamos could not work. It was thought that both U-235 and plutonium bombs could be triggered using a gun assembly (by very quickly bringing two subcritical pieces together). The first sample of plutonium was produced by a particle accelerator. Production could be cost-effective, however, only if produced by special nuclear reactors designed by Fermi. Fermi reasoned that reactor-produced plutonium would result in a heavier Pu isotope than laboratory-produced plutonium and thus have a larger spontaneous fission cross section. Early on he pointed out that such a gun-type plutonium bomb would have very low yield because of preignition. He saved the Los Alamos program by insisting on a chemical implosion trigger for the plutonium bombs. For more details, please see Chapter 21. To me, Fermi made the most crucial contributions at Los Alamos as well as showing in Chicago that a nuclear chain reaction was possible. It seems to me that Fermi deserves more credit than any other person for the invention of the A-bomb.

After World War II, Fermi was in my opinion unjustly criticized by Communists and some liberals in Italy for his work on the A-bomb. But during the war Fermi knew that we were in what was thought to be a close race with Germany in producing an A-bomb. Germany did have a head start in fission experiments. It was believed by many that Germany also had a head start in the race to achieve a self-sustained nuclear reaction. If Hitler had beaten us to the A-bomb, he could have forced a U.S. surrender. After the war it was learned that Germany had not even succeeded in achieving a self-sustained nuclear reaction. However, the Soviet Union was probably within one year of the United States in the development of an H-bomb. (Some of my opinions expressed here are based on discussions with Hans Bethe and Dick Garwin.)

And after the war Fermi's anti-H-bomb statement indicated that he advocated a joint U.S.-Soviet agreement not to work on thermonuclear weapons. However, after Fermi and his colleagues had learned that the Soviets were working on an H-bomb, Fermi realized that now we were in an even closer race—this time with the Soviet Union. It is not Fermi's fault that the political leaders of both sides would not listen to scientists such as Fermi, Rabi, Bethe, Wilson, and Szilard. And even if both Truman and Stalin had promised not to work on the "Super," we now have good reason to believe that Stalin could not be trusted. It is a darn shame that some citizens of Italy were rejecting their modern-day equivalent of Galileo. Fermi had brought Italian physics up from the bottom to the top in a very short time. I am not aware of any anti-Fermi movements in the United States. I was very active in the Federation of American Scientists, and I never heard anything negative about Fermi. I was a council member of the FAS for many years and for 1966–67 was chairman.

After the defeat of Hitler, Fermi and Oppenheimer (and other advisers) were consulted by President Truman to choose between military use or else a demonstration explosion of the first A-bomb. I suspect Fermi felt that the Japanese military leaders were in a kamikaze state of mind and that they were too fanatical to be influenced by a test explosion. But surprise use on a city of military value might result in a surrender. And if it did not, President Truman and these advisers decided that the first bomb should be followed by a second city of military value to be followed by an offer to let the people keep their emperor. They felt that this was an offer the emperor and the people could not refuse. It amounted to a relaxation of our firm condition of unconditional surrender. At least in hindsight we see that Fermi gave advice that resulted in a prompt Japanese surrender and most certainly a savings of hundreds of thousands of lives on both sides. Fermi never advocated use on cities of no military value. With hindsight, my personal opinion of what was wrong with our policy was the killing of so many Japanese civilians. We instead should have told the

Japanese public about the A-bomb, given them a list of two or three target cities and a list of two or three target dates (far enough in advance to permit evacuation). Then if the Japanese army blocked the evacuation, they, and not us, would have been responsible for the killing in Hiroshima and Nagasaki.

Japan suffered three devastating blows in a row: (1) Hiroshima, (2) Nagasaki, and (3) a powerful invasion of Japan by the Soviet Union followed shortly by an offer to let the people keep their emperor. This was an offer the people and the emperor could not refuse. At this point hundreds of the military officers committed suicide and an armistice was signed.

Chapter 13
Fermi and Religion

I was not able to obtain much on this subject, so this shall be a short chapter. What I did find was that Fermi did not spend much time on this topic. Whatever time the average person would spend thinking about spiritual and supernatural subjects, Fermi would spend trying to discover the truths of nature.

Laura Fermi did cover Fermi's view on religion in her book *Atoms in the Family*. On page 98 she wrote, "Enrico, who takes an agnostic view of all phenomena. . . ." I think this was her way of saying he didn't believe in physical miracles or supernatural phenomena. On page 108 Laura relates a conversation she had with her young daughter:

> Laura: No, I believe that he [Jesus] was a very good man, who taught people to love each other, but I don't believe that he was God's son."
>
> Nella: "What does dad believe?"
>
> Laura: "I was not prepared for that question. It is hard to explain to a child the attitude of one who called himself an agnostic, who admitted that with science he might be able to explain almost anything except himself, but who looked at other's spiritual needs with objective rationality."
>
> Laura to Nella: "Well. . .," I said, "Dad is a scientist. . . .Like many other scientists he isn't quite sure that God exists. . . ."

Segré's book reveals that even Fermi's parents were not religious. On page 3 he wrote, "The children (of Enrico's grandparents) were brought up religiously, and all except Alberto, Enrico's father, remained faithful to the church. As a grandmother, Giulia was sorry that Alberto's children had not followed the family's religious tradition, but she respected the will of the parents." On page 5 Segré wrote, "[Alberto's] children did not receive religious instruction, although they had been baptized in deference to the grandparents' feelings. Enrico Fermi's attitude to the church eventually became one of indifference, and he remained an agnostic all his adult life."

Gerald Holton quotes Enrico Persico as saying, "From his adolescence onward, Fermi had a quite definite, positivistic view of the world, although it is doubtful that he would have accepted this or any other conventional label for his philosophy. He had not been raised in a religious environment, and so did not have to pass through a religious crisis, as many Italians do when they reach the age of autonomous thinking."

Some have asked me to compare Fermi with Newton. I usually answer that Newton was not like the many other scientists to whom Laura is referring. When I was an undergraduate at the University of Chicago I took the many required courses and one of them was a philosophy course. We first read some of Newton's scientific writings. Then we were assigned just one page where Newton seemed to be scientific, but he used undefined terminology such as a nonscientific use of the scientific term "vortex." This was sneaky of Newton because he gave no warning that he was no longer using the usual precise scientific definition of vortex. Both professors in this course insisted that we spend at least one hour on this one page. One of the professors, Joe Schwab, was a very keen biological scientist as well as a philosopher. The next day they asked us to explain what Newton was talking about. No one, not even the professors, could explain anything on that page—not even I who was the only physics major in the class. This experience made me lose some respect for Newton. I may, because of this experience, be somewhat prejudiced against Newton. Certainly Fermi (or Galileo) could never be accused of unclear, supernatural-type writing. If Newton was trying to develop a scientific background to support religion, he seems to have failed in spite of his output of more religious writing than science writing.

Today the pyramid power people claim to have located four sacred vortices in the Sedona area of Arizona. I have stood on what they call the vortexes near the tops of Cathedral Peak and Bell Rock and observed a feeling of great beauty but nothing that meets my definition of supernatural. However, the feeling was strong enough to bring tears to the climber's eyes.

Early on I had invited Chandrasekhar to the Cornell symposium and he seemed quite interested in coming. Then when I learned he had recently given a series of lectures on Newton in England, I told him about my experience trying to understand Newton while a Chicago student and suggested that he give a talk comparing Fermi and Newton or at least be available for such questions. The next I heard from him he had firmly decided not to come to our Fermi Symposium.

There is more to religion than the spiritual and supernatural. To me, the most important contribution is a moral philosophy or how to tell right from wrong. Just where do these moral truths come from? Is it inborn, or as claimed in the United States Declaration of Independence: "We hold these truths to be *self-evident*, that all men are created equal and. . . ." How to tell right from wrong is not an easy question to answer. There are other moral debates and disputes where the answers seem not capable of proof.

As a University of Chicago student, Nella (in Chapter 26) gives an example where she got the best over her father in this area, and to his credit Enrico was quick to admit it. I quote from her talk in Chapter 26. Nella said,

> On one occasion, I managed to teach my father something. In college we read the works of Thoreau. I came home full of ideas of civil disobedience. My father did not approve. "It is the citizen's duty," he said, ponderously, "to obey the law. He may try to change the law, but until it is changed, he should obey it." I saw the counter argument and found it readily enough. "What about Hitler and Mussolini?" I asked. I could almost hear the wheels spinning in his head. In five seconds, the answer came out: "You're right." I reflected that not many people are so open to rational argument.

In the previous chapter we saw how Fermi was forced to deal with moral questions that directly involved the life and death of millions of people. Should the bomb be secretly dropped on a city of military value, or should the world first be shown a test explosion? Should the occupants of the target cities first be given a chance to evacuate? Just who are most qualified to make these decisions? How to compare American lives to Japanese lives? Short-term fatalities to long-term fatalities? Short-term survival to long-term survival of the human race? These are deep, difficult questions. Would you trust a religious fundamentalist or a career militarist to make the decisions? Who could we trust? Nella also answered questions about dinner table discussions of McCarthyism (fanatical anticommunism and persecution of political liberals). Fermi opposed the Edward Teller attacks on Robert Oppenheimer that resulted in the loss of Oppie's security clearance. Nella pointed out that Fermi was more of a personal friend of Teller than he was of Oppenheimer. But he testified on behalf of Oppie and he tried without success to change Teller's views.

Chapter 14
The Fermi Family

Figure 19.
Ishbar Fraser standing under the plaque marking the birthplace in Rome of her great-grandfather, Enrico Fermi. This photo was taken shortly after the ceremonial unveiling of the plaque on November 27, 2001. Ishbar, her mother, her uncle, her grandmother, and Jay Orear were on the platform that had been assembled for the ceremony. We heard a good speech from one of the Rome town fathers.

Material on the Fermi family can be found in other chapters of this book (4, 13, 21, 23, and especially 26—by Nella Fermi—and 30—by Laura Fermi) as well as in the books by Laura Fermi and Emilio Segré. In fact Laura's book has "family" in the title. The film *The World of Enrico Fermi*, produced by Gerry Holton, is also a good source for family details. Much is known about the Fermi family via writings of Laura, Nella, and Rachel Fermi and by granddaughter Alice Caton, Gerry Holton, Emilio Segré, and Enrico Persico.

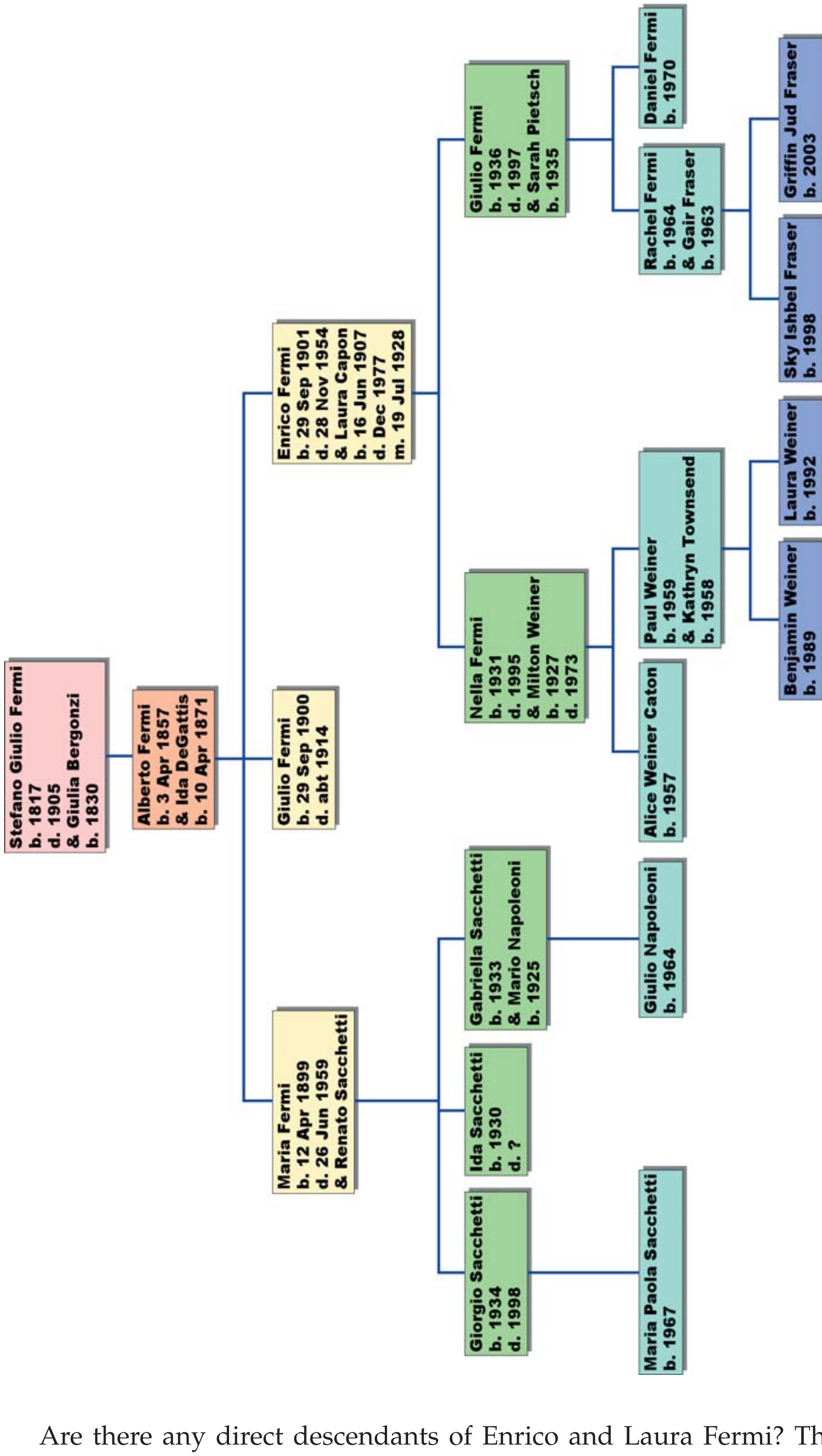


Figure 20. The Fermi family tree starting with Enrico Fermi's grandfather up to the year 2003 as prepared by Rachel Fermi Fraser and as drawn by Dean J. Robert Cooke.

Are there any direct descendants of Enrico and Laura Fermi? The answer is yes. In previous chapters I have referred to Enrico's daughter, Nella, and son, Giulio. They both received college educations in the United States. They both married and each had two children. So far two of those four grandchildren have given birth to four great-grandchildren. Figure 19 shows the second youngest, Ishbar Fraser, who is the daughter of Rachel Fermi. The family tree starting with Enrico's grandparents is shown in Figure 20. Each generation is shown in a distinctive shade or color. Six generations are shown starting with the two grandparents and leading up to the current list of great-grandchildren.

Figure 21. The official wedding photo of Laura and Enrico Fermi. The distinguished naval officer is Laura's father.

Figure 21 is the official wedding photo of Enrico and Laura. The distinguished Italian naval officer in the photo is Laura's father. He had retired as an admiral by 1938. His family was Jewish, and by 1938 the Jews in Italy were being mistreated. Enrico was fortunate to get his wife and children out of Italy just in time. Nella in her talk (Chapter 26) gave some information about the Nazi execution of Laura's father.

According to Segré, Enrico's grandparents (especially the grandmother Giulia) were religious. To Giulia's disappointment her son Alberto (Enrico's father) did not follow the family's religious tradition (Roman Catholic). However, only in order to please Giulia did

Alberto have his children baptized. And as we saw in the previous chapter, the baptized Enrico thought of himself as an agnostic as soon as he was able to think about such things. For more family detail please see the references given in the first paragraph.

Chapter 15
Fermi and Creativity

Perhaps the most famous example of Fermi's extraordinary creativity is his beta decay paper in 1933. Fermi first submitted it to *Nature* for publication, but it was rejected. The referees thought it was too far-fetched and impossible. (This is one of the shortcomings of peer review.) They didn't like the four-particle interaction that created an electron and neutrino out of nothing, and they didn't like taking the neutrino so seriously. After Pauli had proposed the neutrino in 1930, many physicists thought of it as some kind of bookkeeping procedure. They didn't think of the neutrino as a "real" particle that had an interaction cross section. But Fermi's theory did predict a well-defined energy-dependent collision cross section with protons. Fermi liked to reason by analogy and he felt that if there could be electron-positron pair production in nature, there could also be electron-neutrino pair production. He then submitted the paper to a less prestigious Italian journal where it was quickly published. Segré, on pages 73 and 74 of his Fermi book, comments, "Fermi's paper, written at the end of 1933 has stood the test of time with singular success; in fact, except for the nonconservation of parity, even today very few changes would have to be made to it. . . and his uncanny choice of the vector interaction was correct." (The most famous example of his extraordinary intuition.) The final version of his paper on beta decay was published in 1934 in *Zeitschrift für Physik*.

In 1951 Fermi said in a conference report, "Theoretical research may proceed on two tracks: 1. Collect experimental data, study it, hypothesize, make predictions, and then check. 2. Guess; if nature is kind and the guesser clever he may have success. The program I recommend lies nearer to the first track." He referred to track 2 as a big leap where great progress can be made all at once. He must have had his beta decay paper in mind as an example of "track 2." To me, it is an example of very high creativity in science. I don't think any other physicist in 1933 was close to producing this theory of the weak interaction. But, like any other discovery, in most cases it would have come a few years later. In this remarkable case it took 25 years before Lee and Yang and others made the final improvements.

To me Fermi's weak interaction was a much greater intellectual leap than Newton's checking the ratio of the acceleration of a falling apple to that of the moon falling toward the earth. (Today's high school students can easily calculate v^2/R for the moon and divide it by $g = 9.8\ m/s^2$. Fermi was truly a great theoretical physicist, a creative mathematician, a great experimental physicist, a great teacher at all levels, and a great engineer. Newton was also a great theorist, experimentalist, mathematician, and engineer (I love his reflecting telescope), but perhaps not one of the best teachers. (See my remarks in Chapter 13 on how Newton is misleading present-day college students.) Maxwell and Galileo were in the same league as Fermi and Newton. They also were excellent in both theory and experiment.

Segré, with help from others, has made a collection of the 270 most important papers of Fermi, and he has republished them in two volumes by the University of Chicago Press. Some of these papers have given birth to entirely new fields of physics. Segré also lists 13 books. I have read only a few of these papers and books. My short list of Fermi's most notable accomplishments is (1) the first understandable paper in quantum electrodynamics, (2) Fermi statistics and theory of solids, (3) the Thomas-Fermi model of the atom, (4) the weak interaction and beta decay theory, (5) neutron-induced radioactivity, which includes transuranic isotopes and not fully understood fission products, (6) confirmation of fission and measurement of neutron yield, (7) first self-sustained nuclear reactors, (8) nuclear reactor patents and design—the age of nuclear power, (9) neutron diffraction applications to solid state physics, (10) nuclear weapons, (11) pion beam designs, (12) his role in helping to create the nuclear shell model, (13) pion-proton elastic scattering, (14) discovery of the L=1 excited state of the proton, (15) the acceleration mechanism of cosmic rays, (16) the statistical model of particle production, (17) the approach to equilibrium, and (18) his approach to physics that by now has influenced scientists all over the world. Many of these discoveries opened up whole new fields of science and engineering. He was the father of quantum field theory, beta decay theory, solid state physics, neutron physics, reactor physics, the "Atomic Age," meson physics, cosmic ray production, and a father of nuclear weapons.

In this paragraph I shall attempt to deal with the question of who is the best physicist of all time. This is really a meaningless question unless the criteria for judging are specified. Should it be the best theoretician of all time, the best experimentalist, or the best combined theoretician and experimentalist? Should technological contributions that are beneficial to the human race be counted? How about weapons technology that can save lives and be helpful to one's country and friends and perhaps their very survival? In spite of these difficulties, *Physics World*, the house organ of the British Institute of Physics, did take a poll of its readers in December 1999 asking who is the best physicist in history without specifying any criteria. According to their results the top 10 choices for number one are Einstein, Newton, Maxwell, Bohr, Heisenberg, Galileo, Feynman, Dirac, Schrodinger, and Rutherford. I was disappointed that Fermi was nowhere on the list. At that same time *TIME* magazine named Einstein as its *person* of the century and put him on the front cover of its centennial issue. I am happy with Einstein being chosen by nonscientists, many who never heard of Fermi. If the criteria were that the physicist must be tops in theory, experiment, engineering, teaching approach, no mixing of science and the supernatural, and beneficial contributions to mankind, then from my close vantage point I would choose Fermi as the greatest scientist in all human civilization. But I am not enough of a historian of science to make expert comparisons with Maxwell and Galileo. I have read a book of Einstein quotations and I do not agree with all of them. Also I feel that quantum mechanics would not work unless "God *does* play dice with the universe." Perhaps Einstein's most famous quote is "God does *not* play dice with the universe." Einstein made some great discoveries in theory, but he was not an experimentalist or engineer. It has been said that he was not even aware of the relevance of the Michelson-Morley experiments to special relativity. Telegdi (see Chapter 20) says that Fermi did have some negative things to say about Einstein and Robert Oppenheimer. John Heilbrun, a leading historian of science, has pointed out to me that by adjusting the criteria, I can force Fermi to be number one. But I do think my list of criteria is reasonable and unbiased. I think most of the contributors to Part B of this book agree with the characteristics I have assigned to Fermi. I find it remarkable to have such close agreement. These contributors were not chosen because they happen to be in agreement. The sole criterion for choice was that they happened to know Fermi personally. So I end this first part of my book with my conclusion that Enrico Fermi was the master scientist of all time.

I know that some highly creative people in physics tend not to learn by studying textbooks in the conventional manner. Instead they try to work all the interesting problems. If such a person has trouble with a problem, he or she then goes to that part of the text. We know that Fermi used books in such a manner. I also know that Lee and Yang studied together in such a manner. I recently learned that Fermi, when he was 16 and 17, learned much of physics from a 5,000-page set of five volumes by the Russian Chwolson. Fermi was fortunate to have obtained his own personal copy of the French edition. He first did a quick run-through the entire book to eliminate the 1,000 pages he already knew. Then he spent several months on the remaining 4,000 pages until he had mastered them. Uri Orlov, who also learned physics from Chwolson, has told me that it, like other textbooks of its time, does not contain problems. I do know that through most of Fermi's life if he was told of a new discovery, he would independently work it out for himself to achieve a true understanding. Sometimes his independent derivation would be superior to the original. A complete and early mastery of Chwolson is one of the secrets of Fermi's success. He always seemed to know more diverse physics than any of the rest of us. In the next two paragraphs I will go a little deeper into just what is creativity and how to train students to be more creative.

Some educators feel that it is not possible to teach creativity. But I claim through personal experience that it is possible to train for creativity. Perhaps educators could get some clues by studying the methods used by people like Fermi, Garwin, Lee, and Yang when they were young. I am certainly not at their level, but I was fortunate to have had special intensive training for creativity while in the twelfth grade. I was one of three leading students per year in Chicago who did have this special training compared to about 100 who did not. We were all tested for creativity, and always the three with the special training beat the 100 in the creativity tests. My math teacher, Beulah Shoesmith, trained 10 or so honor students as candidates to compete in the Chicago citywide annual high school math competition based on problem solving. There were over one hundred contestants with no more than three entrants per school. But each year my school *always* won the top three prizes! Statistically this is impossible. If all the math students in those high schools of Chicago that participate in the citywide math competition are randomly distributed in creativity, then the winners of the competition should be randomly distributed among the contestant population. But just one of the high schools offered the Shoesmith training method and that one high school won all the prizes every year until the time of her retirement.

It is clear that Miss Shoesmith had a method to train us for creativity that was superior to that of the other schools. The best possible teaching would involve private tutoring from the best possible teacher. In this private tutoring ideal the student could be active almost 100 percent of the time. Shoesmith was widely acknowledged as the best math teacher in the Chicago public school system. The typical problem in the math competition did not involve much math, but it *did* require clever reasoning in new and unforeseen situations. Shoesmith proved that it is possible to train for these skills, and these are just the skills needed for creativity. She had a file of all the previous contest problems plus others of her own. I was able to do much better on these problems after receiving her training. She spent one hour (before classes) each morning drilling us on these problems. We each had a place at the 10 blackboards and spent most of the hour trying to solve these sample problems. She would dictate a random problem and then watch each of us as we progressed. It was like private tutoring of 10 students at a time. She seemed to have 10 sets of eagle eyes. She would make comments to individual students, and at times to the group as a whole and at other times call on a student to give his or her reasoning. It was very intensive and active learning for an hour each day. At the end of the year she chose the three best out of her 10 candidates to enter in the citywide competition. I was her number two choice and I did win second place in the entire city of Chicago.

This training was ideal for becoming a successful physics student and researcher. Some of the problems indirectly involved some physics. It was far more useful than my high school physics course or even other college courses.

I experienced the best of two worlds of creativity, first with Shoesmith and then with Fermi. Even so, I never reached the level of those Fermi students who received the Nobel Prize. I have no complaints; it was and still is a very exciting and rewarding life. It gave me the taste of Nobel Prize–caliber work. I am most proud of being the first to show convincingly that two major decay modes of the K-meson are of opposite parities (that parity conservation is violated by the weak interaction). And it was Enrico Fermi, the Master Scientist, who taught me how to make the statistics convincing and correct.

Part B
Fermi as Seen by Others

Part B contains some of the papers delivered at the 1991 Cornell Symposium. Chapters 22 (part 1), 30, 31, and 32 are additional materials from authors who were unable to attend.

16.	Welcome to Cornell	Dale Corson
17.	The Italian Navigator	Carl Sagan
18.	Pilgrimages to Rome	Hans Bethe
19.	Fermi and the Nuclear Age	Al Wattenberg
20.	Fermi at Chicago	Valentine Telegdi
21.	Fermi at Columbia, Los Alamos, and Chicago	Harold Agnew
22.	(1) Working with Fermi	Robert Wilson
	(2) Fermi and Politics	Robert Wilson
23.	Laura Fermi and Family	Jane Wilson
24.	Glimpses of Fermi in Chicago and Los Alamos	Dick Garwin
25.	Fermi and Technology	John Peoples
26.	A Different Perspective	Nella Fermi
27.	Comments of Some Former Grad Students	Art Rosenfeld
28.	Glicksman Comment	Maurice Glicksman
29.	Wolfenstein Comment	Lincoln Wolfenstein
30.	My Life as a Physicist's Wife	Laura Fermi
31.	Enrico Fermi	C. N. Yang
32.	Fermi Centennial Comments	Leon Lederman

List of Figures

22. Dale Corson
23. Carl Sagan
24. Hans Bethe, Boyce MacDaniel, and Bob Wilson in Synchrotron Tunnel
25. Cowboy Wilson bareback on horse
26. Neutron flux as a function of number of layers in the nuclear pile
27. The Rome Fermi Museum
28. Fermi and Wilson in a Los Alamos group picture
29. Bob Wilson, Jane Wilson, and I. Rabi
30. Dick Garwin speaking to Hans Bethe
31. Laura Fermi presenting her paper at Erice
32. Frank Yang
33. Leon Lederman and Bob Wilson singing their song

Chapter 16
Welcome to Cornell

Dale Corson
Chancellor, Cornell University

Figure 22.
Dale Corson, chancellor of Cornell University. Corson had worked with Fermi, Wilson, and others at Los Alamos. After the war he left Los Alamos for Cornell with Bethe, Wilson, Feynman, Morrison, and others. He became chairman of the Physics Department, dean of the College of Engineering, president of Cornell University, and president emeritus of Cornell University.

Professor Orear, ladies and gentlemen. Cornell is pleased to welcome you to this program today. My qualification for being the greeter is simple: I'm in town. *(laughter)* You should be greeted by the president or the provost, but they're on their way back from the West Coast, where they attended the Cornell football victory over Stanford on Saturday. . .moral victory, that is *(laughter)*. . . .I, first of all, want to thank Jay Orear for his role in organizing this program today. I've known Jay for 30 or 35 years, and I knew him first as the author of Fermi's book on nuclear physics. I still have my copy, Jay, and I looked it up a couple of days ago, and it's pretty beaten up. I used it a lot.

My association with Fermi was brief. I knew him at Berkeley, in the pre–World War II days, and I knew him, as Jay said, at Los Alamos, just at the end of the war. Fermi had a way of understanding the physical world and helping others to understand it that's absolutely unparalleled in my experience as a physicist. I remember a seminar at Berkeley, probably in the spring of 1940, on particles passing through condensed matter. He took the problem apart, he looked at the pieces of it, he looked at the relativistic contraction of the electric field of the high-velocity charged particles, he looked at the polarization of the matter that the particles went through, he looked at the shielding produced by that polarization, and elucidated the whole progress that led to the anomalous range. After the seminar, there was a party at Emilio Segré's house, and I said to Fermi, "You made it so simple, I feel like I should have been able to do it myself." And he said, "Of course you could have done it yourself! *(laughter)* Just look at the problem, piece by piece, and put it all together." That was excellent advice.

There was another seminar I remember, in fact I looked up my notes on it, in the spring of 1940, in which Fermi was talking about the possibilities of chain reaction from fission, and he looked at it piece by piece, considered the circumstances under which there might be a brief chain reaction, but not sustained; he described that as a situation which would be unpleasant for the experimenter but would not be socially significant. It was the same clear, lucid thinking about what would happen. There's a story that I think was attributed to Arthur Compton, after Fermi died in 1954. Compton related a train trip with Fermi, I think going to Los Alamos, or someplace in the west, where they were going to higher and higher altitudes, and Fermi started thinking about what this would do to his watch, with the balance wheel entrapping less and less air as it went to higher and higher altitudes, and he calculated the rate change of the watch from that effect. He understood the physical world and all its aspects. There were some things that were missed. I have a vivid memory of the discovery, of when we learned at Berkeley about the discovery of fission, in early 1939, it must have been, and Segré was totally dismayed. The Rome group had looked at uranium in some of slow neutron work in the early '30s, and they had used ionization detectors, which they put beyond the range of the alpha particles, so they wouldn't have all those counts in the chamber. Had they put them a little bit closer, then they would have seen the huge pulses from the fission fragments, and I've often wondered how history might have been changed had Fermi and Amaldi and Segré, and all those other wonderful people who worked there, just moved that detector a few—two or three—millimeters closer to the target.

Thank you for coming. It's a great program, and I'm looking forward to it.

Chapter 17
The Italian Navigator

Carl Sagan
Astronomy Department, Cornell University

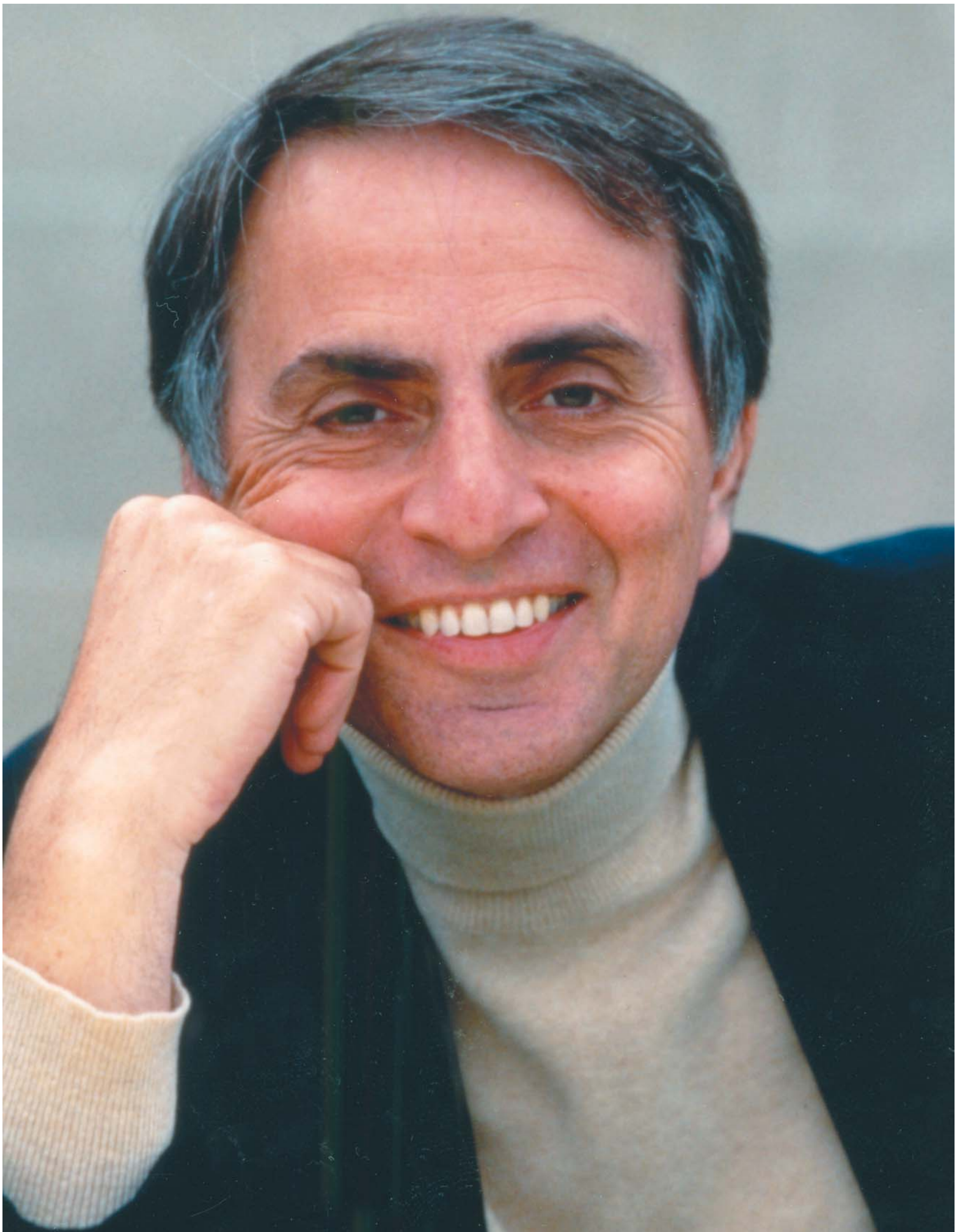


Figure 23.
Carl Sagan, professor of astronomy, Cornell University. In addition to his Cornell teaching and research, Sagan produced TV programs and prizewinning books to help nonscientists understand science. Photo courtesy Cornell University Astronomy Department.

Thank you, Dale. I'd like to welcome you again to this extremely interesting, it seems to me, retrospective on the life of Enrico Fermi.

There is a Fermi Sea, a Fermi Energy, a Fermi Paradox, Fermi Statistics, to which the name of Dirac is also associated, a Fermi class of elementary particles—the Fermions, a Fermi Constant—a coupling constant for the Fermions, a Fermi Level, a Fermi Surface, a Fermi Mechanism—for the acceleration of cosmic rays, a Fermi Age—neutron diffusion, a Fermi unit of distance—which is roughly the size of a nucleon, two Fermi Golden Rules, a Fermi Prize, a Fermi Institute, a Fermi High School—which is represented here, a Fermi National Laboratory, and a chemical element named after Fermi—having the nice round number of 100 and identified from the debris of the first thermonuclear reaction explosion in November 1953, in a way, fittingly enough. It's hard to think of another physicist of the twentieth century who's had so many things named after him—and this surely is an indication of the respect and affection with which he is thought of in the community of physicists, and in a larger community as well. Fermi was, clearly, gifted, productive, insightful. . .had an extremely rare mix of the talents of both theoretical physicists and experimental physicists and, for good or for evil, he was one of the leading pioneers of the age of nuclear weapons and nuclear power. There is a kind of metaphor, or allegory, that occurred when the first self-sustaining nuclear reaction occurred in Stagg Field, at the University of Chicago, in December of 1942, in which one high official made a secret phone call in code to another high official to describe the success of that attempt. Jay Orear has kindly put together a film, which is a reconstruction of the secret phone call. Now, what was actually said, according to Laura Fermi's book, *Atoms in the Family*, is shown here on the left, and what was reconstructed you will see right now, between Arthur Holly Compton and James Conant.

(Tape begins, voice of Edward R. Murrow)

> The scientists went up to the street level, most of them never returned to that room until this week. They carried their secret with them. When it was necessary to inform Dr. Conant at Harvard, who would relay the news to Washington, Dr. Compton did it in a most guarded telephone call. Dr. Compton said he was no actor, but he agreed to repeat it for us, as a tribute to Fermi:
>
> "Dr. Conant?—Jim, this is Arthur. I thought you'd want to know that the Italian navigator just landed in the New World. . . .Yes. . . .The natives were friendly. Everyone landed safe and happy. You ought to know the next. . . .get that on to you as soon as we can. That's all today." *(videotape ends)*

Thanks, Jay. Could we have the lights back on, please?

Well, the level of secure, cryptographic communication has improved along with everything else since then. The connection between Enrico Fermi and Christopher Columbus is, therefore, made explicit for us by this phone call. One could draw some comparisons, although I think they are, in large part, strained. Columbus, for example, never understood his accomplishment and never foresaw its possible misuse; the same was true with Fermi when he first bombarded uranium with neutrons in 1936. But by December 1942 Fermi did understand his accomplishment and its possible misuse. The clearest demonstration of that is the addendum to the October 1949 report of the General Advisory Committee to the U.S. Atomic Energy Commission, in which there was an addendum by Enrico Fermi and I. I. Rabi. This was a report on whether it was a good idea to build the first thermonuclear weapon, and the main report, signed by Robert Oppenheimer and others said, "The extreme danger to mankind, inherent in the proposal by Edward Teller and others, to develop a thermonuclear weapon, wholly outweighs any military advantage" and the addendum, by Fermi and Rabi, made that point even more strongly. It said, "The fact that no limits exist to the destructiveness of this weapon makes its very existence, and the knowledge of its construction, a danger to humanity. It is an evil thing." Which is, to my mind, a very strong statement.

This symposium is being held close to Columbus Day, in part because of a misunderstanding. *(laughter)* Fermi was the master of the sufficient approximation, and so all the anniversaries are good to first order. We're two days to October 12, we're one year from the five hundredth anniversary of Columbus's discovery of America. . .that was the misunderstanding. When Jay told me that Dick Garwin was going to be here around October 12, I misunderstood. I thought it was 1992, not 1991, and I suggested why don't we have a symposium honoring Enrico Fermi *(laughter)*. . .but one part in 500 is high precision. *(laughter)* It's also roughly one year from the fiftieth anniversary from that first self-sustaining nuclear reaction. So, if we're concerned with only a few percent accuracy, this symposium is exactly, in the spirit of Fermi, at the right time. Dick Garwin also says that it's very customary to begin a celebration a year before, and then have a year to celebrate, *(laughter)* so I don't think I have to feel guilty that I misunderstood Jay Orear. One could ask why Cornell, and not the University of Chicago, and I think that's just a matter of there being a larger, or at least a large number, of Fermi's colleagues here at Cornell, plus other virtues Cornell has over the University of Chicago. *(laughter)* This will be a day of anecdotes, and I'd like to give two. I should say that I myself never knew Enrico Fermi. I went to the University of Chicago as an undergraduate, partly because of him and two other people, Harold Urey and Gerard Kuiper. I got to know Urey and Kuiper very well, but Fermi, to my great regret, died before I got to be a graduate student in physics. In fact, I hope to learn more about him today. The first anecdote comes from Laura Fermi's wonderful book, *Atoms in the Family,* which many of us have been reading in preparation for this, and it's quite brief, I'll just read it:

In the spring of 1941, Enrico and a few other professors at Columbia University organized a society of prophets. On the first day of each month, during the lunch hour at the Men's Faculty Club (a little hint of what it was like in 1941) society members wrote down 10 yes or no questions about events likely to occur during that month. Would Hitler attempt to land in England? Would an American convoy be attacked by German ships in violation of United States neutrality? Would the British be able to hold Tubruch? The prophets wrote down their answers, these were checked on the last day of the month, records were kept of each prophet's score. By the time the society dissolved, Enrico had the highest score and was the prophet. Ninety-seven of his predictions had come true. In foreseeing events, Enrico is helped by his conservatism. He maintains that situations do not change as fast as people expect. Accordingly, Enrico predicted no changes. *(laughter)* Hitler would not attempt a landing in England during the month considered, the British would hold Tubruch, no American convoy would be attacked. His conservatism made him foresee no German attack on Russia during the month of June, and in this way, he missed a perfect score."

I think that gives a very interesting insight.

The other one was a story that I heard at Chicago, and I'm relying on memory, but it goes something like this: During the war, Fermi was told that so-and-so was a "great general." I can't remember who so-and-so was, but for the sake of explicitness, let's pretend it was George Patton. Fermi said, "How many great generals are there?" then divided that number into the total array of general officers and came up with some number of a few percent. And then he said, "And what is the criterion by which you determine what is a great general?" and there was some to and fro, in which whoever he was talking to said it was "reputation." Fermi wanted a numerical, quantitative definition, and so what finally came back to him was "having won several consecutive battles." Fermi wanted to know how many, and so by successive approximations, a number like five was agreed upon. Five consecutive victories in battle makes you a great general. Fermi said, "Now wait a minute. Let's suppose that all armies are equally matched, and *(laughter)* therefore, it's merely a matter of chance who wins the battle. *(laughter)* One-half to the fifth power is a "few percent," and therefore there will always be a few percent adjudged great, independent of their ability." *(loud laughter)* This also seemed to me a very interesting insight, and in a way, again, that same conservative temperament.

As a final remark, about the purported connection with Christopher Columbus, there is, of course, a debate going on right now about whether Christopher Columbus was, on the whole, good or bad for the world. I think it's fair to say that, while he unintentionally let loose great suffering, he was also the agent of unification of the planet. I think that's a noncontroversial statement. Maybe the same thing is true of nuclear weapons and, in that case, maybe there is a deeper connection.

I'm very happy to be in the role of chairing this meeting, although I know next to nothing about the subject, compared to the many here who were close colleagues of Enrico Fermi, and I'm delighted to have the chance to introduce the next speaker, our own Hans Bethe, who knew Fermi from the early thirties in many different roles. I'm looking very forward, Hans, to a discussion called "Pilgrimages to Rome."

Chapter 18
Pilgrimages to Rome
Hans A. Bethe
Transcript of the talk given at the special one-day symposium

Memories of Enrico Fermi

Figure 24.
Hans Bethe, Boyce McDaniel, and Bob Wilson riding bicycles in the tunnel of the Cornell electron-positron collider. Bob Wilson, who grew up on ranches in Wyoming, is riding his bike as if he is on his horse. Hans Bethe was director of the Theory Division at Los Alamos, and Bob Wilson became director of the Experimental Division. Fermi consulted closely with both of them. Wilson, when he left Los Alamos, became director of the particle physics lab at Cornell. Boyce McDaniel was also at Los Alamos and he became the second director at Cornell. This photo has been retouched.

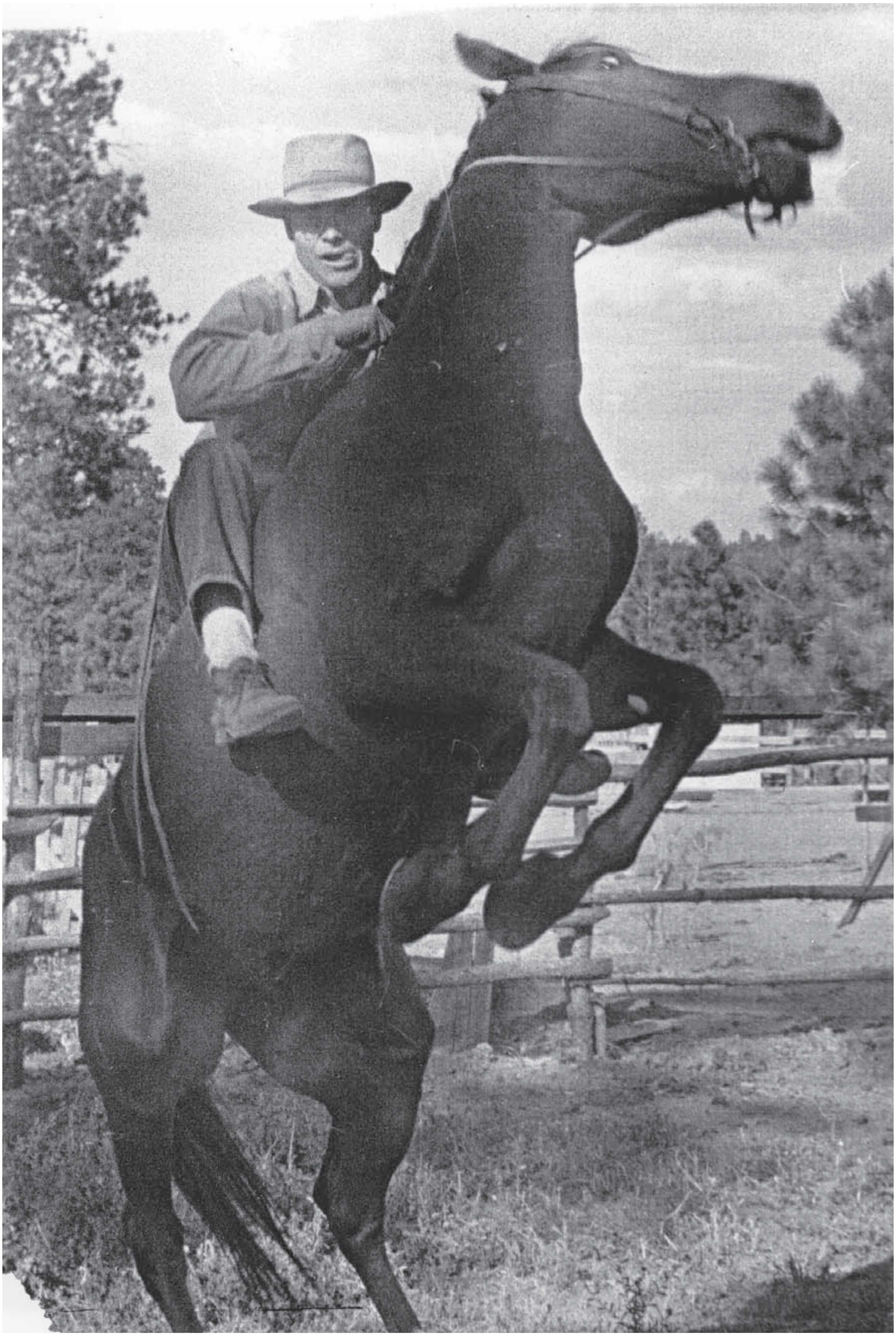


Figure 25.
This photo has not been retouched. It shows Wilson riding a horse bareback in a manner similar to how he rode the bicycle in the previous photo. According to Chapter 22, he was expert with a gun as well as a horse.

About 60 years ago I went to Rome to work under the direction of Enrico Fermi. My professor, Sommerfeld, had got me what was called a Rockefeller Fellowship and had suggested that I go to Rome. I was not at all convinced that that was a good idea, but I soon did become convinced.

Fermi worked in the Institute of Physics, which was on a small hill in the middle of Rome, surrounded by a sea of traffic but very quiet on that little hill. There were trees, ponds, a nice garden, a fountain—really quite an oasis in the hectic traffic of Rome.

Fermi was 29 years old when I got there. He would have been 90 a few days ago now. He was a full professor since he published *Fermi's Statistics* at the age of 25. He had a small group of collaborators working with him, and I will project a few names, because clearly one cannot remember these names if just told to you:

(*projects names):*

Corbino, Rasetti, Segré, Amaldi, Fano, Majorana, Pontecorvo, Gentile, Wick

He was very close friends with these three people: Rasetti, Segré, Amaldi. Rasetti, in fact, was about the same age as Fermi, and Segré and Amaldi somewhat younger. There were some other people doing theoretical physics: Racah, known to many physicists for his theory of how to put spins together; Fano, who for many years later was professor at Chicago; and Majorana, who was probably the most gifted theorist of them all—in fact, Fermi and Majorana once had a competition in which they came out even, Fermi with the help of a slide rule and Majorana without. (*laughter*) Majorana was a terribly retiring person; he hardly was even visible. Once he was made a professor at the University of Naples, this gave him a breakdown and he then disappeared mysteriously. Majorana, as most physicists know, found the first correct representation of nuclear forces, superior to the initial one of Heisenberg. Later on, after my time, Pontecorvo joined the group and there were two other physicists also later, Gentile and Wick. So that was a small group, the first four very closely knit.

Fermi, in addition to being a full professor, was a member of the Royal Academy. That was an invention of Mussolini; it was supposed to include all the very best brains of Italy. And somehow by the initiative of Corbino, whom I mentioned was the department chairman, Fermi was made a member, being by far the youngest of that group. That distinction carried with it the title of Excellency, it was "Sua Excellencia Fermi," and more important, it multiplied his salary by 2.5, which was very important because the salary of an Italian professor was equivalent to about $100 a month. Now a dollar at that time is about $10 now, but even a thousand dollars for a full professor is not a very princely salary, and so it was very welcome, that membership in the Royal Academy brought that much higher salary.

Fermi was very open. The door to his office was always open—completely unusual for a professor in Europe. You could come in any time; you could ask any question, and Fermi would probably know the answer. Therefore, by the members of his group, he was called the Pope: the Pope is infallible in matters of the faith *(laughter)* and Fermi was infallible in matters of physics. There was quite a hierarchy. Rasetti was the Cardinal Secretary of State *(laughter)* and another physicist who was then a professor at Turin, Persico, an old friend of Fermi's, was made the cardinal in charge of propagating the faith. *(laughter)* This was very appropriate because when Persico came to Turin, nobody there believed in the quantum theory. The Cardinal did his job very efficiently. And after a few years everybody believed in the quantum theory in Turin. *(laughter)*

I had a big room next to Fermi's, an enormous room, bigger than my office now. *(laughter)* In contrast to my office now, it was completely bare, nothing cluttered. There was a big table. There were two chairs. That was all that was in the office. In my first year, I inhabited this by myself; in my second year, I was there joined by Placzek, whom a few of you will know.

Segré, Rasetti, and Amaldi were experimentalists, mostly working on spectroscopy. Fermi was a theorist and worked at the time on quantum electrodynamics. Just the summer before I came there, he was invited to the summer school in Michigan, where he presented his theory of quantum electrodynamics, which made everything clear. Before him, Heisenberg and Pauli had written a learned paper on quantum electrodynamics which was totally unreadable and terribly complicated. Because it was so complicated, I never read it. *(laughter)* They had tried to do quantum electrodynamics in ordinary space. Fermi had the sensible idea to do it by a Fourier analysis, that is considering waves, and then everything became simple, and this, as Dale Corson has already told you, was Fermi's message altogether.

The access to the institute was via a driveway, which also led to the Department of—I believe it was the Department of the Interior, but I'm not really sure which department it was—anyway, it was an important government department. I never went there, and probably nobody would have let me in. From time to time there was a meeting of important people at that department and then the guardians of the access road would stop everybody and not let us in. Well, when I came across this, I would just go home, as I was turned back, but not so Fermi. He came driving and when they stopped him, he said, "I am the driver to His Excellency Fermi. *(laughter)* And His Excellency would be very annoyed if you didn't let me in." *(laughter)* And, as he told the story later, he emphasized that he had told the whole truth: he *was* the driver to the Excellency Fermi, *(laughter)* and indeed His Excellency *would* have been very annoyed. *(laughter)*

Well, in the first year I was there, I was working on solid state, which didn't interest Fermi very much. I was working on the splitting of energy levels in crystals, and that is done most excellently in the rare earth elements, so I wanted the wave functions of electrons in a rare earth. So, I went to Fermi and he told me how to do this. He said, "Well, you take the Thomas Fermi statistical atom, but you don't take the atom but you take the ion, and that gives you the potential and then you get the wave functions in that potential." So I did. I had learned from Hartree how to do numerical integration quickly and in a rough approximation, so I produced some wave functions. And I was very very proud when Fermi told me to put that result in the Treasury of Psi's, of Wave Functions. He had a small booklet in which numerically computed wave functions were collected. I don't know what happened to it later. I afterwards did the linear chain of spins, which is now known as the Bethe Ansatz; I have no idea what people do with the Bethe Ansatz. *(laughter)*

Well, Fermi and the other group and I talked German. They all spoke German very well. With Laura Fermi, his wife, I spoke English. The only Italian I learned was enough to get me a meal but not much more. Laura was considerably younger than Fermi; I believe she was 22, and Nella had been born just a month before I came.

My conversations with Fermi showed me a completely new approach to physics. I had studied with Sommerfeld, and Sommerfeld's style was to solve problems exactly. You would sit down and write down the differential equation. And then you would solve it, and that would take quite a long time; and then you got an exact solution. And that was very appropriate for electrodynamics, which Sommerfeld was very good at, but it was not appropriate at all for nuclear physics, which very soon entered all of our lives.

Fermi did it very differently, and Dale Corson already described it very well, namely he would sit down and say, "Now, well, let us think about that question." And then he would take the problem apart, and then he would use first principles of physics, and very soon by having analyzed the problems and understood the main features, very soon he would get the answer. It changed my scientific life. It would not have been the same without having been with Fermi; in fact I don't know whether I would have learned this easy approach to physics which Fermi practiced if I hadn't been there.

This method also showed very clearly later on when both Fermi in the United States and Heisenberg in Germany were trying to get a nuclear chain reaction. Fermi thought for a while about how to arrange the uranium and found that it should be put in essentially spheres in a lattice in graphite. Heisenberg, on the other hand, decided it had to be arranged in a way that he could actually calculate and where he could solve the differential equation for the diffusion of neutrons and so he decided to arrange the uranium in sheets. Then it was a one-dimensional problem of the uranium and the graphite, well, in his case, heavy water, and, of course, he never got a reactor, while the Italian Navigator navigated very successfully to the completion of a reactor. The Germans, as most of you will know, at the end of the war, two and a half years after the Fermi pile was going, were barely halfway to having a nuclear chain reaction.

On my second visit, there was some common interest with Fermi. I had done the relativistic stopping power for electrons using a method invented by Mueller in Denmark in which you take just the product of initial and final wave function. That gives you the transition charge density, and then if you have plane waves, it is very easy to get the scalar and vector potential from that distribution and you use that then to calculate the phenomena.

Fermi, as I told you before, was interested in quantum electrodynamics, so he was interested in comparing different methods of doing quantum electrodynamics. There was this simple method of Mueller; there was the Breit interaction; and there was straight and honest quantum electrodynamics. So we sat down for a day together to compare the three methods. I think it was only a day or two that we worked on this. And then Fermi would sit down at the typewriter and would dictate to himself in my presence, would dictate the paper in German, and I would write the equations, which, of course, I read to him and then I put the equations into the manuscript in ink, and that was the paper. It was published in *Zeitschrift für Physik* Vol. 77, and it is one of the papers that Segré collected in the *Collected Works of Fermi*.

Fermi was incredibly disciplined. After each lecture, he went to a big book calendar in which he entered precisely what he had talked about in that lecture, so he would know for the rest of that semester, but also for future years, what should be in that course and he could present it to university authorities, if necessary.

I don't know when he came to the office; it was too early in the morning for me to observe, *(laughter)* but I did observe that he always left the office precisely at noon, and he always returned from lunch precisely at 3:00. You could essentially set your watch by his movements. On the other hand, Placzak and I, of course, came to the office at very irregular hours; in my second year, I usually came about noon when Fermi was about to leave, and the very dignified custodian, so to speak, of the institute said, "Those Germans are really crazy people: when other people work, they sleep; when other people eat, they work; and so on." *(laughter)*

I mostly wrote an article for the *Handbuch der Physik* on one- and two-electron problems. Placzek, who sat next to me, struggled valiantly with also an *Handbuch* article, on light scattering. He spent most of his time in the library, because the library was excellent in very old journals, 1810 and so Placzek would investigate what people said about light scattering in 1810. So he never got finished. *(laughter)*

During my second visit in 1932, Fermi became interested in nuclear physics. There were some very strange experiments by Bothe in Germany in which he had bombarded materials with alpha particles and found that at least in light elements, especially beryllium, some very penetrating radiation came out. Now, penetrating radiation should be gamma rays, should be very short wavelength light, and gamma rays were well known, but these rays didn't behave like gamma rays at all. Joliot, in Paris, and his wife, Irene Curie, did similar experiments and drew totally crazy conclusions.

So Fermi in 1932 thought it would be an interesting idea to do experiments and to find out what really happened. Well, he was too late, because in the same year Chadwick in Cambridge discovered the neutron, which was the explanation of these strange phenomena; and Chadwick had an unfair advantage because not only had he worked with radioactivity for many, many years but also Rutherford had conceived the hypothesis of a neutron many years before.

Then, however, in 1934, the Joliots discovered artificial radioactivity, which they produced by bombarding various elements with alpha particles. Fermi immediately concluded that neutrons should be much better in making reactions in elements, because neutrons can penetrate any nucleus without any potential barrier. So he decided then and there that they would experiment on the interaction of neutrons with various elements, hopefully producing radioactivity.

He organized a group to do this—of course, his old collaborators and friends, but they added d'Agostino, who was a chemist, and most importantly, Trabachi, who was a biophysicist in charge of the biophysics in the Department of Health of the City of Rome. He had a very precious possession, namely one gram of radium. And radium produces all the time, radon, a gas, which can easily be separated because it escapes from the radium, and then you can expose any sample you want to the alpha rays from radon. There are some amusing stories about this in Laura Fermi's book.

Fermi first was unlucky, because being very systematic, he started with hydrogen and went up the periodic table, and none of the elements up to oxygen would show the slightest trace of radioactivity. And thinking about this talk, I figured out that that's how it has to be: you can very easily explain why none of these would give radioactivity. However, the next element, fluorine, was very successful and from then on up, practically every element becomes radioactive under the influence of neutrons, and with the light elements, the reaction is a neutron in and either a proton or alpha particle out; with heavier elements, the neutrons are simply captured and make an isotope of the same elements one mass unit higher.

(projects a table of names occurring in his talk)

Name	**Specialty**
Bothe	gamma rays
Chadwick	neutrons
Joliot, Curie	artificial radioactivity
d'Agostino	Chemist of the group
Trabachi	Radium source
Meitner	Fission experiments
Hahn, Strassman	Fission discovery
Frisch	Fission theory
Laura Fermi	wife and author
Rasetti	Fermi's oldest student

Well, the experiment involved that the exposure of samples in one room and then the experimenters had to run as fast as they could along the second-floor corridor from the exposure place to the counter. Of course, you wouldn't want the counter next to the source of alpha particles and gamma rays, and I believe Fermi had the record of time *(laughter)* of running from one place to another. There was a visit one time from a very dignified Spanish physicist, who wanted to see His Excellency Fermi, and he was shown a man in a very dirty lab coat, running like mad along the corridor *(laughter)*, and he could only see His Excellency while he was sitting next to the counter and observing the counts which would come from this sample.

One of Segré's jobs was to procure samples of all elements. There was fortunately one place in Rome which sold chemicals of all sorts and Segré bought everything, and then asked for rubidium and cesium. "Ahhh," said the owner of the shop, "nobody has asked for those elements for 15 years *(laughter)* and here are the samples. I'll give them to you, free of charge." *(laughter)* What's more (and I read that in Laura's book), he did so, accompanied by a Latin sentence, saying "I give you these samples for the love of God."

In most cases, it was quite easy to determine what had happened in the capture of the neutron, what radioactive material had been produced, and there, of course, d'Agostino, the chemist, was very helpful. But there was one element which defied the ingenuity of all these people; that was uranium. It gave very confusing results. So, uranium was then further investigated by Lise Meitner in Berlin and she found more and more confusing things, many more radioactive elements—daughters and daughters and daughters and so on—and we used to make fun of her and say, "She was not married, she did not have a family and so she needed families of radioactive elements."

Well, her research finally led her friend and boss, Otto Hahn, to the real discovery. Hahn and Strassman, at the end of '38, discovered that the bombardment of uranium led to barium in the middle of the periodic table, and soon thereafter Meitner and Frisch found out that this was due to the fission of the uranium nucleus splitting into two more or less equal parts. This happened at the end of '38 and just accidentally, but interestingly, this was just the time when Fermi got the Nobel Prize for all the radioactivities that he had found with neutron bombardment. So he got the Nobel Prize just as Hahn, Strassman, Meitner, and Frisch discovered what really had happened in uranium. This discovery, of course, kept Fermi busy for about seven years thereafter, leading to the chain reaction in Chicago and then to the atomic bomb.

Also, by coincidence, the Nobel Prize came just at the time when Fermi and Laura had decided to emigrate from Italy. Italy had come completely under the influence of Nazi Germany, after the Anschluss of Austria, and Nazi Germany insisted that Italy, like Germany, institute anti-Semitism, so edicts were put out in September of '38 to practice anti-Semitism. That was difficult in Italy because only about one Italian in 1,000 was a Jew, and the story was current (and this again comes from Laura's book) that a small town wrote a letter to Rome to headquarters, "We would like to practice anti-Semitism. Please send us a sample." *(Audience laughter)* Laura was Jewish, and so Enrico and Laura had planned in the fall of '38 to move to America. Fermi had no less than five offers of professorships at American universities, of which he chose Columbia. So, it was very fortunate that, at the same time, there was the Nobel Prize, and it was very convenient to go from Rome to New York via Stockholm. And the prize, of course, was very useful for starting a new life.

Neutron research led to many surprises. It turned out that, if you (as I remember it from the tales, since I wasn't there) put the sample on top of a wooden table, the radioactivity was stronger than if you put it on top of a marble table. Of course, everything in Rome was of marble, if it wasn't of wood. *(laughter)* And so, I guess they got the idea that maybe different surroundings might make a difference, and so instead of using a lead box around the sample, they decided to use a paraffin box. And the paraffin box was tremendously effective. The radioactive count increased about 100-fold with most of the elements. That was a great surprise, of course. And Fermi, having discovered that in the morning, went to lunch, and over lunch he decided what was the reason for it: namely, obviously the hydrogen, which was in paraffin and in wood, would slow down the neutrons, and he easily figured out that the cross section for interaction of neutrons with elements should go approximately as one over the velocity of the neutrons, so slowing them down would increase the cross section tremendously. It was one of the many examples of Fermi doing physics.

There was a strong variation between elements in the cross section, and I produced a wrong theory of that, which I then used to open my American career at the American Physical Society. In this theory, I assumed that it was the one particle wave function of the neutron, which was responsible, and that could have a resonance in the nucleus if it fitted well into the nucleus. Well, that theory was wrong. It later came back in the cloudy crystal ball of Weisskopf and collaborators. It had something to do with nature, but the important interpretation was found by Niels Bohr; it was the compound nucleus. It was so the Fermi experiments on neutron absorption in nuclei led to Bohr's compound nucleus, which was then, for many years, the most important tool of explaining nuclear reactions generally and was exceedingly successful.

Resonances were discovered. It turned out that, if you interposed a silver foil, then the activity in silver would be greatly diminished, but the activity in, let's say, indium, would not. And so resonances were indeed predicted by Bohr's compound nucleus theory, and later played a great role in nuclear physics and in particular in the development of the chain reaction. Neutron physics became a big field of research. Fermi obviously was the pioneer, and for it, he got the Nobel Prize, and it kept him busy for many years thereafter.

Thank you. *(applause)*

Carl Sagan: We seem to be a little ahead of schedule and Hans says he'd be happy to take some questions if there are any on history or physics.

(question from audience): "Did Fermi comment at all on the discussions between Niels Bohr and Albert Einstein on quantum mechanics?"

Bethe: Not to my knowledge. That was not the kind of thing he would get engaged in. *(laughter)*

Sagan: Other questions? Comments? Yes. . .

(someone in audience speaking): It's mentioned that in Segré's book on Fermi, at the end of Fermi's life, he still had the sense that the final word had not been said on quantum mechanics.

Chapter 19
Fermi and the Nuclear Age

Albert Wattenberg
University of Illinois

(Al's Cornell talk mainly dealt with Fermi's work with nuclear reactors and neutron beams at the Argonne National Laboratory and the achievement of the first nuclear chain reaction at the University of Chicago. Some of the more technical parts have been deleted and replaced by other less technical material given me by Al.—J. O.)

Early Work on a Nuclear Pile at Columbia University

I, Al Wattenberg, had joined Fermi's group at Columbia University as a grad student in 1942. It was Leo Szilard who had the foresight to enlist the help of industry in producing the required high-purity materials. Szilard was a brilliant visionary. In fact, he really was the inventor of the nuclear chain reaction. In 1933 Ernest Rutherford remarked in a public lecture that anyone looking to nuclear reactions for useful energy was "talking moonshine." Szilard took this as a personal challenge. Szilard had maintained the hope (first expressed by H. G. Wells) that a nuclear bomb could lead to a more peaceful world. As early as 1934, Szilard took out patents relating to possible nuclear chain reactions. In his 1936 patents Szilard proposed using neutrons to initiate a nuclear reaction that might emit enough initial neutrons to start a nuclear chain reaction. He sought financial support to develop these hypothetical processes, and he arranged for physicists like Fermi to perform relevant experiments.

In January 1939, when Niels Bohr brought the news of Otto Hahn's discovery of uranium fission to Washington, Szilard was already primed to use the fission reaction to implement his ideas on nuclear chain reactions. Fermi had just arrived in the United States to work at Columbia. He had gone to Stockholm the previous month to receive his Nobel Prize with the secret intention of leaving Fascist Italy for good. Szilard cajoled Fermi and Anderson into working with him and Walter Zinn at Columbia. Thus in 1939 and 1940 the Columbia group consisted of Anderson, Fermi, Szilard, and Zinn.

The neutrons emitted in the fission process are fast (higher energy), but the cross section for fission is much greater for slow than for fast neutrons. Now there was the problem of how one might slow down the neutrons emitted during fission. Another problem was to minimize the absorption of neutrons in parasitic nonfission processes. The Columbia group decided to use graphite as the moderator to slow down the neutrons. The first experiments using stacks (or piles) of graphite blocks were discouraging. The problem was the presence of impurities in the graphite. Szilard was a chemist as well as a physicist, and he knew where to go to obtain highly purified graphite. Now with the improved graphite, they obtained encouraging results.

The following is a typical (and true) Szilard story. In the 1940s he wrote a set of memoirs, calling them "My Version of the Facts." He showed them to Hans Bethe, who asked him what he intended to do with the material. Szilard said he wasn't going to publish it; he just wanted God to know the facts. When Bethe asked, "Don't you think God already knows the facts?" Szilard replied, "But He may not know my version."

The basic structure consisted of a rectangular lattice of uranium oxide "spheres" imbedded in equal layers of graphite blocks. The entire structure was called a pile. If the effective neutron multiplication factor is $k = 1$, the production of neutrons would be self-sustaining and nuclear energy would be released at a steady rate. One goal would be to obtain a large value for k and then the energy would be released at a fast, increasing rate, i.e., a nuclear explosion. The first piles were assembled in the physics building in a highly populated area in New York City.

CP1 in Chicago

The name CP1 stands for Chicago Pile 1. As the values of k grew closer to 1, the project needed more space and was moved for this and other reasons to a lower-populated region of Chicago (under the west grandstand of the abandoned football stadium at the University of Chicago). Now Fermi was in charge of a group of about 40 physicists and he devised a scheme on how to predict days in advance just when k =1 would be reached and how to slow down the rate of increase of energy release when k was greater than 1. In addition, there were redundant safety devices that would quickly turn off the reactor if the rate of energy release would exceed a preset low value. In the minds of anyone who had any knowledge of the situation, it was a completely fail-safe, quantitative experiment. Fermi, himself, insisted on participating in every stage of the experiment. Even the dirty jobs such as the machining of the graphite blocks. There is a scene in the film *The World of Enrico Fermi* of a machinist stripped to his waist wearing safety glasses encountering a black cloud of graphite dust, and that person is Enrico Fermi. As new layers were added to the pile the counting rate of a neutron counter inside the pile would increase as shown in Figure 26a. The critical layer is that layer when k becomes just slightly greater than 1. Then the value of N would reach infinity, or 1/N would reach zero as shown in Figure 26b. This figure shows that the pile would go critical when layer 56 was complete. Even when there were 10 layers less, one could predict the correct value for criticality days in advance. On the morning of December 2, 1942, just after layer 56 had been installed, the counting rate increased in a way to indicate that criticality had been reached. That is when Arthur Compton, the project administrator, phoned James Conant with the cryptic message "The Italian Navigator has arrived in the New World and found the natives to be friendly." It was indeed the birth of a new age for both weapons and nuclear power.

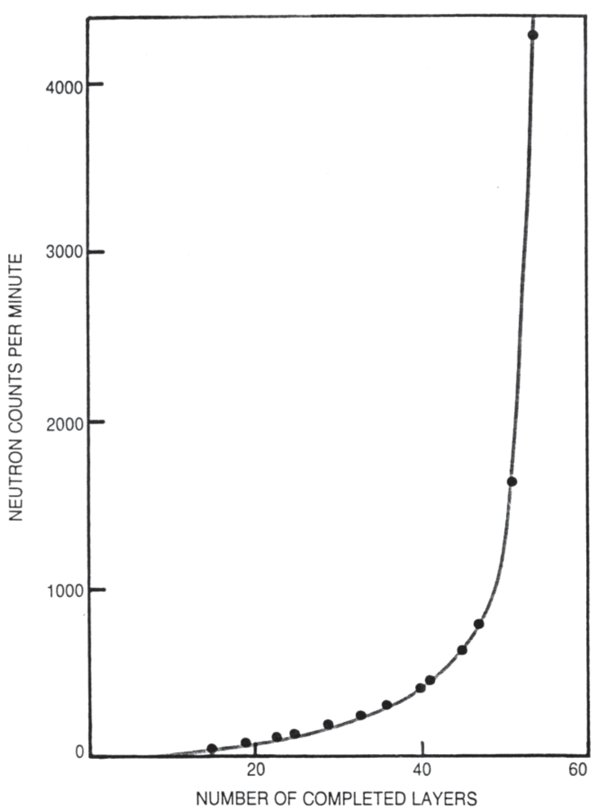
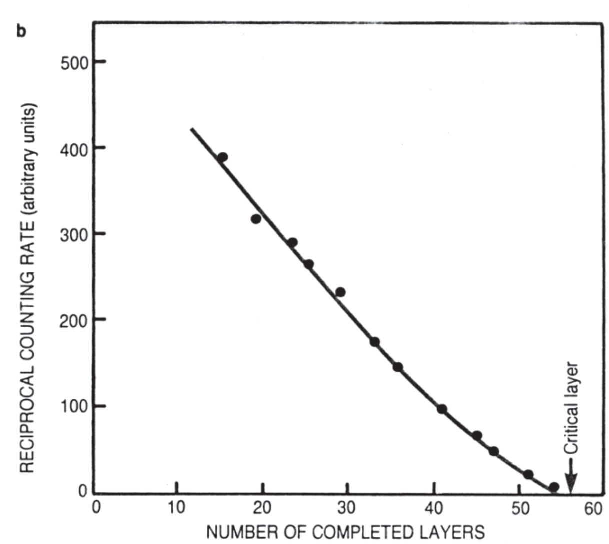


Figure 26. (a) Plot of neutron counting rate, N, versus the number of layers in the pile.



(b) Same as in (a) except that 1/N is plotted versus the number of layers.

Neutron Beam Experiments in the 1940s

The Fermi experiments in the 1940s include some that aren't well known, in fact, very unappreciated because of publication problems. The reason that some of things aren't known about some of the work that was done at the Argonne National Laboratory was because of declassification primarily, and this gives you an example of the problems that arose *(shows a transparency listing several experiments involving nuclear reactors)*. This is from the first of November, 1944—it was not declassified until 1956; in other words, these documents, and some of the experiments that were very important, never became public in the *Physical Review* until the collected works of Enrico Fermi were pulled together in 1962, by which time most people didn't even know that that was where it started. The reason, of course, is that shortly after the first burst of publications—1946, '47, '48—we had the likes of Senator Joseph McCarthy and Admiral Lewis Strauss. Essentially, they clamped down on the declassification.

If you look at this particular experiment *(points to one on the list)*, it has here the reflection of thermal neutrons from mirrors. In June of 1944, one of Fermi's greatest toys got on the air, namely, the heavy water reactor at Argonne National Laboratory. This provided the opportunity for enormous beams—very precise beams of neutrons. One of the first things he did that July was to set up a beam of 1 millimeter vertical height using slits with a 1-millimeter vertical gap at the beginning and about 5-millimeter gap further downstream, and then they went out a kilometer beyond the back of the pile, and there was this beautiful, well-defined beam of neutrons. There were 700 per minute in that beam, even though there was a lot of air scattering of the neutrons and attenuation. The other thing is, according to my recollection, it was asymmetric; namely, there were more neutrons sort of going down, and that was an observation that neutrons are also attracted by gravity—they're not anti-gravitational or anything else. Anyway, we didn't play with that. What Fermi had done—while the heavy water reactor was being built by Zinn—was that he had arranged for the Chicago machine shop to make some mirrors of graphite, aluminum, beryllium oxide, things of that nature—because what he wanted to do was show that neutrons—well, he wanted to determine something, namely the scattering amplitude—but what the experiment was to show is that neutrons obey the same laws of reflection as do light rays. Namely, if you have an index of refraction, less than 1, then there will be total reflection. Just identical to light rays. He wasn't sure which elements were going to have such an index and which ones would turn out to have an index greater than 1—that would be his real interest in the problem.

Anyway, let me show you the mirror setup *(shows diagram)*. As I say, the work was not declassified, so this diagram is from a similar mirror experiment done years later by Don Hughes in 1952. The mirror is here, and then downstream you have the counters. The critical angle for total reflection depends on the wavelength of the neutrons.

This next diagram is what Fermi observed, and it's published in that monthly report. And it shows this is the beam of neutrons—the original beam—and it shows that, as you tilt the mirror and go further out with your counter, you see a sharp peak. You move the mirrors and you move your counter, and you see the peak come up where you start seeing the neutrons that are slow enough to have wavelengths that totally reflect and these are even longer wavelengths, slower neutrons. You get lectures on quantum mechanics and other things, but when we saw the total reflection of the neutrons, it was the most convincing thing that neutrons are both particles and waves—you really got to believe it when you start seeing that. The other things we first saw were a little more obtuse. But we had seen phenomena in the first graphite reactor.

After the war, there seems to have been a palace coup that occurred, because let me now show you how the laboratory had changed—this is 1947, it's a quarterly report. Fermi is no longer the director of the laboratory—Walter Zinn is the director of the laboratory. Again, this was declassified nine years later.

If you look inside, Fermi's name doesn't appear with any authority at all, but if you look in Group 6, which was my group—I'll show you page 27—you'll see that one of the people I was fortunate enough to have working for me in my group was Enrico Fermi. *(laughter)* I want to go back to this list. This list of experiments that are here under my name are almost all being done by Fermi, and it shows you the type of thing he was doing. Using this heavy water reactor and studying the interaction of neutrons now with matter. It opened up a whole new field of solid-state physics. On the list of experiments led by Fermi is the cross section of hydrogen-deuterium molecules, using the scattering amplitude interference. Then he has the crystalline effects. The polarization experiment didn't work out well—we were trying to use a magnetite mirror. And then he has the interaction between neutrons and electrons. In that case, he was trying to look for the meson cloud in the neutron, using the electron as a probe. The molecular beam apparatus did not work for several years—it was to get the spins and magnetic moments of radioactive nuclei.

Fermi had a broad program at the Argonne National Laboratory. In 1947, volume 71, page 666, there's an article that really is the classic article that lays the foundation for all of the work that had gone on in the use of neutrons, the optical properties, the crystalline scattering, and interference phenomena for all of solid-state physics, chemists, and biologists. That article, which was published with Leona Marshall, is a revelation for chemists, physicists, etc. He publishes the scattering amplitudes of 22 different isotopes, showing it can be done. They did it with both the mirrors, with scattering, and with transmission and experiments having used a mechanical chopper that he built. So he really lays the foundations in that article for neutron diffraction, and the write-up of that experiment and the importance of that paper is given in the *History of Neutron Diffraction*, which was published just four years ago.

I want to finish with the fact that I was fortunate to have him as a worker, but one aspect of Fermi that wasn't so fortunate for me was that he was also my chauffeur. I would drive the car over to his house twice a week for a year and a half or so. Sometimes we picked up Leona (Marshall) and we would drive out to work together. And we kept talking about doing estimates of things—he was constantly estimating things. I tend to be an unbeliever in his intuition. It was that he had calculated things, and he remembered what he'd calculated. These little tiny estimates of things—he was always doing it, and he remembered them.

Anyway, one of the days we were at a railroad crossing, and he was the driver. I forget what we were calculating in our heads at the time, but anyway, the train went by and, of course, we went ahead because we have the calculation on our minds. It was two tracks, not one, and we almost got hit by the train coming in the other direction—missed by three seconds. *(laughter)* And I don't know whether he was jocular about his fatalism or not, but he said, "You see? It is exceedingly important that you always be with me when I drive." *(laughter)*

Fermi as a Teacher According to Wattenberg and then Yang and Feld:

I was a graduate student at Columbia in New York City when Fermi arrived in 1939. I registered for his quantum mechanics course, and in subsequent years Fermi was my teacher in three formal courses. There was a clarity and logic in Fermi's presentation that made his lectures very easy to follow. He minimized proofs and topics that would divert the flow of thought. He knew what was important and what could be neglected, and his brief plausibility arguments were very persuasive. Since he did not use textbooks, students had to take notes. It was difficult to recollect all the arguments from pure memory. His homework assignments were of an analytic nature and frequently were applications to physics problems that required numerical answers. He put effort into preparing notebooks for his lectures.

According to Yang:

> As is well known, Fermi gave extremely lucid lectures. In a fashion that is characteristic of him, for each topic he always started from the beginning, treated simple examples, and avoided as much as possible "formalisms." (He used to joke that complicated formalism was for the "high priests.") The very simplicity of his reasoning conveyed the impression of effortlessness. But this impression is false: the simplicity was the result of careful preparation and of deliberate weighing of different alternatives of presentation. In the spring of 1949 when Fermi was giving a course on nuclear physics (which was later written up by Orear, Rosenfeld, and Schluter and published as a book), he had to be away from Chicago for a few days. He asked me (Yang) to take over for one lecture and gave me a small notebook in which he had carefully prepared each lecture in great detail. He went over the lecture with me before going away, explaining the reasons behind each particular line of presentation.

According to Bernie Feld in 1954 at the Fermi Summer School in Varenna:

> Here was Fermi at the height of his powers, bringing order and simplicity out of confusion, finding connections between seemingly unrelated phenomena; wit and wisdom emerging from lips, white, as usual from contact with chalk. . . .

(Wattenberg has closed with his own evaluations as well as those by Yang and Feld of Fermi as a teacher and a person. It is remarkable how similar are nearly all the opinions about Fermi.—J. O.)

Chapter 20
Fermi at Chicago

Valentine Telegdi
University of Chicago and CERN

(Telegdi spent part of 1990–91 in Chicago doing research on Fermi. He asked me and some other associates of Fermi to send him personal recollections. I sent several pages and asked him to give a physics colloquium at Cornell in return. It turned out to be most convenient for him to come in 1991 at the time when Dick Garwin was to be at Cornell as the Bethe Lecturer. This evolved into the 1991 Cornell Fermi Symposium. The following is our transcript of Telegdi's Cornell talk. We thank Telegdi for permission to print our transcript of his talk. Both he and Bethe were invited to give one-hour talks. Views of Telegdi appear in Figures 2 and 4.—J. O.)

1. Introduction

None of the great scientists who worked at Chicago ever had a greater impact on his immediate and worldwide surroundings than did Enrico Fermi. Nobody in the history of modern physics possessed greater versatility than he. He had just as great achievements in pure theory as in concrete experimental work. He could with equal ease solve abstract problems or design and build with his own hands astonishingly useful experimental tools. (See Fermi's trolley in Figure 9.) He was, as one of his best Chicago students, M. L. Goldberger put it, the "Compleat Physicist." To these qualities he added those of an exceptionally lucid lecturer and expositor as well as an active and patient thesis supervisor. It is imaginable—hypothetical situations are by definition hard to evaluate objectively—that some other physicist (or group of physicists) might have obtained the research results that Fermi achieved while in Chicago (including the first nuclear chain reaction), but it defies the bounds of human inspiration to speculate that any other man or woman might have played Fermi's role as a teacher in the broader sense of this term. Through the influence of his students, Fermi effectively revolutionized the training of students in the United States and one hopes in the whole world.

Ampere (1775–1836) was, like Fermi, a universal genius, but there the analogy stops. He was, to use de Broglie's own words, a "tormented genius," much influenced by the vicissitudes of his personal life and much given to philosophical, yea even metaphysical speculations. Fermi was an as, well, balanced, dispassionate person as one can imagine, little interested in matters outside (all of) physics. For this reason, we shall concentrate on Fermi's professional activities, i.e., his research (Section 2) and his teaching (Section 3), in discussing these, we shall furthermore confine ourselves to his two Chicago periods, thus omitting some of Fermi's greatest glories, e.g., Fermi statistics, the theory of beta-decay, and his initial work on neutron-induced reactions (that led to his Nobel Prize), which were achieved in Rome. The reader is referred to the booklet "E. Fermi, Physicist" by E. Segré for a more balanced picture.

Notwithstanding our (and Fermi's!) preference for physics over psychology and sociology, we offer a short section (No. 4) about Fermi's human personality and his style of work. To those who do not have the good fortune of having known Fermi personally, the few anecdotes reported in that section may convey a better feeling for his nature more than any literary effort on this writer's part could. In this context the reader is advised to peruse the charming, but objective book *Atoms in the Family* by Laura, Fermi's wife. Note that that account was written in Fermi's lifetime. Fermi joined the Chicago faculty in January 1946 and died at Billings Hospital in November of 1954, six weeks after being admitted. He was felled by a multiply metastasized cancer that had escaped early detection. He was a physically strong person and only 53 years old.

2. Fermi's Research at Chicago

Enrico Fermi spent two distinct periods at the University of Chicago. During the first of these, from spring '42 until September '44, he was the key figure of the Metallurgical Laboratory, the top-secret wartime project aimed at developing nuclear reactors and, ultimately, the atomic bomb. This work culminated on December 2, 1942, in the start-up of the first reactor or "pile," a graphite-uranium assembly erected under the West Stands of Stagg Field (this reactor was soon removed to the site of Argonne National Laboratory, and Stagg Field yielded its terrain to the Regenstein Library). Today the approximate location of that first reactor is marked by an abstract statue due to Henry Moore. It is safe to speculate that if the city of Chicago should ever be destroyed (possibly as an ultimate tragic consequence of the work done in the Metallurgical Laboratory), a new monument would be erected on the same spot to commemorate forever the place where man first unleashed nuclear energy.

The work headed by Fermi in the Metallurgical Laboratory is described in over 60 declassified reports. These were preceded by some 17 analogous reports, all already directed toward the goal of achieving a self-sustaining chain reaction, which Fermi and his collaborators wrote at Columbia University from the fall of 1940 on. Although Fermi's style, about which we shall say more later, emerges unmistakably from any report that carries his name, there is a marked difference between these two series. At Columbia, Fermi directed a small group of physicists and participated in even the most menial experimental tasks. The experiments were on a small scale, and the theory was in a field where Fermi was already the accomplished master. In Chicago the project assembled a large group of scientists and engineers from various fields. Fermi had to assign the execution of experiments to various subgroups (he called this doing "physics by telephone"), but he generally evaluated the data by himself, mostly in his office in Eckart Hall, because he preferred his knowledge to be firsthand. His efficient leadership was greatly enhanced by his marvelous powers as a lecturer; he instituted a series of lectures on neutron physics. Later, at Los Alamos, he gave a more extensive course, complete with homework problems. The transcripts of all these lectures, available in his *Collected Papers*, are good introductions well worth reading even today.

At the tenth anniversary of the first chain reaction, i.e., after returning to the University of Chicago, Fermi published an extensive paper on the construction and start-up of the first pile. Nothing in that paper reveals anything about the dramatic nature of the event. In fact, on about December 15, 1942, Fermi wrote tersely in a progress report: "The activity of the Physics Division [of the Metallurgical Laboratory] in the past month has been devoted primarily to the experimental production of a divergent chain reaction. The chain reacting structure was completed on December 2 and has been in operation since then in a satisfactory way."

A personal account of Fermi's "matter of fact" attitude at the critical moment has been given by his closest associate, H. L. Anderson:

> The next morning, December 2, I came bright and early to tell Fermi that all was ready. He took charge then. Fermi had prepared a routine for the approach to criticality. The last cadmium rod [control element] was pulled out step by step. At each step a measurement was made of the increase in the neutron activity, and Fermi checked the result with his prediction, based on the previous step. That day his little 6-inch slide rule was busy

> for this purpose. At each step he was able to improve his prediction for the following. The process converged rapidly, and he could make predictions with increased confidence for the following step. So it was that when he arrived at the last step, Fermi was quite sure that criticality would be attained then. In fact, once the cadmium rod was pulled out entirely, the pile went critical, and the first self-sustaining reaction took place.

In September 1944, after witnessing the start-up of the first plutonium-producing pile (at Hanford, Wash.), Fermi left Chicago for Los Alamos, a laboratory he had frequently visited previously. Since this essay is devoted to Fermi at Chicago, we need not to say much about his activities there, mentioning merely a few facts. Fermi witnessed the explosion of the first atomic bomb on July 16, 1945. Characteristically he had foreseen a simple way to estimate the power of the bomb: At the appropriate moment, he dropped a few slices of paper and measured the distance to which the blast blew them. His estimate agreed closely with that obtained by the sophisticated "official" methods. At the laboratory, he was in charge of a division (quite aptly called F-Division) that concerned itself with special projects. E. Teller worked in that division on the "Super," i.e., the hydrogen bomb. It is conceivable that Fermi's postwar opposition to the actual building of such a device was based on technical knowledge gained during that period and subsequent summer visits to Los Alamos. In the summer of 1950 he shared an office with the mathematician Stan Ulam and Dick Garwin. Fermi and Ulam investigated the thermonuclear reaction in a mass of deuterium. They concluded that ignition would not propagate. *(Is this Telegdi's way of saying that Teller was unable to design a thermonuclear bomb? Now that E. Teller has given Garwin most of the credit for the first thermonuclear explosion, it would be hard to believe that Garwin did not consult with his office mates. I have been with Garwin and Fermi at times and conclude that their minds cannot help but resonate together. So, I at least, suspect that Fermi, also, is a partial inventor of the H-bomb.—J. O.)*

After V-J day, the original mission of Los Alamos had been fulfilled, and the unprecedented galaxy of scientists assembled there began to disperse. They were anxious to return to their customary academic habitat, but with a new attitude: the wartime effort had ushered in "Big Physics," the use of large-scale equipment and the availability of massive financial support. Fermi, together with a group of other brilliant senior scientists (e.g., Bill Libby, Cyril Smith, Leo Szilard, Edward Teller, Harold Urey) and their junior wartime associates (e.g., the physicists Herb Anderson, Bob Christy, John Marshall, Darragh Nagle, and the chemists Nate Sugarman and Tony Turkevitch) accepted offers from the University of Chicago. Some kind of "package deal" was involved (it is rumored that the same package had proposed themselves earlier to the University of Washington, but that that deal fell through). President Hutchins, by training a philosopher and personally not particularly drawn to the exact sciences, realized the immense potential of this "package." He found the means to launch three new research institutes: one for nuclear studies (today named after E. Fermi), one for the study of metals (now the James Frank Institute), and one for biophysics. All three were meant to be interdisciplinary and their scientific members to serve on the faculties of their respective fields. Fermi was expected to run the Institute for Nuclear Studies, but he gratefully left the formal directorship to S. K. Allison, a distinguished American-born physicist who had served the university already before the war.

Fermi arrived in Chicago on January 2, 1946 (exactly seven years after reaching the United States). He immediately took up his teaching—both in the classroom and by sponsoring graduate students—and research. The experimental facilities in the Physics Department being minimal at that time ("the shelves were empty"), Fermi realized his interest, conceived in 1943, to exploit the intense flux from a reactor for experiments in neutron physics. The CP-3 reactor, at the original Argonne site, was well suited for this purpose (Argonne was then still a section of the Metallurgical Laboratory and became a National Laboratory only in the spring of 1947). Nine remarkable papers, all but one produced in collaboration with Leona Marshall (née Woods), came out of this research over a period of two years.

All of these have the hallmark of Fermi's style: extreme economy of technical means, efficiency of execution and self-contained theoretical discussion, formulated in the most elementary terms. The most interesting of these investigations was a search for an interaction between the neutron and the electron, i.e., in modern terms a possible determination of the charge radius of the neutron (this particle being only overall electrically neutral). That experiment was unfortunately not sensitive enough to give a positive result—it yielded only an upper bound for the quantity sought. At this rare occasion, Fermi was "scooped" twice: on the one hand, Rabi and his collaborators Havens and Rainwater succeeded in obtaining, almost simultaneously, concrete results using a different technique, while on the other L. Foldy could prove by a simple theoretical argument that the known magnetic properties of the neutron lead without any specific model to the existence of the sought electron-neutron interaction.

After these neutron physics investigations, Fermi's personal participation in experiments came temporarily to an end. He returned to what he considered to be his main vocation, theoretical physics, focusing his interests on entirely novel topics. In the years 1946–47 some of the most exciting results come from cosmic-ray physics, and this primarily from experiments done in Europe. A group of young physicists in Rome reported an extraordinary anomaly: the negative "mesotrons" (the old name for mesons) when brought to rest in carbon did not appreciably get absorbed by the carbon nuclei as they were expected to do—*if* they were indeed (as their mass had suggested and as had universally been believed since their discovery in 1937) the mesons postulated by Yukawa as the quanta of the nuclear force field (i.e., the carriers of the strong nuclear interaction, to use modern parlance) but decayed in about 10^{-6} seconds. Fermi and Teller, and independently V. Weisskopf at MIT, gave convincing arguments that this could not be explained in terms of some anomaly in the slowing-down process, as had been conjectured by some very eminent physicists, and estimated that process to be 10 million times faster, i.e., to last but 10^{-13} seconds. The three authors published a joint Letter, followed by an extensive article by Fermi and Teller. Shortly thereafter, Occhialini, Powell, and their collaborators discovered, examining in Bristol tracks produced by cosmic rays in photographic emulsion, that the cosmic-ray "mesotron" (now called muon) was but the decay product of a heavier particle (now called pion) which indeed exhibited the properties of the particle postulated by Yukawa. Incidentally, the terms "pion" and "muon" were coined by Fermi. These particles were previously called "pi meson" and "mu meson" respectively.

Thus two new fields opened up: pion physics, the study of the interactions of the Yukawa particle, and muon physics, the study of the heavy electron (which the muon turned out to be). The Bristol discovery was quickly followed by the production of "artificial" pions (positive, negative, and neutral) at the Berkeley accelerators. The era of high-energy physics had begun. It was decided to equip the new Institute for Nuclear Studies with a 450 MeV synchrocyclotron; its construction was directed by H. L. Anderson (Fermi's closest associate since Anderson's graduate student days at Columbia) and John Marshall (also a wartime associate). This accelerator, with its experimental area specifically laid out for experiments with external meson beams, started operating in the spring of 1951. It was, for a few years, the highest-energy accelerator in the world! Fermi contributed in many ways to the cyclotron project. He calculated the orbits of the pions from the production point ("target") to the experimental area using the MANIAC electronic computer at Los Alamos (Fermi immediately realized the potential of electronics computing and became fluent in writing programs in machine language). He also designed and built a small electrical cart with which one could readily move the target around the periphery of the cyclotron, in a region of high magnetic field; the latter served as the stator for the cart's motor. Fermi was quite proud of this device, universally called the "Fermi trolley" (see Fig. 9). He also devised a simple way to measure the intensity of the internal beam through the energy deposited in, i.e., the temperature increase of, the target (generally a piece of beryllium metal).

As was implied above, Fermi's prime research in 1947–51 was theoretical. The first major paper was one on the origin and acceleration of cosmic rays, where Fermi advanced the idea that a galactic magnetic field played the key role in the acceleration mechanism. This paper was stimulated by E. Teller (often Fermi's favorite intellectual sparring partner) who has argued, together with H. Alfven, that cosmic rays originated in the solar system. Fermi presented this work at the Basel/Como conference in September 1947 and returned to the subject in later years. Another remarkable paper was one that Fermi wrote in collaboration with C. N. Yang (formally a student of E. Teller's), entitled "Are Mesons Elementary Particles?" Conventionally, it had consistently been assumed that pions and nucleons had the same mutual relationship as photons (light quanta) and electrons, i.e., that of a field and its source. Fermi and Yang advanced the bold hypothesis that the pion is a *bound state* of a nucleon-antinucleon pair. This hypothesis, neither readily verifiable nor very useful in itself, subsequently paved the way for several radical ideas in the theory of elementary particles, e.g., "nuclear democracy," "bootstrap," etc. Today, we picture the pion indeed as a bound state of a quark-antiquark pair! The next theoretical problem that Fermi attacked was to provide simple estimates as to what would happen if, say, a proton hit a nucleus at extremely high energies (such as were available then only in cosmic ray induced events), e.g., with what probability a given number of pions would be produced in a single collision. In doing this, Fermi discarded dynamical consideration entirely and based his deductions solely on statistical arguments. Incidentally, although Fermi's versatility was and is legendary, it is probably fair to assert that he had a deeper feeling for statistical methods than for any other subject (indeed, the "Fermi statistics" constitutes probably his most lasting theoretical contribution). As always, Fermi was fully aware of the limitations of his simplified model and meant it to serve only as a guideline. He hence resented it when experimental departures from his predictions were raised as serious objections.

During the summers, Fermi liked to return to Los Alamos, where he served as a consultant. There he worked on a radically different class of problems (we can obviously discuss only what has been declassified). One of these concerned the famous "Taylor instability," a subtle problem in hydrodynamics. With his characteristic gift for simplicity, Fermi first discussed this phenomenon in terms of a glass of water turned upside down. Subsequently, partly in collaboration with John von Neumann, he wrote a very technical paper on the subject, extending it to the case of two incompressible liquids. What a wrestling of titans that collaboration must have been! An anecdote, told by the late H. L. Anderson to the present writer, can give us a hint: One day, after spending a whole afternoon at the blackboard with von Neumann, and thereby completely exhausted, Fermi met Anderson and said to him, "Herb, that guy Johnny knows as much more about differential equations and all sorts of mathematics than I, as I know more than you. . ." (Recall that Anderson was a pure experimentalist.)

Once the institute's large synchrocyclotron began operating routinely, Fermi returned to experimentation. Before we discuss this important phase of his work, let us pause to mention the outstanding textbooks he produced while at Chicago. The first of these was *Nuclear Physics*, an extensive set of lecture notes compiled by his students Orear, Rosenfeld, and Schluter. It is a classic, a compendium of simple (or at least seemingly elementary!) solutions of all the relevant nuclear problems of its time. It is still of value today. The second was *Elementary Particles*, the written version of his 1950 Stillman Lectures at Yale University. Because of the explosive development of our factual knowledge of "elementary" particles, this work is today primarily of historical interest. Fermi also planned to produce an American version of a high school physics textbook that he had published much earlier in Italy. Because of frequent disagreements with an "educational expert" appointed by the publisher (who, for instance, refused the use of a vector notation) Fermi, who was extremely punctilious about the text of his publications, abandoned this project. What a boon to U.S. education this book would have been, especially after Sputnik, when the country suddenly realized the low level of its high school education. *(Telegdi obtained this information from Prof. R. A. Schluter of Northwestern University. I also had received similar information from Fermi. He told me about the three-volume high school text he had written with Laura as a co-author and he gave me a copy. He also told me that he and Laura were planning a revised edition, presumably to be both in English and Italian. I never learned the outcome of this project, but I did receive in the mail a new three-volume high school text by Amaldi and his wife that seemed somewhat similar to the earlier text by Enrico and Laura. I am not sure, but I think I mentioned to Enrico that I had received the Amaldi complimentary copy just after it had arrived. I don't recall any response other than a glum look. It may be that Enrico did not want to discuss the subject any further. I do not recall him ever speaking negatively of any of his co-workers.—J. O.)*

We now turn to Fermi's experimental work with the cyclotron. This concerned almost exclusively the interaction of pions (both positive and negative, designated as π^+, π^-) with protons, i.e., the transmission of pions through, and the scattering by, liquid hydrogen. Some measurements of this kind had been done slightly earlier, at lower energies, at the Nevis (Columbia) cyclotron, and D. E. Nagle (as mentioned, already associated in wartime with Fermi) was the first to propose such measurements at Chicago. Fermi undertook the pion experiments in close collaboration with Nagle and H. L. Anderson: in the earliest stages of the work Dr. E. A. Long, of the sister Institute for the Study of Metals, contributed his expertise in the construction of cryogenic targets. Occasional collaborators were a visitor from Norway, Arne Lundby, and G. B. Yodh, R. Martin, and M. Glicksman, three good graduate students.

The work described in a series of nine experimental papers led Fermi and collaborators to two outstanding discoveries: (a) the nucleon (i.e., both the proton and the neutron) had an *excited state*, with an excitation energy of some 180 MeV; (b) the pion-nucleon interaction obeyed a symmetry principle, "charge independence" (already sketchily known from nuclear physics), which is characterized by a new conserved quantity, "isotopic spin." The excited state manifests itself as a peak or "resonance" when the probability of interaction (cross section) is plotted versus the energy of the incident pion beam. An explanation in these terms by K. A. Brueckner had anticipated some of the most striking data of Fermi's group by several days. According to Herb Anderson, "In fact, Fermi could (and did) read the preprint of Brueckner's paper the very day he found the [astonishingly] high [π^+P] cross section. Brueckner had seized upon isotopic spin as being an essential element in the pion-nucleon interaction. Arguing that the dominant state was one with total angular momentum 3/2 and isotopic spin 3/2, all the features of the experiments could be understood at once. It took hardly more than a glance at Brueckner's paper for Fermi to grasp the idea by himself in his office, he emerged with this happy conclusion. 'The cross sections will be in the ratio 9:2:1,' he announced. He referred to the π^+ elastic, π^- charge exchange, and π^- elastic processes, in that order."

As seductive as the resonance hypothesis was, Fermi could not consider it proven until he had completed the detailed analysis of the angular distributions in terms of certain parameters (called "phase shifts"). He performed the requisite numerical analysis, in collaboration with N. Metropolis and E. F. Alfei, on the MANIAC electronic computer at Los Alamos. Unfortunately, at least two possible fits emerged: the one favored by Fermi did *not* correspond to the proposed resonance. *(The word "favored" is unclear in this context. See Chapter 11 for a more detailed discussion.—J. O.)* The delicate matter of the correct solution was settled, including data from other laboratories, only after Fermi's death (confirming the excited state).

Fermi looked for possible additional *experiments* that could distinguish between the various solutions alluded to above. He realized that the polarization of the recoil proton in the scattering experiments, i.e., the orientation of the proton's intrinsic angular momentum (spin) with respect to the normal to the scattering plane, would have a high discriminating power. In a short theoretical paper, one of his last, he showed that this was indeed the case and that large effects were to be expected.

Even in the midst of the excitement of his experiments on pions, Fermi took time to further his theoretical interests in other areas of physics (in the broadest sense). Thus during the fall of 1952 and in the winter and spring of 1953, Fermi met with S. Chandrasekhar once a week for two hours to discuss a variety of astrophysical problems related to hydromagnetics and the origin of cosmic radiation. Two major joint papers came out of these discussions, one titled "Magnetic Fields in Spiral Galaxy Arms," the other "Problems of Gravitational Stability in the Presence of a Magnetic Field." The first of these bears the stamp of Fermi's power to obtain estimates by simple means, the second of Chandrasekhar's analytic virtuosity. What a fertile meeting place the Institute for Nuclear Studies was! Fermi returned to these topics in August 1953 in his invited H. N. Russell Lecture to the American Astronomical Society. He was the first (and probably the only) nonastronomer to be so honored and was quite pleased by this appreciation coming from outside his field.

We have already mentioned Fermi's particularly deep feeling for statistics, or more precisely for the theoretical study of the behavior of systems composed of a large number of identical objects, e.g., molecules or mass points (statistical mechanics). In the summer of 1953 Fermi, in collaboration with J. Pasta and S. Ulam, decided to check by a computer experiment whether the standard conjecture that in such a system the energy would be shared, after some time, equally among the mass points was indeed fulfilled ("trend towards equipartition"). Surprisingly, the result was negative. This calculation was completed and published only after Fermi's death.

Fermi's interests and contributions during his postwar Chicago period ranged even farther than can be deduced from his publications. Maria G. Mayer, who received (together with J.H.D. Jensen) the Nobel Prize for proposing the correct shell model of nuclear structure, acknowledges that she was put on the right track by a single crucial question raised by Fermi (who, characteristically, does not refer to this fact in his own published discussion of that model!). Not reading the literature, Fermi sometimes invited experts to bring him up to date o some topic of current interest. One of these experts was V. Weisskopf, who lectured for several afternoons to Fermi and a select group of physicists on the "Collective Model" of nuclei. During these lectures, Fermi would occasionally extract from his coat pocket a very large sheet of paper, covered with algebra and multiply folded, and compare it silently with Weisskopf's writings on the blackboard. At the end of the lecture series, he simply said, "Well, this all agrees with what I already know. . ." Another lecturer invited by Fermi was Richard Feynman (well known to him from their Los Alamos days), who gave several talks on liquid helium. Last but not least let us mention Fermi's interest in superconductors, both theoretical and technical. It was he who got Berndt Matthias (then on the Chicago faculty) interested in high-temperature superconductors, by raising at lunch the question "Would it not be enormously important to have superconductors at, say, liquid hydrogen temperature?"

No single individual in this century has contributed so much to physics, through theory as well as experiment, as did Enrico Fermi. Still, in this writer's opinion, his greatest contribution in the Chicago period lay in his teaching. Through his students and their teachings, the Fermi spirit is still alive today.

3. Fermi's Teaching

(At this point Teledgi projected a slide showing all the courses taught by Fermi while a professor at Chicago. They ranged from the introductory three-quarter course to advanced courses in quantum mechanics, thermodynamics, nuclear physics, and physics of solids. The usual teaching load was one course per quarter for three out of the four quarters. In his seven years there he should have taught 21 courses, but actually he had taught 23 courses [in the summer quarter of 1949 he taught three courses in a row at 8:00 a.m., 9:00 a.m., and 10:00 a.m. to earn future teaching credits.] Fermi's goal was to teach every course in the physics curriculum, and he encouraged others to do the same.—J. O.)

What is remarkable about this list is the variety of subjects taught by Fermi and the fact that he generally carried more than his share, as was his custom in any undertaking he joined. Faculty members were expected to give service for three quarters per year and were only paid for the same. As we mention earlier, Fermi generally spent his summers as a consultant at Los Alamos. Fermi once taught the big sequence of general (i.e., introductory) physics courses, although (or because) he abhorred the humanistic approach to science teaching that prevailed then in the college (they want to discuss how Galileo thought but not teach what he thought about).

We have already mentioned Fermi's legendary talent for classroom teaching. His simplified exposition of any subject was no accident; it was the fruit of careful preparation. Many a time one could see him, well in advance of the appointed hour, going up and down in front of the classroom consulting a sheet covered with formulae. Fermi seemed to derive pleasure from the *act* of teaching, without regard for the result. He never showed annoyance at a student's failure to grasp for the first time (or even the second!) what Fermi was trying to teach. On the contrary, if Fermi had to repeat an explanation he seemed to derive twice the pleasure. An apparent corollary was Fermi's disinclination generally to evaluate students. One of his former students has conjectured that all of the students at Chicago were so inferior to Fermi in talent that he could not (or did not think it useful to) recognize differences between them.

Fermi's style of lecturing was not entirely above criticism and differed radically from his private approach to working problems. (In class, he often chose to discuss general problems in terms of specific examples, with all factors carefully adjusted to be of order unity—and hence rapidly dropped). In his own calculations, generally performed on 2 x 3 drafting sheets—far from the proverbial back of an envelope—all factors were carefully kept, even those which by convention are often put equal to one. He delighted himself in giving simple derivations of results which on the part of others required elaborate calculations—but he occasionally sidestepped certain topics for which he too did not have a very elementary argument (e.g., the Thomas precession in atomic physics). His lucid lectures had an almost hypnotic effect; in class, the student felt that he had understood everything, but subsequently often felt empty-handed. The present writer found those of Fermi's lectures most fascinating that covered familiar notions. It was like "the view of a landscape as seen by an eagle—all remarkable points stood out clearly."

In several of his courses Fermi handed out mimeographed notes before each lecture. These contained mostly formulae and little text. Fermi said that he did this because he personally was unable to listen and take notes at the same time. For this reason he had hardly ever taken any notes during his student years at the University of Pisa; some of these mimeographed notes (e.g., *Quantum Mechanics*) were subsequently published by the University of Chicago Press in book form. In our opinion, they do Fermi's memory a disservice: they present the formulae but not Fermi's comments. It is like showing a skeleton instead of a full-length portrait.

Fermi's way of thinking about, and teaching of, quantum mechanics deserves a special mention. His attitude was an entirely pragmatic one: Quantum mechanics is acceptable *because* its predictions agree with experiment. He once said "the Schrodinger equation has no business agreeing so well. . . ." Nothing else counted. He devoted no time to such topics as "the quantum theory of measurement." He was immune to the "Copenhagen spirit," both by temperament and by educational background. He was completely self-taught in quantum mechanics, an outsider to the Gottingen-Zürich-Copenhagen circle of its founders. It may be supposed that Fermi always needed to draw a firm line between physics and "philosophy." Although endowed with remarkable analytic powers, Fermi often affected an aversion to abstract mathematics. Two anecdotes may serve to illustrate his attitude: (1) Once a notice appeared on the bulletin board announcing a course on the fundamentals of quantum mechanics. This notice read, "Students should be familiar with the mathematics of Hilbert spaces and Banach spaces." Fermi commented, "Unfortunately I cannot learn about the fundamentals of quantum mechanics; I know about Hilbert spaces but not about Banach spaces." Even when a mathematical argument had played a role in his initial thinking about a problem, he was careful to erase all its traces from his final account. Chandrasekhar once was to talk in a seminar; when he expressed doubts as to what he should talk about, Fermi advised: "If I were you, I would not be technical." And Chandrasekhar asked, "Do you mean, if I were you, or you were me?" This baffled Fermi: it was the only occasion Chandra got the better of him.

We now turn our attention to Fermi's doctoral students. No other physicist has ever trained such a score of eminent pupils (in Rome *and* Chicago); one might object to this statement by mentioning Rutherford and Sommerfeld, but the first of these trained only experimentalists and the second only theoreticians, while Fermi trained both categories. *(At this point Telegdi showed a slide listing these students. The same list with photos makes up the caption to Figure 4 except for the absentees Lee, Yang, Chew, Goldberger, and Rayne. Steinberger and Garwin were not absentees, but they had to leave before the picture was taken. J. Friedman was there, but Fermi did not survive to the end of Jerry's thesis. Fermi was chairman of the Ph.D. committees of G. Yodh and L. Wolfenstein at the time of their Ph.D. orals. In fact, Fermi was both the theoretical physics and experimental physics member of Wolfenstein's committee. Telegdi pointed out that Fermi attracted outstanding graduate students, but brilliant junior faculty as well. People like M. Gell-Mann, R. Garwin, V. Telegdi, R, Dalitz, J. Cronin, etc. He failed to mention the attraction of talented undergraduates like Carl Sagan.—J. O.)*

4. Fermi's Personality

Fermi was completely devoted to physics, and his whole existence centered around it. He appeared to have very few outside interests such as literature or the fine arts. He engaged in sports, e.g., in mountaineering and tennis, but one often got the impression that it was all for "mens sana in corpore sano"—i.e., to be in the best physical condition for doing physics; it must be added that in sports as well as in parlor games (which he occasionally organized in his home) he liked to win, being fiercely competitive. His salient features as a scientist were absolute integrity, total dedication to the task, and an incredible gift for efficiency. He was a very clear thinker but not an exceptionally quick one (compared, say, to Landau or Teller). He solved simple and difficult problems at the same steady pace. In his dealings with others he displayed much reserve and great modesty—the latter in the sense that he "did not like to throw his weight around." A characteristic incident may serve to illustrate this. One day he needed an oscilloscope owned by somebody outside his own group. He asked one of his associates to go and fetch it—but added, "Don't tell him that it's for me." Fermi liked to pass as an ordinary man, a "man of the street," simply a good artisan who happened to specialize in physics. He liked to do what "ordinary people" (as opposed to highbrows) do: when American intellectuals, in the early fifties, ridiculed the possession of a television set, Fermi promptly bought himself one (and fell asleep in front of it by 10:00 p.m.).

Fermi had very regular working habits and a frugal lifestyle. He usually came to work before 8:00 a.m., in good weather either walking or biking. He had worked for several hours before. Fermi was totally secure in his own physics talent and almost never displayed jealousy of another. The only exception, as one of his students recalls, was Einstein. More than once Fermi expressed annoyance at the attention Einstein received from the press. He also disliked "high-class mannerisms"; once he commented about Robert Oppenheimer, "He was born with a golden spoon in his mouth." The day the Oppenheimer case broke, we were having lunch with Fermi at the Quadrangle Club. He said, "What a pity that they took him and not some nice guy, like Bethe. Now we have all to be on Oppenheimer's side!" Fermi's testimony at the hearings (Grey report) were, as expected, sober and not damaging.

Fermi hardly ever made disparaging comments about the scientific work of others. In the same vein, he refrained from laudatory remarks and rarely provided the encouragement that would have meant so much to the young people around. Curiously, during his first stay abroad, at Göttingen at age 23, he did miss receiving encouragement from Born. *I agree that Fermi made few personal remarks. But just after I passed my Ph.D. exam, he invited me to stay as a research associate and he did give me praise and encouragement.—J. O.*

Fermi did not have an exceptional memory and in fact claimed to have a very poor one. He created for himself an "artificial memory," an encyclopedic collection of notes, summaries, calculations, numerical data, etc., classified according to a decimal system invented by him. This "memory" is conserved (approximately 20 boxes at the University of Chicago).

Fermi displayed hardly any of the behavior patterns that one (rightly or wrongly) often attributes to Italians: loud speech; vivacious gestures; gregariousness; fondness for wine, food, and song; concern for well-tailored appearance; assertion of authority ("you don't know whom you are speaking to," etc.). He possessed, however, one Italian quality, one that many American intellectuals lack: a total absence of psychological complexes (prewar Italy was the country with the smallest number of psychoanalysts per capita).

Fermi was perfectly well integrated to American life, preferring to be "Enrico" rather than "Egregío professor" or "Herr Geheimrat." He participated in the students' social life, going to their modest parties and inviting them to his home for square dancing (the girls were provided by his wife, Laura). Although he never lost his Italian accent (e.g., he would always say "veertual"), his English was excellent: he delighted himself in using vernacular expressions and typically Anglo-Saxon constructions, such as "is it—is it not?"

Friendly with everybody, always helpful, Fermi seemed to eschew close personal relations. In our opinion, he felt that these would interfere with his quest for efficiency. His ability to provide order-of-magnitude estimates on the spot was phenomenal, and he would sometimes exercise it under surprising circumstances. An authenticated anecdote can illustrate this: William Zachariasen, a distinguished crystallographer and close colleague, was in the hospital recovering from a heart attack. Fermi decided to pay him a visit there. Zachariasen complained bitterly that he was given too little to eat, only 1500 calories worth. Fermi asked him, "Willy, you are a great reader of detective stories, are you not? Zachariasen replied, "Yes. I am." Fermi then asked Willy, "How long does it take a corpse to cool?" to which Zachariasen replied, "four to five hours." After some thought about heat losses, Fermi concluded, "Then you cannot possibly survive on 1500 calories."

Fermi did not lack a sense of humor, even at his own expense. At the yearly Christmas parties, the physics students would compete with the faculty in various tests (always loaded in favor of the students!) and put on theatrical sketches. In some of these an electronic computer able to provide instantly order-of-magnitude estimates, aptly named the ENRIAC, was displayed. This computer consisted of a large box, complete with blinking lights, and contained a junior faculty member who could imitate Fermi's voice and accent. *(One of Telegdi's talents was remarkably good imitations. He even imitated Jay Orear in the presentation of a paper at an American Physical Society meeting.—J. O.)* The ENRIAC was asked: "Yesterday a corpse was found inside the cyclotron tank. What should we do?" To which the computer, in Fermi's voice replied, "An average adult weighs 60 kilos, 40 percent of which are water. The pumping speed of our cyclotron is so-and-so many liters per hour. The corpse is well desiccated by now and there is hence no point in opening the tank. . . ." Fermi shared in the general laughter.

Fermi's sense of humor and gift for irony are illustrated by the following two anecdotes: (1) Once somebody presented a talk about the H (read eta) Theorem: his argument seemed little convincing to Fermi. So he asked, "Are you talking about a theorem or simply about an H? (2) One of the best pastimes in Los Alamos was fishing. Emilio Segré enjoyed it and tried to convince Fermi to come along with him. Fermi did not seem to show any interest in doing so. Segré then tried to convince him of the intellectual merits of fishing: "You see, Enrico, it's not so simple. The fish are not stupid, they know how to hide. One has to learn their tricks." Fermi replied, "I see, matching wits!"

Another example of Fermi's humor is told by T. D. Lee. At some point, Fermi decided to teach his private seminar group theory. He took out his index cards on that subject and started to discuss, first Abelian groups, then Burnside's theorem, then the center of the group, and only much later he got to the concept of group itself. Some of the students were a bit confused by this unorthodox approach. The master said, "Group theory is merely a compilation of definitions." Therefore he simply followed the *index* at the end of Weyles's book.

Fermi looked at his surroundings mostly with a physicist's eyes. Once, answering a question of Bill Libby's (in the institute seminar) about mixing in the ocean, he derived instantly an equation describing that phenomenon. There was only one parameter in it, the wavelength of surface waves. For this, Fermi promptly inserted a numerical value of 200 meters. Somebody in the audience asked: "Enrico, is it not rather 600 meters?" to which Fermi replied, "Maybe so. But it was certainly 200 meters when I last crossed the Atlantic." During a trip to Brazil, one of the things that impressed him most was that there the moon increases on the opposite side from what it does here.

One of Fermi's greatest assets was his wife, Laura. She was a beautiful person in every sense of the word, of considerable intellect and great charm. During the frequent parties in their home, she managed to make the younger crowd, especially the Europeans overawed by the presence of the Master, feel perfectly relaxed. No picture of Fermi's would be complete without her: Behind every great man there is a great woman. (See Figures 21 and 32 for views of Laura.)

5. Acknowledgments

The author would like to thank those associates of Fermi who kindly answered his request for personal recollections (Drs. Chew, Garwin, Goldberger, Haber-Schaim, Lee, Nagle, Orear, Schluter, Steinberger, Wolfenstein, and Yang). May the author be forgiven for not incorporating all of the material he received and for plagiarizing some of it verbatim.

Chapter 21
Fermi at Columbia, Los Alamos, and Chicago

Harold Agnew

Former Director of Los Alamos National Laboratory

I find it interesting when people talk about past history, and I've become more and more suspicious about historians and people who write about history after the fact. Fortunately Laura Fermi did write her book, Emilio wrote his book, and there are still a few of us around who actually experienced working with Fermi.

I had completed a degree in chemistry at the University of Denver. I don't think I'd heard of a neutron. I certainly hadn't heard of Enrico Fermi when I was asked to come to Chicago. I came to Chicago in January of '42 and was assigned to work with Enrico Fermi and Herb Anderson, both who are my heroes in this whole business; I was very fortunate to be with those two individuals. I wasn't there very long when I was told that I was to go up to Columbia, that there was an experiment going on there that Fermi had been conducting for quite a while, and he wanted to finish it up. We were really in a hurry, so we went around the clock doing experiments there with Indium foils, and you heard about Enrico in his lab coat, running back and forth to measure these foils, which were irradiated in a large pile that he had there. So we did that, and indeed, we did have to run. I was intrigued by the fact that all the counters—they were little lead cylinders which had Geiger counters in them—all had names from Winnie-the-Pooh. There was Pooh and Piglet, and the Heffalump—you were told which one you were going to run to, and there was the name, right on the side of it. The experiments weren't very encouraging, and Enrico put a tin can (I'll call it a tin can, it was actually an 8-foot cube, it was a big thing) encapsulated in galvanized iron, had vacuum pumps, and was evacuating. He thought perhaps it wasn't working so well because it captured nitrogen, or something like that, but the real problem was that the graphite was impure and the uranium oxide he had wasn't very good, either. Nevertheless, we had put in vacuum pumps and were evacuating it, but that didn't work. Then we decided that we were going to fill the thing with hydrogen, some hydrogenous material, to slow the neutrons down. Now this, remember, you heard it, it was in 1942, up on the I don't know what floor. So here we were, and we had propane tanks all hooked up, and we were going to fill the thing with propane, and Elizabeth Graves, who had come with us from Los Alamos, said, "Enrico, we can fill it, but how are you going to get it out?" And all of a sudden, the prospect of having an explosion in the middle of the Columbia campus in the Physics building was, in no small part, why we decided to cut that experiment and move to Chicago. *(laughter)* Clearly, safety was an issue in nuclear power from the very beginning, and some of us still think that there are some things that we can do to improve this.

We went back to Chicago, and I first was assigned the same as Al was, Al [Wattenberg] was sort of a straw boss under Zinn. There were two teams that worked in the building on the pile; there was the daytime crew and this was Zinn, and Al Wattenberg, and the night shift was Herb Anderson and his guys. Graphite was an awful material; it's heavy, and dense, and very slippery. Those things are heavy, and you could really get your fingers pinched and also hurt your knees because you had to crawl on this pile of graphite. Fortunately, after a while I was assigned to do instrumentation and to work on the vacuum system. You may not have noticed, but the whole thing was being built inside of a cubicle rubber balloon that Herb Anderson had ordered; he convinced the Goodyear people that was the wave of balloons of the future. *(laughter)* We had this big balloon and we were filling it up. All during this time, it was very precise, we always stopped for lunch, and we had a sort of a team of us who always went to lunch together at the Commons, and we talked about things. Not about work things, but I remember one thing that really impressed me was our fear of where the Germans were. This was a real thing that maybe every third lunch would come up. Where do we think we are, where do we think they are; it was a concern during those days.

Fermi liked to compete, and he was excellent in everything. I had lettered in swimming, so I thought that I was a hotshot swimmer. He used to go to Lake Michigan, off the 55th Street Promontory, and he liked to swim across to the 44th Street whatever it was called. I went with him once. I dove in, and after a little while I realized that I was having trouble. Fermi had this very interesting sort of a dog paddle–type of stroke, and he would just come back, look at me, swim around me, "Are you all right? Are you all right?" and then he'd go off. *(laughter)* Herb Anderson was a very good swimmer. Until his death he swam every day. Enrico and I finally reached where we'd been heading for, and I just barely had the strength to pull myself out of the water—he turned around, dove back in, and swam all the way back. *(laughter)* Most amazing. He'd never ice-skated, and we went out behind the North stands—they had ice-skating in the winter, and in two evenings, Fermi could ice-skate. He was very good in tennis. One thing he could not do—he was a complete failure at trout fishing. Absolutely a failure. But Segré, his great buddy, was a great trout fisherman, and Segré always ribbed Enrico about his inability to catch a poor fish. *(laughter)*

After Chicago, some of us were asked to go to Los Alamos. I went to Los Alamos, and it was there that, I think, Enrico and his perception really made a difference in our weapons program. You remember the first plutonium was done at Berkeley, in the cyclotron, but then the plutonium that we were planning to use, and we were planning to use it in a gun assembly—all the effort originally at Los Alamos was for gun assembly—essentially take two pieces of plutonium or U-235, put them into a gun barrel, shove one or both of them together and, clearly, if you have more than critical mass, you'll get an explosion. But Enrico at a meeting said that the plutonium that we'd been working with, and it was in microgram quantities, had come from an accelerator. The plutonium that we were going to get was to come from a reactor, and it was going to be exposed for a long time to neutrons, and it may absorb a neutron, and it may be that neutron, in such a neutron-rich nucleus, that might be coming out, that we might have. . .well, I don't know if we'd called it "spontaneous fission" then, or not. But it was that phenomena that he worried about. To show that I really think that he came up with this and thought about this was the fact that the person who was assigned to find out if this were true or not was Emilio Segré. Emilio had a group: Owen Chamberlain, Clyde Wygand, George Farwell. And they isolated themselves way out on one of the mesas, because they only had a few micrograms of material to work with—to attempt to measure the spontaneous fission rate of reactor plutonium. This experiment showed that there was, indeed, a problem with pre-initiation in a gun assembly, and you couldn't do that. This changed the whole direction of the project and really accelerated the work which Nedermeyer had tried to institute toward using the implosion technology. *(In his talk at the 2001 Rome Congress Agnew said, "he [Fermi]* saved *our nuclear weapons program when he came up with the idea that plutonium from Hanford would be different than that produced in a cyclotron and had Segré confirm his worry." In addition, it appears that Fermi was the only one who could have proven by that time that a critical reaction was possible. No wonder he is thought of as the father of the atomic bomb.—J. O.)*

Now after the war, I was very fortunate. Enrico said that I could come back to Chicago, and he got me a National Science Foundation fellowship to complete my studies. We couldn't find a house when we came back after the war, and it so happened that Laura had not seen her sister since they left Italy—so there was Giulio and Nella, who was about 13, maybe 14—I thought she was 12, but that's okay—and Enrico. (Laura had left for Italy.) So we moved in, and we had a two-year-old daughter. So the Fermi family took us in. Laura showed Beverly what Enrico liked to eat and how he did things around the house; I was sort of the handyman—mowed the lawn, washed the windows, and helped when something broke. In Chicago, if it rained very hard, you had to run down to the basement and screw a plug in the floor, otherwise the basement would flood. Nella did the cooking and the housework. Enrico liked plain foods; he drank some wine but usually diluted it with water. We learned lots of good foods—quick saffron and rice, all sorts of good things. I tell a story about Nella: the kitchen—it was a very nice kitchen—had a pantry. Nella always wanted to help. Enrico, in addition to pasta, liked mashed potatoes. We would make mashed potatoes: cook potatoes, add milk and butter, and then you whip them with a beater. One day, Nella was going to help and was going to whip them, and when she was finished one day, instead of turning the beater off, she took it out, and it made an awful mess. I was sort of angry, because I was sort of the "cleaner-upper" in this arrangement. *(laughter)*

[from the audience] **Nella Fermi**: Sorry about that!

Agnew: That's okay. *(laughter)*

It was raised earlier about Enrico getting involved in politics. He got involved, I would say, in a sense, in the system—not in the actual "who's good, who's bad," and so on. I remember one suggestion he made, which I certainly think is true today: He said, "You know, I've been looking at the American political system, the voting and the candidates, and there's one thing wrong. There should always be a third place: None of the above. *(laughter)* If an incumbent happens to be running, he's out! And the place is just vacant until a new election comes along and you vote somebody else in." He said, "I have the feeling you're really voting, most of the time, for the lesser of two evils." But he liked this idea that we should change the system to include "None of the above." So he was into politics.

Another interesting thing after the war, just to show you what a straight shooter and a modest individual he was: when Laura came back from Italy, she said she'd really like to have a dishwasher and a washing machine. Now, she had a Bendix washing machine, screwed to the floor, and it rotated parallel to the floor, and when it ran—there was no automatic balancing—the whole house sort of shook. It was quite a thing. But she wanted a new one. We were at dinner, and Enrico had just come back from Hanford, I guess. I asked him, wasn't he working for General Electric? Didn't he know the boss? And Laura said before that that she had gone down to the local hardware store and put her name on a list to get a washing machine and a dishwasher, which was what you did after the war—it wasn't like today, you had to get on a list and wait. And I said, "Enrico, gosh, you could call your friend, the president of General Electric, and they'd bring it by helicopter, and you'd get it for free, I bet!" *(laughter)* Laura was intrigued with this idea. *(loud laughter)* Enrico would have no part of it. No way. He would not use his influence, or whatever you want to call it, to get ahead in line.

Now, several people have talked about how quick Enrico was. Maybe it was because I was so slow that he didn't go speeding ahead, or maybe some of the people who said how quick things came out—they were that way, and maybe Fermi, in his competitive way, had to keep ahead of them. With me, he was always very methodical, very sensible, very cautious. I should say that, at Chicago, at that time—and everybody came back—talk about a magnet school. In our group alone, there've been four Nobel Prizes to date: Lee, Yang, Chamberlain, Steinberger. And there are some that I think are even smarter than those guys; you know, we had Goldberger, Rosenbluth, Reitz, Chew, a whole gaggle. Here I was with a poor degree in chemistry, coming back after a three-year hiatus. So I was pretty frightened of taking that qualifying exam. It was Laura who convinced me to take the exam. "Take the exam, don't worry about it, take the exam!" So I went in to see Enrico and I said, "What should I read?" And he said he really didn't know, he really never read. *(laughter)* I asked, "How do you know what you know?" He said, "Well, people come and talk to me, and that's how I learn." *(laughter)* At that time, we were taking a course from Zeyner, and I said, "For instance, we're doing Brillion zones." He said, "Well, look, there are really only about 10 things that you really have to understand. If you understand these 10 things. . .*(tape changeover)*. . .In fact, what startled me was, he said one of the fastest people to grasp things was Oppie. But he said, Oppie really didn't understand many things. *(laughter)* So, Enrico's meaning of "understanding," I think, was really quite different than that of most of us. We pretend we understand. But he really understood, and because of that, he could take these ten things and do such wonderful things with them. Now, someone mentioned his notebooks; what amazed me about his notebooks was not that he kept his notes in there, but they were indexed. The back of every notebook was indexed, so he could find whatever he wanted in the notebooks. I never quite understood that; I thought that was amazing.

We used to have parties at the Fermi house, and he and Laura liked young people. No question, they preferred to have young people around, rather than his peers or older people. But whenever he had to have some of the older people, he would say, "Well, they have to be diluted." He used this phrase; he "diluted" these older people with young people. He preferred young people. After the War, after Los Alamos, I had an opportunity to stay with Enrico for a while at Chicago. I'm one of the few people who decided that I didn't like Chicago; I wanted to get back to the West, having come from Colorado and New Mexico.

But we did go back, and Enrico would come in the summertime, and he brought Dick Garwin. *(They shared the same office.—J. O.)* Dick had been with us in graduate school, and between the two of them, they made *tremendous contributions* toward Los Alamos in those days. *(And it was those days when the H-bomb was invented and tested. Certainly when Fermi and Garwin are in the same office, sparks fly, and new inventions occur—that otherwise would not have occurred. Agnew seems to be hinting that both Garwin and Fermi made* tremendous contributions *to the H-bomb. Even Edward Teller in a* New York Times *interview gives most of the H-bomb credit to Garwin. In the April 24, 2001,* New York Times, *William J. Broad states in an interview that Edward Teller said: "So that first design was made by Dick Garwin." Broad continues: "And then Teller repeated this credit, ensuring there would be no misunderstanding." It is well known that Teller was the first strong advocate of a thermonuclear bomb, but he seems to be telling the* New York Times *that he was never able to produce a workable design by himself. Fermi, in his October 4, 1954, press conference stressed that it was the product of many at Los Alamos. I suspect that neither Garwin nor Fermi would like to be known as a father of the H-bomb. In conclusion, I do not know who should receive the most credit, but it appears that Fermi did make some contributions.—J. O.)*

Let me mention one other thing: we went to Enrico's house in January of 1942; he'd moved to Leonia, New Jersey, they had a house there. John Mann and I went there for dinner. I don't know whether I had noticed it before, but Enrico had a sort of gray tweed suit, and the thing that amazed me about that suit was that all the pockets had zippers. *(laughter)* The side pockets and the back pockets. I don't know whether Nella remembers that or not, but when he came to the United States, he had zippers put on his pockets, and he said he kept his money in the backyard. He said he had his money hidden out in the backyard. *(laughter)* I thought, my goodness, what an individual this is. *(laughter)*

(At the end of his talk Harold showed a home movie he had taken of Fermi mowing the Agnew lawn with a motorized lawn mower. Also he showed the photo of Fermi presented in Figure 27, which he considers the best photo ever taken of Fermi.—J. O.)

Fig. 27. Personal photo of Fermi taken by Harold Agnew; one of the many exhibits in the Fermi Museum that accompanied the Rome Fermi Congress. Harold regards this as the best photo he has ever seen of Fermi. This splendid museum was to be disassembled later in October 2001. The young lady is one of the museum visitors.

Chapter 22
Two Papers about Fermi

Robert R. Wilson
Cornell University and Fermilab

Figure 28.
Fermi and Robert Wilson together in a Los Alamos group picture. As pointed out in Chapter 22, they spent every Friday afternoon together. Photo courtesy LNS archive.

(Bob submitted two papers for the report to this conference. This first contribution to my knowledge has never been presented. The second contribution is the presentation he gave to the Cornell conference.—J. O.)

I. Working with Fermi

A Reluctant Division Leader

"No! No! No! I won't do it!" I shouted at Oppenheimer, who had just offered me the job of heading the new Research Division (R-Division) at Los Alamos.

The year was 1944, and the laboratory was being reorganized because of the Fermi discovery of the high rate of spontaneous fission in plutonium. Bob Bacher headed the old Experimental Physics Division, which had been split into two new divisions in August of that year. One part became Gadget Division (G-Division), which was to develop a plutonium bomb based on Seth Neddermyer's implosion ideas. The other part became R-Division, which would consist of the remaining four groups from the Experimental Physics Division: the Cyclotron Group (R-1), headed by me; the Electrostatic Group (R-2), headed by John Williams; the D-D (Deuterium-Deuterium) Group (R-3), headed by John Manley; and the Radioactivity Group (R-4), headed by Emilio Segré.

Bob: "Look, Oppie. Just pick one of the other three group leaders. They are all much more senior than I am, and each would hate working for a young fella like me."

Oppie: "Not as easy as you think. I have already tried to pick, in turn, each one of them, but in each case, the other two threatened to quit. So you, Bob, are elected, faute de mieux."

Bob: "No, not me! I did not come here to be an administrator. Why don't you just bite the bullet, choose one, and let the chips fall where they may?"

Oppie (weakly): "I thought I had done just that in selecting you."

Bob (looking him straight in the eye at the implied criticism of him): "Well, bite a different bullet then, because I came here to do physics and not to become an administrator."

Oppie: "Maybe we ought to think about it."

The next day, Enrico Fermi asked me to accompany him on a walk. He had been sent by Oppie to talk me into the R-Division job.

Bob: "You're a fine friend, for I have been following your example in turning it down. You would never do that sort of thing."

Fermi: "It's something you have to earn, and you're not Fermi yet!"

He then went on to instruct me on how to avoid administrative duties. Essentially, it came down to just saying no.

Bob: "Yeah, but how about all the technical work of the other groups? Wouldn't I need to know about it in detail?"

I was, up to this point, doing a pretty good job of saying no to Fermi when suddenly he volunteered to help me with the technical work. I was astounded! I could hardly believe my ears! The idea of working with Fermi made it a whole new ball game. I had worked with him on the reactor project at Columbia University, so I knew what a valuable experience working and learning from Fermi could be—never mind all the delightful fun of just being with him.

Fermi promised that he would be available whenever a problem came up. To clinch our bargain, we agreed to meet together <u>every</u> Friday after lunch to discuss the physics being done in the division and also the physics that should be done. I was ready at that point to sell my soul for this chance, but I still had a few conditions for Oppenheimer. One was that I could continue as group leader of the Cyclotron Group; another was that I not have a special office with a secretary. Finally, I insisted that each of the other group leaders ask me personally to take the job. Sure, I sold out—but then everyone has his price, and mine was a few moments each week with Fermi.

In any event, my life was little changed except for the delightful weekly meetings with Fermi. Usually in our discussions, a student-teacher relationship prevailed in which Fermi clarified the physics by simplifying it to a level I could understand—he was a master at that. Nor was it that I was completely unintelligent, for perhaps I knew more about accelerators and particle detectors than he did. We made a pretty good pair. As division head I gave the group leaders essentially free reign. Happily, I had never heard about people staying in channels because Oppie would usually go directly to the person concerned. On the other hand, I would get several calls per week from him about the practicality of experiments being considered for our division as well as an ordering of the priorities for the whole project. I always had the feeling of knowing too much rather than too little about what was going on at Los Alamos. One of my duties as a division head was to attend the weekly meetings of the administrative board. We usually considered serious matters involving the project. But on the light side, I recall that Joe Kennedy and I had dedicated ourselves to making the life of the G-2 army security officer miserable. We would hit him both coming and going. His security measures either grossly interfered with the work of the project, or they seemed to us to be totally inadequate.

Once I remember Kennedy giving this particular officer a hard time about not providing enough surveillance. The officer remarked, "Joe, how do you know that the little kid who followed you over here was not one of my agents?" *(Wilson looked young for his age.—J. O.)* Kennedy looked at him coldly for a few moments and responded, "Yeah, if he's your agent, he's your best agent." Actually, the meetings were exciting for we were kept abreast of all sorts of important information about the project, such as when and how much U-235 and plutonium would be made available to us.

Sometime in March 1945, the nature of the R-Division changed dramatically. We were given, in addition to what we were then doing, the responsibility for measuring the nuclear phenomena resulting from the test explosion of the first atomic bomb. This test was to be made in the Jornado del Muerto desert near Soccoro, New Mexico. Philip Moon of the British Mission had already done some preliminary design and construction. But time was running short and not much was getting done, so Oppie asked us to reconsider the whole problem about what experiments should be conducted for the Trinity Test. We pitched in with gusto to do what could be done in the three or four months remaining before the expected time of the test shot.

Fermi was particularly interested in this phase of the project. He and I used our regular discussions as one way of satisfying his interest. Of course he had many other channels open to him, and I am sure he used them, too. As I recall, the members of our division decided who would do what, not by general meetings but by meetings between me and the individual group leaders. My procedure was simply to inform them of what had to be done and to ask them what they wanted to do. After they had discussions within their groups, they came back to me with a list of who would do what. I suppose there was a bit of pushing and pulling, but somehow we easily came up with plans that covered all the measurements that needed to be done, and then we made the equipment and installed it in the desert. Writing this now, it sounds authoritarian, and perhaps it was. But I think not, for we were such a small division (perhaps about 40 physicists) that we all interacted frequently enough so that no formality was necessary—or so I thought.

My continued meetings with Fermi were pure pleasure—well, with one exception. My usual function seemed to be to bring up problems that, to my great satisfaction and admiration, Fermi elegantly solved without much participation on my part. Only occasionally would I argue with Fermi's physics, and then with great trepidation—he was just terribly good. I did learn a lot because he worked out what he was doing in a very clear manner that I could easily follow. Yet being human, I wanted to participate more in the physics process. I do remember once, though, when, to my great satisfaction, I caught him in an egregious error. Then without remorse, I made him suffer for being right so much of the time. This joyous occasion occurred when I had invented a device for measuring the rate of increase of neutrons (the e-folding time) during the explosion of the bomb. An electron-multiplier tube was to be used to measure the radiation as it emerged from the detonation of the bomb. Fermi thought about this for a few seconds, went through his calculations, and then informed me that it would not work. "Too slow," he said with his usual confidence, "by a factor of hundreds compared with the 10^{-8} second resolution you expect." I informed him that that must be wrong. Again Fermi went through his calculations, this time out loud and slowly for my benefit. "My dear Enrico, you are losing your grip. Perhaps it's too elementary," I said with an assurance that worried Fermi slightly. He made more calculations, this time on a piece of paper, again with the wrong result. He had made an error that I knew he was not likely to find. That put me for once in the "catbird seat."

Fermi's error was due to our custom at Los Alamos of finding a particle's speed at some energy by simply scaling up that of a thermal neutron. Fermi had been doing this automatically over the past years, and he was not likely to break out of this ingrained habit. I let him wallow in his misconception while I privately delighted at his discomfort. Eventually, I asked him to his embarrassment, if he had ever heard that electrons were 1,800 times less massive than neutrons.

We tended at first to be somewhat casual about the Trinity Test. One day John Dewire and I were discussing possible electrical pickup signals in the various detectors being built. We knew that the next day there would be a test explosion of 100 tons of TNT at the site of the future test. We asked ourselves whether or not we could find out anything from the explosion.

Well, no, we decided. But just seeing it might be a valuable experience for us—or at least some fun. So on a whim, we called Oppie's office to tell the guards at Trinity Site that we were on our way. Then we put a portable electrical generator, a long coil of electric cable, and an oscilloscope into a pickup truck, stopped to tell our wives (we did not have telephones in our private homes), and headed for Trinity Site some 200 miles to the south. It was dark when we got there, and we had to talk our way into the site past the guards.

We were able to spend the night in the crude barracks at the base camp. The next morning, we drove over to where about a dozen people were stacking a huge pile of boxes of TNT. We joined in and helped stack boxes for awhile—strangely, no one else seemed worried about dropping a box because, I gathered, a detonator was required to start an explosion, but I was worried!

Soon, I had an idea for our experiment—simply to put the shorted end of our cable deep into the pile and then run the cable several hundred feet away from the pile to our oscilloscope and gasoline-powered generator. Not much of an experiment, I must say, but it was better than stacking boxes of TNT! Of course we expected no signal. That night, we found the explosion impressive. It even had a quality of beauty. The next morning we developed the photograph that had automatically been made of the scope trace. To our surprise, there were huge signals. We had to understand the source of those signals, how much worse they would be in the ambiance of an exploding atomic bomb a hundred times more powerful, and how we should shield against that. This unexpected finding was a good example of the value of laziness and fear.

Back at Los Alamos, significantly large amounts of separated ^{235}U began to arrive from Tennessee. One experiment that I can recall was to measure the multiplication of neutrons in a sphere of this material about 1 inch in diameter. Oppie insisted that the material be guarded all the time. For some reason, Fermi's personal guard, John Baudino, was assigned to us. In fact, there were two identical spheres, one of ^{235}U and the other of normal uranium. We were to make a comparison of the two. I liked to amuse myself by switching the spheres around rapidly and then asking Baudino which sphere was the one he was guarding. He would confess that he did not know and would ask which one should he be guarding. I could tell because the ^{235}U was warmer because of its greater radioactivity.

We wanted the measurements to go on all night, but we had to stop so that Baudino could sleep. I had the idea that were I to be issued a pistol, then I could do all the guarding myself—after all, I came from Wyoming where every red-blooded boy learned to shoot before he could walk. *(Figure 25 suggests that he also could ride a horse before he could walk.—J. O.)* Oppie agreed and asked security to issue a pistol to me. My friend Pier de Silva agreed to do so, but he reasonably insisted that I be checked out first on whether in fact I could safely use a pistol. This he did by taking me to the firing range, pulling out a 38 Colt police revolver, and giving me a lecture on its use.

"This little lever is the trigger. These little gadgets are cartridges and should be put in these holes that spin around here. You line up the front of the gun with this v-shaped hickey in back and with what you are shooting at. Here, I'll show you," de Silva said. With that, he carefully fired six shots at a target.

"Now you do it," he said, loading the gun. I had learned in Wyoming to "roll" a pistol in order to get a lot of shots off accurately and rapidly. That's just what I did. Most of my shots were closer to the bullseye than were his. None of this fazed de Silva in the slightest. He repeated his earlier lecture in its entirety, together with his demonstration. He finally wrote out a beginner's certification and issued the revolver to me for the duration of the experiment. He had put on a terrific show; not once did he crack a smile!

I took full advantage of the pistol to impress my friends with what a macho type I was. I carried it, ostentatiously tucked into my belt, everywhere in the technical area and spent no little time at all explaining to the military police why I had the gun; eventually I had to show them de Silva's authorization. When the experiment was completed a week later, I was most reluctant to give it back. I am proud to this day that the uranium spheres had not been stolen on my watch!

I became involved in a dispute with G-Division that did not end well. As more and more ^{235}U and plutonium was delivered to us toward the end of 1944, measurements of assemblies close to criticality were started by the Critical Assemblies Group of G-Division. At first these measurements involved small cubes of uranium hydrides (such as UH_{10}), which were stacked up into larger cubes until criticality was approached. Later, less hydrogen was used, and the procedure became more serious—more dangerous. The Critical Assemblies Group decided not to have the elaborate safety devices that were used, for example with cyclotrons. Instead they decided to depend on their wits alone. These physicists were the best and the brightest of the project. So although I did not like their arguments I could see that there were good reasons for going ahead as they had decided. For instance, each assembly might be quite different. After expressing my views forcibly, I subsided. After all, they were not in my division, and indeed it was none of my business—well, in a fashion.

A few months later, I became more involved because they wanted to use the fast modulation of the cyclotron (neutron pulses of less than one-tenth of a microsecond), which was okay of course. I was the crew member whose turn it was to help the single physicist who showed up. His equipment consisted of a small wooden table, a single neutron counter, and boxes containing the small cubes of enriched uranium hydride. I was impressed by the simplicity of the equipment, as advertised, "So simple nothing could go wrong." Not quite. The physicist began stacking the uranium cubes as I stood next to him and watched with considerable interest. It was my first experience with a prompt neutron reactor approaching criticality, and I was thrilled in expectation.

After a while, as the stack got quite large, I asked why the neutron counter was not counting. I was assured that this was regular and that it would not start counting until we were closer to the critical point. Uncomfortably, I gave the neutron counter a hard going over and asked if the signal light on the high-voltage supply was operative or if it was burned out—as is often the case. The voltage was indeed turned off, so the neutron counter was not working. When the voltage was turned on, the counter to my horror started blazing away. A few more cubes and the stack would have exceeded criticality and could well have become lethal. I was outraged. This incident was my closest brush with death. The reason given was that a wooden table instead of a metal table was being used for the first time, so thermal neutrons were reducing the critical point. After chewing out the physicist for his carelessness, I went to his group leader. Not satisfied, I complained to the division leader. Still not satisfied, I flew into a fit of anger over the incident with Oppenheimer. At the time, we were all hysterically busy. I was due back at Trinity the next day. And I went there of course. I should have stayed at Los Alamos to pursue the incident further—for if I had, I might have saved the lives of two people. To this day, the incident is on my conscience. The Trinity test was soon upon us. R Division had occupied the North Bunker at 10,000 yards from the bomb locally and had acquitted themselves well, not that any credit was due to me, but I still take great pride in them—however, Trinity is a separate story.

Once we had seen the explosion in all its grandeur and implied horror, we didn't need any of our measurements to know that it was a success—they would have been more meaningful had it failed. I exulted with my colleagues in the gratification we felt in a job that had taken five long years of dedicated hard work. It was an epiphany for all of us. For what had been theoretical before had now become all too real—but in a different way for each one of us. For me, the project was over. I could hardly wait for John Manley to take over the division and to reorganize it into the Physics Division that now bears little resemblance to the tiny group we were then.

II. Fermi and Politics

One is inclined to speak about Fermi as the quintessential scientist, dedicated and engrossed in his subject, eschewing the humanities, not much music, less poetry, antithetical to politics, and, perhaps, relaxing only in sports and jokes. There is, perhaps, some truth to that characterization, but I want to relate one story, at least, of one important occasion when this characterization of Fermi did not apply at all. Let me start my story the day after Trinity. We had done our job, and now the questions had become, "What had we done, and what did it mean?" Most of us stopped the physics that we were doing and began to think hard about that meaning. Three weeks later, the bomb was used at Hiroshima, then we knew, existentially, I suppose, what we had done, and we knew that it should not happen again. We knew that we, also, had not done our job, as perhaps we had thought before. We knew that we, not the army, not the government, should do our best to bring about a general understanding of the mysteries and implications of nuclear energy. We began thinking anew, as social beings and as citizens. We had many arguments. The arguments became furious at times on the hill. Some were agonizing, some were furious, and the wives joined in, all the people on the hill joined in. Five hundred people were involved, and in another three weeks, we had organized the Association of Los Alamos Scientists to help us with what we had appointed ourselves to do: to tell other people about what we would do to have it not happen again. Vicky was one of the principal organizers, but I see organizers all around. Hans was helping write out documents for us, and all of the old-timers I see were involved. Any old-timer was involved, whatever his political notion happened to be. As a result of all of this discussion, we developed what, I suppose, was a litany of what we regarded as facts. You've all heard that many times, but we could see that there was no secret, and this seemed obvious to us, but we could show that this was so for atomic bombs. There would be no monopoly, we could see that other countries could do it, and could do it quite fast, on the order of five to 10 years. That there would be no defense, we asserted very strongly, and we had the arguments to back it up. We also felt strongly about international control as the only way that one could avoid an arms race, and one might be able to avoid a future war.

But that's not my story. My story is where was Fermi in all this. Well, since there were arguments going around, Fermi was always in the center of arguments, we were always looking to him for advice and guidance. He couldn't get out of the arguments, he was trapped as they went around. . .I mean physically, we would be arguing around and about him. He seemed to be somewhat aloof to our arguments, I must say, and withdrawn. I think he questioned many of the things that we said, and he questioned our qualifications to address such political and social problems. Nevertheless, I should say that there were two people at Los Alamos to whom I went for advice and wisdom. One, of course, was Oppenheimer, and this was because of his social compassion and his demonstrated wisdom in running the laboratory. We all admired him tremendously, and I was very much enthralled with him. The other person, of course, was Fermi, because of the intellectual power we've heard so much about today, and because of his dedication to truth. He was not a person who I ever heard come faintly far from the truth. Not at all.

Well, these two people to whom I turned could not have been more different. Oppenheimer, people and poetry oriented, and Fermi, delighted to delve deeply and thoroughly into physical mysteries—both artists, I would say, in their fashion, but each of a different métier: Oppenheimer, of course, the romanticist, and Fermi, the realist. But I loved them both. I loved them passionately, in a sense, in that they were both possible good friends because they were complementary. Now, it became that those were the people who I went to, to help with the problems that we young scientists at Los Alamos were having, as we didn't have much confidence in ourselves. It became hard to see Oppie, unfortunately, because he was in Washington. I have to explain about Oppie: about every five years, he would have a personality crisis. He would change his personality. I mean, when I knew him at Berkeley, he was the romantic, radical bohemian sort of person, a thorough scholar. Then at Los Alamos, he was the responsible, passionate person that we all knew so well there and who was so effective. Later on then, he had another metamorphosis, becoming the high-level statesman who could call Acheson by his first name (and such other high-level people), but as a result of that was able to put forward the international plan for controlling atomic energy through the United Nations that we had all agreed was the necessary ingredient for continued survival.

Fermi, on the other hand, I never knew him to change, from the time I, as a student, had seen him to the time at Columbia, when I was involved in a collaboration with him, to the time at Los Alamos, to the time after that. It always seemed that Fermi was Fermi! *(laughter)* He was always doing the same things and doing them very well. So, Fermi was the natural one to turn to, and I did turn to him, even though he was a little aloof and wouldn't hear us very well. I still would go after him like a gnat, I suppose. One thing about Fermi: if you were saying something that was nonsense and was wrong, he would straighten you out about that. Whether you were right or not was a different question. *(laughter)* He was polite about that.

About then, with all of this going on at Los Alamos, about the first of October, a few weeks after the organization of the Association of Los Alamos Scientists, the nefarious May-Johnson Bill came before us. When we looked at it, it seemed to not be a good bill. We could see many defects in it. I went to Oppenheimer to get his opinion of that. To my surprise, Oppenheimer backed the bill and said that this was not a good bill, but it was the best bill that we could expect. There were many bills in the wings, which were worse than that, by all means, and what we should do was back that bill, because what was needed was to change the control of atomic energy from the military (under whose control it was throughout the war and the Manhattan Project) over to the government. Just to run, and to continue to operate and to deal with this rather large industry, one needed such a bill as the May-Johnson Bill. Okay, Oppenheimer was a very persuasive person and, because of our trust in him, we accepted that, in spite of our better feelings, and with lots of arguments at our meetings. There would be 500 people at some of those weekly meetings, and we would debate that sort of thing. In spite of that, we all revered Oppenheimer so much that we decided that in spite of our sophistries, perhaps, we would back that bill.

Some time later, it came to pass that a diverse group of scientists were to meet in Washington, D.C., at which I was sort of an observer (I was too young to be a part of any group.) I think that this was in the middle of October now. Most people came from different places; it was an accident, more or less, that we happened to be in Washington. Oppie and I were there to testify at a hearing on science for the Kilgore Committee, which was considering the legislation for various kinds of national science foundations to be organized. Senator Fulbright was present, and Oppie made his usual very dramatic talk about the future of nuclear energy and the kind of education that might be appropriate for that. Senator Fulbright, as we've all come to know better for his fellowships and for his opposition to the Vietnam War, was there and began to ask questions about the May-Johnson Bill. Again, I was surprised that Oppie, who was less comfortable in defending that bill, when he had been so idealistically oriented previously. Still, to my surprise, he was backing that bill and giving advice to Senator Fulbright. It mystified me. At that time, he also read a letter that he had from Fermi, who wasn't in Washington, then.

Fermi, on the other hand, did criticize the bill and did not approve it. He criticized it because of the secrecy measures, which he thought were too much, and he also criticized it because he thought it was overly organized and that that would be something that would keep the young scientists, particularly, from making suggestions and making inventions, and might dampen their creative abilities. I think that was absolutely correct. Well, okay for that. After the lunch, five of us young physicists were so impressed by Fulbright that we went around to see him. There was Curtis, from Oakridge, and Borst, and Rabinovich, from Chicago. . .I've forgotten all of them. . .a small tier of half a dozen or so who went to see Fulbright and spent the whole afternoon with him. We found out that he was very astute in the way in which he was able to ask us questions about that which we had already made up our minds, and could advise him about. It was a nice afternoon. I got to know the other scientists from the other laboratories—the Radiation Laboratory in Boston as well as the people from Chicago.

That evening (and I'm finally coming to my story!) a dinner had been arranged by Watson Davis, who you are probably not familiar with, but he was the person in Science Service, and he played a very valuable role in science at that time, and he was very much of a liberal person, and he helped in organizing the young scientists tremendously. In any case, he had the idea of having a dinner, at which there were scientists sitting around a rather large table, and between each scientist there was to be a politician. So, a senator, a scientist, a senator, a scientist, and all the way around (all senators, I believe, except for Wallace (the vice president), who came, I believe with a person named Neumann, a mathematician from Yale, who was to become very important). The kind of people who were there. . .there was Oppenheimer, and Fermi was now in town, and Leo Szilard, Shapely. . .there were half a dozen young scallawags, of which I was one, and that made up the dinner. Well, it started off. . .of course the senators had heard about Oppenheimer, but not about Fermi. . .so they asked a few questions of Oppie, about various aspects of nuclear bombs. One of them asked the question of Fermi, then. These were all social questions, about what we should do about nuclear energy. Could it be kept secret? Could other nations do that? These were bright people asking these questions. I'm afraid that Oppie, because of having to back the May-Johnson Bill before Fulbright, was a little confused, and so he tended to be a little bit wooly in response, and he could be that without much trouble at all. *(laughter)* Fermi was about the clearest and could speak very simply and with great understanding, had been listening to us when we had been like gnats, arguing around him. . .my apprehensions had not been justified.

The senators, when they heard the first answer from Fermi, with his clarity, from then on directed all their questions to Fermi. There were a lot of questions, and each time they asked a question of Fermi, I had not much confidence, because I thought he'd say, "Well, I don't really know about that. . .what do we know about sociology, what do we know about politics?" I could just see him doing that. (*laughter*) *He was not like that at all*. He went right down our party line, without deviating in one way. Now, with Wallace present, with Fulbright present, with Toby, who was the senior Republican, there, I think it was one time that it made a big difference that somebody spoke out clearly and forcefully, and *that man was Enrico Fermi.*

Thank you.

Discussion:

Male voice from audience: Before you go away, Bob, perhaps you could remind us of the fate of the May-Johnson Bill.

Wilson: Well, I'm glad you asked that question, because that evening, I went over to the Statler Hotel with Fermi and Oppenheimer, where Oppie had his room, and we were joined by Lawrence and by Szilard and Ed Condon. Condon and Szilard had been very busy about the May-Johnson Bill. Szilard, it seemed to me, was capable of devising systems, but here he had gotten Ed Condon, and he had gotten this man Newman, who was a good friend of Wallace, and Wallace, to come together, and to develop another bill. The May-Johnson Bill, which was from the army, or really, from the War Department, had presidential backing, at that time. . .Truman's backing. So, at present, they had this new bill, which the older scientists had not heard about, and they came in, and they were giving them a chance to get off the bandwagon that they seemed to be on, which was the May-Johnson Bill, the official bill, because they said they could join with them. They (Szilard et al.) had a bill which, they explained, was going to become presidentially backed, and it was going to replace the May-Johnson Bill. That became the McMann Bill, which became the Atomic Energy Commission. So, it all ended well. I must say, at one point, Fermi said to Szilard, "Leo, sometimes you say things just to sort of stir the pot, and I'm not sure that I'm convinced by what you have said." It sounded absolutely incredible. I was sitting on the floor. . .I had no business being at such a high level conference. Anyway, at this question of his veracity, Szilard stood up at his full 5 foot 5 inches, *(laughter)* and Condon stood up behind him, and they marched out of the room. All I can say for myself is that I got up and marched out behind them. *(I am also puzzled by what Fermi said. Was it a joke? They should have asked Fermi for clarification.—J. O.)*

Carl Sagan:

Thank you, Bob. The next presentation is from another witness at Los Alamos, and her topic is the Fermi family.

Chapter 23
Laura Fermi and Family
Jane Wilson

Figure 29. Jane Wilson, Bob Wilson, and I. Rabi. Courtesy Cornell Laboratory of Nuclear Studies.

In the early May of 1943, just a bare month after I found myself in an army camp in northern New Mexico, I was thrilled to be entertaining, what I wrote in my diary as "a rather celebrated crowd." Fermi, the greatest physicist in the world, according to my husband, *(laughter)* Edward Teller, and Vicky Weisskopf. An Italian, a Hungarian, and an Austrian. I noted that Fermi was a pleasant character, who didn't seem conscious of being a great man, that he just sat on the floor, like a regular human being, and that he seemed to have a sense of humor. The conversation, after a bit of talk about where the Balkans began. . .after Hungary? after Austria? It came down to the fact that Vicky and Fermi were comparing their childhood visions of Emperor Franz-Joseph. For Vicky, he had been Christ, and for Fermi, he had been the devil, incarnate. For me, and I was 26, I was so thrilled to be listening to this, and I was very much aware that it was ironic that a group of people from diverse lands would be in this isolated, secret, strange mountain camp, discussing history paths, when actually, almost certainly, they were making future history. I was not the only one bemused by our situation. One lady called all the scientist foreigners together, for what she called a "peace conference." I was not there, but I heard about it on another occasion, when I had a dinner party with Fermi, an Italian, Rotblatt, a Pole, and Luis Alvarez, who was not a Spaniard. *(laughter)* The hostess sat in the "peace conference"—since I wasn't there I can't tell you really what went on—well, yes I can. *(laughter)* It was not peaceful. There was no agreement among the foreigners about what the peace should be like, Segré stopped talking to Joe Rotblatt, possibly for all time, as a result of the "peace conference." *(laughter)* The Italians were particularly heated, and they disagreed very much, and finally, absolutely enraged, Noah Rossi turned to Enrico and said, "How come you think you know more than all the other Italians?!" Enrico said, "Because I'm the only Italian who ever won the Nobel Prize." *(laughter)*

I taught English in the high school, and, possibly because I was going to have a future pupil (in just a month or two, as a matter of fact) from the Fermi family, once when I was dancing with Enrico—well, he was interested in everything—he was curious about the abilities of the kids in the school, what I was teaching, and so forth—at any rate, I had given him a very unflattering picture of the abilities of the students I had, and he said to me, "Oh well, Jane, I never liked to write compositions, either." I said, "Well, now, if I gave you 'Little Red Riding Hood' and you read it, you would be able to tell me, wouldn't you, what you read?" And he said, "Well, of course! I'm a pretty smart fellow, didn't you know?" *(laughter)* Laura and the children arrived in August of 1944, and they went to live in a very ordinary, three-bedroom, green, jerry-built, barracks-like apartment. I have a feeling, although it isn't in my diary, that they'd been offered a place in Bathtub Row and turned it down (those were the old houses of the ranch school). If so, Laura may have regretted her egalitarianism because the apartments had many drawbacks (and almost everybody here seems to have done so), you'll remember that twice she mentions one of the huge drawbacks—the acoustics. Below her, she had Rudolph and Gennia Peierls. They were very good friends, but Mrs. Peierls had a very loud and piercing voice, and her voice came wafting up through the floorboards—her laughter, too. Actually, the first time I met Laura, she was with Gennia Peierls, they were good friends, and I noted in my diary that every time Gennia spoke (and Gennia was a person of many words) Laura visibly shuddered. *(laughter)*

At this first meeting, I was somewhat surprised, having known Enrico off and on for quite a while, to find, after this ebullient, extroverted man, this very serious, shy, reticent woman. I think Enrico prided himself on being matter-of-fact and pragmatic, and Laura was unabashedly idealistic. We became very good friends, and my husband and I, like so many other people have said in this room, were invited frequently to the Fermi's. These were very important occasions to me; I noted every one in my diary, and there were quite a few of them. When I read that diary, half a century later, I get a little irritated at myself, because, when much that was important was going on, I seem to write a lot about where I went for dinner, what we ate for dinner, and other trivia of this sort. But then I started thinking about it, and decided that we were young people, and just beginning to have a sense of ourselves, and where we belonged in the world. Being invited around, and feeling ourselves a part of a community, was extremely important to us, that we were not tired of ourselves and sick of asking what we are and what we ought to be (to paraphrase Matthew Arnold), but that we began to feel belonged to physics, and that we really knew who we were, and goodness, we were invited to people like the Fermi's!

We were all, Laura wrote, part of one family at Los Alamos. This esprit de corps was, I think, never quite captured again. Now, why would ladies who were almost 100 percent not physicists feel so much a part of this community? Don't ask me, 'cause I'm hard pressed for the answer. The fact is that we did, and probably my generation still does, somewhat. The social life at Los Alamos was extremely important; we were isolated from the world, and it was a morale builder. Unlike other physics parties, before or since, the conversation was heterosexual, because, fortunately, because of security, they were not allowed to discuss business. *(loud laughter)* It was a blessing. We also played The Game. The Game was a charade game of two sides, and a quotation would be given from one team to the other team, and it was a matter of who was quickest. The team that got the quotation in the fastest time was the winner. I want to tell you of one of the most memorable times this was played, which was at the house of the Peierls. Gennia had a Russian dinner: borscht, buckwheat, etc., as it is written in my diary. The guests were the McMillans, Niels Bohr, Johnny von Neumann, and myself. After Gennia announced The Game, Niels Bohr was presented with (considering he was a Dane) a quote from the Prince of Denmark: "Take arms against a sea of troubles." He had a terrible time of it. *(laughter)* He couldn't even do arms. He was so awful, I was sitting there going like this. *(gestures)* He was so awful that Enrico, who was on the other team and wanted him to fail, ran around the room, pointing an imaginary pistol in the air. Bohr didn't get it. He went home early. *(loud laughter)* The Prince of Quantum Mechanics was vanquished by The Game.

Well, it was the wives who made such a difference in the social life, and here is what Laura wrote in her book, about the preparation for a party in Chicago (the one, actually, after the successful critical mass): "I had cleaned house all morning, I had polished the silver, I had run the vacuum, dusted, and sighed." Note the sigh. She also spread sandwiches, made the punch, baked the cookies, and so forth. You have to remember that, four years before this party, in Italy, this lady had been blessed with two maids; she had been raised in a wealthy household where, she writes in her book, the maids did the housework, her mother picked out her clothes, and she didn't know anything about money. Even when she married Enrico, on a salary of $90 a month, she had a maid. Then here she was at Los Alamos, with amenities few and far between, which Chadwick had characterized as "pigging it" *(laughter)* and we had famous water shortages always, incredible dust, and limited supplies. As for maids, she was lucky to have an Indian maid a couple hours, several times a week. Even under those circumstances, she had one dinner party after another, with good food, and really, it was remarkable. I think she liked it. She writes like this, "we wanted to become genuine Americans." I don't think her American dream was the Mercedes in the garage or the mansion in the suburbs; it really was the Jeffersonian ideal. In her book she writes, "It is the inborn conviction that man is created equal." In a later page she writes, "An American has the spirit of independence, and a firm belief in human rights." I think she bought that far more than many native-born Americans.

In 1961, I met Laura in Rome, her birthplace, the eternal city, the place of monuments, art, and history. She said, "Jane, what do you like best about Rome?" I said, "The maid."

(laughter) "Come on, Jane," she said. "What in this beautiful city do you like most?" I said, "I really do like the maid." She threw up her hands in despair.

She took her duties as an American very seriously. After the war, in Chicago, she was an early advocate of clean air, at a time when most of us were either completely not concerned or, more likely, felt kind of fatalistic about it, as though there was nothing we could do about it—fighting City Hall and big business. But because of public pressure of that sort, of course, things did get considerably better. Another one of her causes was not so successful: an attempt to do something about gun control. At one of these meetings, when she had spoken, a little gentleman with a peaked hat from the National Rifle Association in the back of the room stood up and said, "Mrs. Fermi, it is Mrs. Fermi, isn't it? You don't want me to have a gun? Didn't your husband make the atomic bomb?" *(laughter)* Of course, the great thing that Laura did, and the thing that's been mentioned so often here, is that she managed to write the best book about life with a physicist that has ever been written. There have been several written, but none of them compare to it. So when she died, rather suddenly, and a group of us, friends and admirers, crowded in the chapel at the University of Chicago to hear Alice Smith and Ruth C?? speak about Laura, they were not speaking of her just as the wife of a great physicist, but they were speaking about her as a great lady.

Thank you.

(applause)

Chapter 24
Glimpses of Fermi in Chicago and Los Alamos

Richard Garwin
IBM, Watson Labs
and
A. D. White Visiting Professor, Cornell University

Figure 30.
Dick Garwin explaining to Hans Bethe with Kurt Gottfried as the audience. Courtesy Cornell Laboratory of Nuclear Studies.

My talk is titled "Glimpses of Fermi in Chicago and Los Alamos" because I reckon that a lot of what I had to say would have been said before. But I'm not in disagreement with very much of it. I went to the University of Chicago in the fall of 1947, and my wife, Lois, and I were there until December of 1952. After being at Chicago, attracted there by Enrico Fermi, and I guess I took a course or two from him. . .after a few months, I was not doing anything with my hands and went to Fermi and asked if there was something I could do in his lab. He told me that there was, and I should come and see what I could do. I had a very happy time in 206 Ryerson, where I found Fermi and Leona Marshall and Jack Steinberger. Jack was doing some cosmic ray muon capture. Leona and Enrico were trying to study the formation of positronium. I helped out with Geiger counters, if you can believe it, to which we put little threads that had been soaked in a solution of sodium 22. Unknown to us, early on, we had competition from MIT, and Martin Deutsch scooped us in a technological win, because he was using the first developmental photomultiplier tubes from RCA. Pretty soon we had photomultiplier tubes, but Martin was so far ahead that, pretty soon, we gave up the positronium experiment. However, I used the photomultipliers in my thesis.

People have said that Enrico was competitive, and he was. He would also like to bet. You could provoke him into a bet, which was somewhat unwise. *(laughter)* I had to make a vacuum chamber for my work, which was a piece of plastic pipe, and he bet me it would collapse when I evacuated it, but he had estimated and I had figured, and it didn't collapse.

There has been much said that Enrico could solve complex problems by dividing such a problem into simpler problems and, of course, in order to solve a problem that way, you have to be able to solve all the simpler problems that it's divided into. He told me, after Harold Agnew had left, that he really could solve only six problems. *(laughter)* He went on to say that he could reduce almost any problem to one of the six.

He loved independence of action; he didn't like to wait for anybody else. Being self-sufficient, in theory, he was also self-sufficient in experiment. So when I entered his lab, I found to my pleasure a drill press, lathe, soldering iron—all the things that I liked, and which accounted for the fact that most of Fermi's creations were circular, instead of rectangular, which would have been the case had he had a milling machine. *(laughter)* Indeed, he made the trolley (see Fig. 9), turning to advantage the big magnetic field of the cyclotron, which had puzzled others—how would they get motors to work in this big magnetic field without distorting the field itself? He had this very clever idea of normalizing the particle output of the cyclotron, to the power dissipated in the target by the nuclear interactions and the energy loss of the protons going through it. But there was a considerable time lag and, although the operators in the cyclotron became expert at tweaking the cyclotron, taking into account the lag, it turned out to be useful to make a little analog computer that would compensate for the time constant. Which would have been useful, of course, in 1942, and is probably useful in the control of reactors these days.

Fermi was a person who loved experimental technique. When he voyaged to Europe in 1949, he came back really quite impatient with the rest of us, because, in Europe, he had found a couple of techniques that we didn't have in Chicago. That was the import of epoxy, and of these little knuckles for attaching the bars in vacuum or gas to replace the much larger wing nut devices that had been typical in U.S. laboratories. He got also, from Bill Shockley, six point-contact transistors, soon after their invention. Too soon, in fact. Three of them arrived dead, and I managed to blow out the other two, and the last one had its inherent high noise figure, so I wrote Shockley and predicted an early demise for the point-contact transistor. Shockley wrote back and said that he thought it would have specific applications. In fact, the junction transistor has won over. Things are very different now.

Fermi was not extremely quick. I agree he was not at all like John von Neumann or Edward Teller, but he did solve problems, easy or hard, at the same rate. And he liked to develop intuition; he needed to play with things, either in theory, or in experiment, in order to learn about them. So he was delighted when we got, I think it must have been in 1948, a little klystron-powered microwave test set. One could have in the laboratory light-board experiments, but with wire screens or paraffin prisms for playing with evanescent waves. Experienced, of course, in numerical analysis, he had done a lot of one-dimensional calculations for particle wave functions at various potentials, but he had an idea that one could make an analog computer for the Schroedinger equation, and of course, he knew exactly how he would do it. He would have a compass needle surrounded by a horizontal solenoid; the restoring force would be provided, not by the suspension but by the solenoid, and this would mimic the Schroedinger equation, if the current through the coil varied with time, according to some potential variation with distance. I'm afraid that I slowed him down, because I scorned that approach and told him that I would make him an analog electronic computer, which was two coupled integrators with a curve follower; one could manually follow the curve. He used it, nobody else ever did—although I think that Clive Hutchinson played with it a little after Enrico's death.

Now, I was with Fermi at Los Alamos, the four summers, 1950 to 1953. The University of Chicago paid only for nine months, and especially a junior faculty member had to earn some money the other three months. I was delighted to be able to go as a consultant to Los Alamos in 1950 and to read the history of the nuclear weapons project in the library, in a week or so, before I could contribute, myself. That summer, I shared an office with Enrico Fermi, so I actually saw him working with Stan Ulam and their computers.

(I do not know who to give most credit for the detailed invention of the H-bomb. According to an interview of Edward Teller in the April 24, 2001 issue of the New York Times*, it should be Dick Garwin. But here Garwin is telling us that he worked together with Fermi and perhaps Ulam while at Los Alamos, so they probably deserve credit also. I have at times experienced discussions between Fermi and Garwin. Those two minds resonate together and the result is greater than the sum of the two parts. So it is probably a meaningless question to ask whether one person is the inventor of the H-bomb. We do know that, even before the A bomb was developed, Edward Teller was pushing for "the Super," a bomb of unlimited power whose energy source was mainly thermonuclear. But I doubt that he ever had a workable design. Teller had good physics intuition and he was a good hand-waver. However, in my opinion his political intuition was naïve to say the least.—J. O.)*

The computers of Ulam and Fermi in those days were young women, who would come in—Miriam Caldwell was one—in the morning to present the results of the previous day's run. The run was the use of Marchant mechanical calculators, to fill in successive boxes on a spreadsheet, where various differential equations had been reduced to first-order differential equations, so there was only adding, subtracting, and multiplying, as one crawled one's way across the spreadsheet. And I must say that I understand spreadsheets a lot better now that we have these computers, and one can do 1, 2, 3 or other spreadsheets. In the old days, it really was a pain to do it! But Enrico and Stan would start the calculation, make sure it would run, turn it over to the computer, think about the results, and the next day, provide some other parameters for this problem of the burning of a large amount of deuterium. The first summer I was there, at Fermi's encouragement, I designed an experiment to measure the DT and DD cross sections, and to begin the experimental work, but of course that was more than I could accomplish in a summer.

Fermi was also a lot of fun. You could provoke him not only into a bet but with an appropriate puzzle. We had a little cylindrical toy—I was complaining to Enrico (he and Laura were over one evening for dinner)—this child's toy was a cylinder, and I told Enrico that these houses in Los Alamos were not perfectly level, and I rolled the cylinder along the floor, and sure enough, it went a certain distance, turned around, and came back most of the way. He was glad to accept that these houses were not perfectly level. Then I went to where the cylinder had stopped and I rolled it the other way—and it went a certain distance and came back. *(laughter)* And he was puzzled. Of course, what the cylinder has in it is a rubber band with a weight on it, that it winds up, so if you're allowed to shake it, you see what it is immediately. *(laughter)* And I think it was probably that same summer when we showed him the wire recorder, the predecessor to the magnetic tape recorder, that we used for sending messages back and forth—you'd get 15 minutes, or half an hour, or even an hour of recording on one of these little wires, and send it for a few cents through the U.S. mail, and if your parents or other correspondents had a similar wire recorder, and if they knew how to unsnarl the tangles that could result with this 3 mil wire, or so. So, sure enough, Enrico agreed that it did a pretty good job on my voice, but he was shocked to hear what it did with his voice, *(laughter)* because he didn't realize that with his command of the English language in 1950 or 51 that he still had such an accent.

Enrico Fermi was a world treasure, and it was a great loss when he died at the age of 53.

Chapter 25
Fermi and Technology

John Peoples
Director of Fermilab, Batavia, Illinois

(John Peoples is shown in Fig. 2.)

What I'll actually talk about is Fermi and his patents, but before I start I thought it would be nice if I would retell a story about how Fermilab got named Fermilab. I just checked with Bob Wilson to make sure I was accurate. I remember that I was a pretty junior person at Fermilab (or then the National Accelerator Laboratory), back in about 1974, shortly before it would be dedicated as Fermilab. Now, this was a time when Italian-Americans felt a little uncomfortable. The most widely known, or at least the most widely publicized, Italian-Americans were engaged in very dubious activities. I would say that the Mafia was quite well known to people. But nonetheless, there were a number of powerful politicians in the American Congress who wanted to turn this around, and they thought that here was an opportunity. Senator Pastori, who was then on the Joint Committee of Atomic Energy, and a number of others, including Frank Nunzio (who must have been a relatively young congressman on the south side of Chicago, because he's still a congressman—will still be for a few more years) pushed Bob very hard to name the National Accelerator Laboratory after Enrico Fermi. That raised a number of problems; let me discuss one of the simplest: by this time, that is early 1974, no one called the National Accelerator Laboratory the "National Accelerator Laboratory," they called it "N.A.L.", and Bob was quite concerned that people would go around talking about "FNAL" *(laughter).* So he was adamant that there must be a better name, that we find a way to prevent that. So, after some discussion, the public would be given two choices: it could be the "Fermi National Accelerator Laboratory," or plain "Fermilab." That way, the acronym "FNAL" would never appear. Somehow, miraculously, a sign appeared on the East/West Tollway, even before the actual dedication—and it was "Fermilab"—and the public, as opposed to the Department of Energy, has been very cooperative since. *(laughter)* The DOE has yet to get it right. *(laughter)*

When I was asked by Jay Orear to speak about Enrico Fermi, I was very apprehensive, to say the least. I assumed that I would be the only one who had not known Enrico Fermi and that does turn out to be true. When Fermi died, I was an undergraduate at Carnegie Tech, studying electrical engineering. Physics intrigued me, but it didn't seem to be a practical choice for a professional. Because one of my fraternity brothers was a third-year graduate student in physics, I would get some flavor of physics, and in particular, he told me about the neutrino, and that too seemed pretty implausible and ad hoc, because here was this thing that was stuck in to rescue energy conservation, and I don't think that Reines had done his experiments at that time—I'm not sure. But through those conversations, and with other undergraduates at Carnegie Tech, I certainly learned who Fermi was, and I knew the tremendous esteem that people seemed to hold him in. Although I was, as I said, a lowly undergraduate in engineering, something about what those people told me must have stuck because, after about five years of being an electrical engineer, I decided to go off to Columbia University and be a graduate student in physics. That's probably one reason why I'm here.

The real reason I came to this symposium was because I wanted to learn more about Fermi and see some very old friends at Cornell. I've accomplished both these goals: I've heard some very nice things, and I've seen some people I haven't seen for 20 years. In fact, the most surprising thing to me, not related to Fermi, is this particular room. My last recollection of this room was that it was a big drafty, barn-like thing—although I always thought it a beautiful ceiling—but the rest of it left a lot to be desired. So, when Jay Orear said that he was going to hold this thing in Rockefeller Hall, I wasn't too sure that he had a very bright idea! But it's worked out very well. Now, a year ago, I gave a speech about Fermi, as part of his posthumous induction into the Inventors Hall of Fame. That occasion forced me to learn about some of his inventions and, of course, inventions are patents, at least some of the time. But the occasion also allowed me to read a lot about Fermi, things that I'm sure all of you know—in fact, I've learned that you know most of these things. In the process of delving into the things that Fermi had accomplished, I was able to get a list of all of the patents that he had been awarded. I did this partly because Fermilab has a history of accelerators project (one of the members of this audience, Adrienne Kohl, works on that). I went to her and asked for some help because it was getting late, and I really needed to know something about Fermi, *(laughter)* and I was curious about his patents. So, with a great deal of effort, she was able to delve through the material and go down to the Regenstein Library, and she was able to assemble the list of patents that I wanted.

Now, I was struck that the accounts of Fermi's experimental work all testified to his uncanny ability to design and build experimental apparatus that would answer experimental questions very cleanly. One can see this just by reading the accounts of his research—granted, I didn't have personal experience—and you can follow how he came to design a nuclear reactor. You start back with those lovely experiments in Rome and work your way through, and it becomes obvious as you read through those things the basic principles of a nuclear reactor. Of course, the CP1 and the CP2 were prototypes that demonstrated most of the principles of nuclear reactors. Now, Fermi—and this is what impressed me (I'm an experimentalist, and I have some interest in detail and in getting things "right")—he didn't just sit down and enunciate a few elegant theoretical principles; he was deeply involved in the practical matters, like controls, shielding, and, according to what some people said here today, safety, although with regard to safety, I doubt if Fermi's CP1 at Stagg Field would make it past Admiral Watkins's tiger teams. *(laughter)* In fact, I believe that CP1 is somewhere out on the Argonne reservation, and they don't know where it is, and it caused a fair amount of difficulty with the Argonne tiger team, trying to account for it—where was that stuff? At any rate, there is a list of 13 patents, and my talk will be short—I will just go through that list, just to find out how things started.

The first patent he made was the process for the production of radioactive substances, and that was with the entire group working in Rome: Amaldi, D'Agostino, Pontecorvo, Rasetti, Segré, and Trabacchi. It was applied for in 1935, and it was the only patent that was awarded to him in his lifetime. The award was sometime in 1940. All the other patents came quite a bit later (because of security regulations). But already the idea for a moderator was there. From things that I've read, it was suggested by Professor Corbino that this was an important thing and you ought to get a patent for it. But the fact that all these other things come along later suggest that Fermi really did have some notion about what was going to be useful—the perception that some of these ideas might be very practical.

The next thing was the test or exponential pile—and I suspect that that derives from work at both Columbia and Chicago, and then there are various things called neutronic reactor. Now, the neutronic reactor is considered to be the first and basic U.S. patent for nuclear reactors. And then a method of operating a neutronic reactor, in about 1944. Then, in 1945, there are eight patents, all rather important, having to do with the details of how you proceed. Now, I have to remind you, these are not the dates the patents were awarded. I think (for security reasons) the earliest is about 1955. They all occurred after his death.

So, a chain-reacting system, with Leverett; a neutronic reactor again, by himself; an air-cooled neutronic reactor, with Szilard; and again here is a very practical sort of thing:

testing material in a neutronic reactor, with Anderson; again, a neutronic reactor with Szilard; again, another neutronic reactor, with Zinn and Anderson. I find one interesting because Zinn, in many ways, did an enormous amount of work on nuclear reactors—very important work. So, here are people he wrote these patents with, sort of starting off doing very important things. And again, another one, a method of testing neutron fission materials for purity, with Anderson. The last one, in 1945, was the method of sustaining a neutronic chain reaction, with Levertt. And finally, another very practical thing—you've got to shield these things—a neutronic reactor shield.

My perception is that these patents are really the fundamental base from which the real nuclear reactors developed. I look on it today as a director, instead of someone following this as a student, years ago. One of today's "hot buttons" in Congress is "technology transfer." The relevance to the DOE's laboratories is being questioned today. Congress wants to know what we've done lately to improve America's competitiveness. What's really very nice is to look at how Fermi and his Chicago colleagues transformed some very basic physics ideas into patents for the technology of nuclear reactors. They certainly did a superb job of "technology transfer." I only wish that we were as successful at this sort of thing today as he was then.

Thank you.

Chapter 26
A Different Perspective

Nella Fermi

Daughter of Enrico Fermi

(Nella Fermi is shown in Fig. 2)

In speaking about my father I immediately run into a major problem, and that is that my mother used up so much material about him in her book that I have problems getting to something that is not in her book, that maybe some of you don't know. But as for memories of my own, and I think maybe, well I think that a very different point of view was Harold's [Agnew], because, after all, he showed my father from his feet, and that's a different point of view. *(Agnew had shown a video of Enrico and Nella's footprints.—J. O.)* But my point of view is that of a daughter, and that of a child, really, because my father died when I was only 23, and so that most of my memories of him are as a child or a very young adult.

Like my parents and my brother, I was born in Rome, but I remember little of Italy and less of my father. At the time we lived there, my father was distant from me as a child. There's a picture in the family album, and you saw it today, of him awkwardly holding me as a baby and it shows that he was a little bit awkward with babies. You didn't see the companion picture, which is of him holding a black sheep, in exactly the same pose. I think both pictures are in the family album, and it got to be kind of a family joke, and it wasn't one that I really appreciated. I can appreciate it better now.

But anyway, for the most part, my father had very little to do with us when we were children, and I think it's too simple to say that he was too busy with his work and that he had no time for my brother and me. I think he was certainly absorbed in his work, I don't think any of you have any doubt about that, but beyond that, he was a man of reason, and he was a physicist through and through. And he could not relate to us on an emotional level, so it wasn't until we were old enough (and I quote from him) "to talk to" that he could approach us, and that he could approach us on his own level. With adult hindsight I am convinced that it wasn't that he lacked emotions but that he lacked the ability to express them, and I don't want to go into that very deeply, because it would take me an hour, *(laughter)* but I just want to say that. Particularly, as has been mentioned by various speakers, that he was not really warm, and I agree with them, but it was not a lack of feeling, but a lack of expressivity. *(I feel he was at least as warm as might be expected for a thesis adviser and that he was warmer and kinder than many other physicists. See my appendix at end of this chapter.—J. O.)*

My mother also, as so many mothers do, interposed herself between us and him: "Don't bother Daddy." Occasionally she appealed to him for help with discipline, so he appeared a stern and unapproachable figure, very different from my later view of him. On one occasion when we still lived in Italy, he came back from a trip to North Africa. Don't ask me what he was doing in North Africa. But he had many presents for my mother from Tripoli and Angola. There was a woolen bag with pictures of animals woven on it, there was jewelry, and there were other delights. I sat in my parents' big bed when my mother opened my presents, and finally I couldn't restrain my fears: was there nothing for me? My mother was conscience-struck. What did I want? Any of the presents I could have. I loved her for it, but what I wanted was something that my father had brought specially for me. Years were to pass before my wish was fulfilled, before my father could give me, not a material present, although I suspect he gave me material presents as well, but something of himself. As you know, my father was a talented teacher, and although I did not know him, as some of you did, as a teacher in the formal sense, from him I learned many strange and wonderful things. I could not always keep up with him, but that did not diminish the wonder. Just as a magician takes a rabbit out of his hat, and the audience is fascinated because they don't understand, so my father took rabbits out of his head, and I was fascinated. Increasingly, as I grew older, there are occasions that stand out in my mind when my father unexpectedly turned his attention to me; often these occasions opened new vistas.

One afternoon, when I was about 10 years old, my father offered to teach me algebra. A friend's older sister took algebra in high school, so I knew algebra was a very difficult form of math. At that point the only math I'd been acquainted with was arithmetic, as, of course, most 10-year-olds are, and I wasn't very good at arithmetic. I never could add, I never could subtract, multiply, and divide. I still can't, I have to use my calculator and my computer. So, he figured he could teach me algebra in a couple of hours. *(laughter)* And I'm not sure if this was a tribute to me as a scholar or to him as a teacher, but I suspect the latter (he was not always modest). *(I would say he was modest about many of his great contributions to science, but he was not modest about other things such as his knowledge of English grammar and spelling when correcting my thesis.— J. O.)* And he was an excellent teacher, and he began by explaining that in algebra you use letters instead of numbers; for instance, you might use x, y, or z to represent some number.

"But what number?" I asked.

"Well," said my father, "it could be any number."

I was puzzled and intrigued. First of all, I was—well, it was obvious why I was puzzled, but I was intrigued because it suddenly struck me that you couldn't possibly have to multiply letters and, therefore, that would get you out of this whole problem of addition and multiplication, *(laughter)* and that seemed to me a good thing.

"But what number?" I asked.

"Well," said my father, "it could be any number."

I was puzzled. My father pointed to a door and asked me what it was.

I said, "a door."

"No," said my father, "it's a beaver."

"It's not a beaver! It's a door!"

"If I say it's a beaver," said my father with assurance, "then it's a beaver. I can name it whatever I want. Just like Humpty Dumpty from Alice in Wonderland. 'When I use a word,' Humpty Dumpty said, 'it means what I choose it to mean. Neither more, nor less.' 'The question is,' said Alice, 'is whether you can make words mean so many different things.' 'The question is,' said Humpty Dumpty, 'which is to be mastered, that's all.'"

Like Alice, I was puzzled. And I didn't learn algebra that afternoon, that was one of the things that was over my head. Yet some of the lessons stayed with me. I was too young to absorb what he told me, but later when I did take algebra in school, I took to it readily. I had always had trouble with arithmetic, and suddenly math became surprisingly easy. What my father had tried to teach me that afternoon, together with other abstract concepts, laid the groundwork for my later understanding of the basics of algebra. Characteristically, he had gone to the heart of the matter and reduced it to its simplest element. You can substitute one word for another, one symbol for another, a letter for a number. He had expressed himself in words that I would remember, because I could understand their literal meaning, even though not their implications. He had approached me on an intellectual level, because he only knew how to approach me on an intellectual level, and he had overestimated my ability to meet him there. Stretching my mind after knowledge that is just out of grasp is something else that I learned from my father.

In our living room, we had a couch with a wooden back, and this was probably two or three years after the algebra thing. Whenever people sat in it, they pushed it against the wall, marring the paint. My mother wanted to prevent this, so my father put stops on the floor behind the legs of the couch. I followed around, watching him work and fetching tools. My father found two pieces of scrap wood in the basement and proceeded to nail them to the floor behind the legs of the couch. My mother was horrified. Two such rough pieces of wood in her living room? Why couldn't he use nice wood? The living room was filled with elegant furniture, much of it antique. Although my parents could not take money out of Italy, they were able to have their furniture shipped. My father protested: the wood couldn't be seen behind the couch! My mother retreated in a huff. The wood was there, whether it could be seen or not. My father turned to me and said, "Never make anything more accurate than is absolutely necessary." *(long laughter)*

I'm not sure if he intended this statement for my edification or only to justify his carpentry. I was delighted, and as time went on, took this possibly offhand comment as a kind of eleventh commandment. It wasn't easy to live up to. When I became a jeweler, I puzzled what constituted "absolutely necessary exactitude." Should one leave the backs of jewelry rough since, like the stops on the couch, they could not be seen anyway? Or should jewelry be perfect back and front? The back of the jewelry paralleled my mother's feelings about having rough pieces of wood in my living room. I was torn between my mother's teachings and my father's.

My father was not always consistent. Once, when I was in my teens, my mother went away for a time, leaving me to keep house for my father. During this period, he hovered around the kitchen a lot, something he seldom did when my mother, a more competent cook, was in charge. His hovering was partly in the spirit of helpfulness, and partly to avert such catastrophes as I might concoct. One of these times, I took out a package of frozen spinach and started to put some of it with water in a pot. My father was intent on the instructions.

"Wait," he said. "It says half a cup of water." And he went to get a measuring cup.

"Never make anything more accurate than is absolutely necessary?" I muttered under my breath. *(laughter)* Once I did impress him with my cookery. I made some iced coffee. My father drank it with great satisfaction. "Very good iced tea," he said. *(laughter)*

One day, and again, this probably when I was in my teens, and maybe even my brother, who is Rachel's father, and a good bit younger than me, was also perhaps in his teens, my father brought home a strange substance which was soft like well-chewed chewing gum, yet could be shattered like glass. He told Giulio and me that he had been given a sample of this new material, so that he could suggest possible applications for it. We were fascinated. He showed us how we could pull it into a long thin string like chewing gum if you pulled slowly, but as soon as you jerked, it cracked! You could shape it into a hump or scratch designs on it, but leave it alone, and it melted into a blob. A blow with a hammer shattered it like glass and sent it flying all over the room. My father wouldn't demonstrate that one (I had to take it on faith), because if he did that it would be all over the room and we'd never get it back. I asked a lot of questions and got a physics lesson: this stuff was basically like glass, my father said, it was a liquid. Glass is not a liquid, I said. It is, said my father. I thought he was pulling my leg, but he convinced me. Glass had the molecular structure of a liquid, and given sufficient time, would melt into a blob, but it would take ages. I wouldn't be around to see it, he wouldn't be around to see it, none of us would be around to see it. We spent a happy afternoon with the odd material. My father was puzzling about possible applications but also taking boyish delight in the strange properties of the material. We thought about using it to patch up cracks on windows, but that would be no good, it would only drip down into a blob. He asked us for suggestions for possible uses, but we could come up with none, and neither could he. In spite of the fun that we had with it, we missed the obvious use. It was a great toy. Later it was marketed as Silly Putty. *(laughter)*

On one occasion, I managed to teach my father something. In college we read the works of Thoreau. I came home full of ideas of civil disobedience. My father did not approve. "It is the citizen's duty," he said, ponderously, "to obey the law. He may try to change the law, but until it is changed, he should obey it." I saw the counterargument and found it readily enough. "What about Hitler and Mussolini?" I asked. I could almost hear the wheels spinning in his head. In five seconds, the answer came out: "You're right." I reflected that not many people are so open to rational argument.

It was hard to buy presents for my father; he disliked neckties and was a man of simple tastes. Like so many fathers, he was well provided with much of what he wanted or needed. Only once did I succeed in finding a present which really grabbed him. I was walking by a store window and saw a display of strange birds, which were dunking their heads in a glass of water, over and over and over again. Now, they don't seem to build birds quite like they used to, but there he goes. *(laughter) (At this point a friend of Nella's brought such a bird to the podium and started it in motion.— J. O.)* At first I thought that some hidden electric mechanism was responsible for their motion, but on closer examination, this was not so. Were the birds perpetual motion machines? Of course I knew this was impossible; well, I only knew it on faith, really, I knew it because my father told me so, right? But anyway, I could find no logical explanation. Just the same, I thought it was a perfect present for my father. It should provide him with amusement, if nothing else. It did that, and more. My father was delighted and set about figuring out how the bird worked. Characteristically, he mixed learning with play, and both with teaching. In this my brother and I were his first pupils. We were his audience as he set about figuring out how the bird worked. He was learning, teaching, and having fun. I had thought that the workings of the bird would be obvious to him. Instead, I saw how he approached a problem. I learned not only how the bird worked but more importantly, something about how to learn. As the bird swiveled on a metal stand up and down, my father puzzled over the bird, and then he did some simple experiments. First he filled the glass with alcohol instead of water. *(laughter)* The bird's motion accelerated. *(laughter)* Next, he put a large glass bowl over the top of the bird, glass and all. Gradually the bird slowed down, and finally, came to a stop. "You see! It must be alive," he said, "It gets drunk on alcohol, *(loud laughter)* and it asphyxiates if it doesn't get enough air." He thought about it some more and then came up with a solution. He considered putting a question about the bird on the Qualifying Exam. *(loud laughter)* But unfortunately, he was unable to restrain himself, and he talked about it to some of his students, perhaps someone in this room, I don't know, and of course, after that, it wouldn't have been a fair question.

I would like to make two small awards. One to Jay, for having done such great work, and one to Rachel Fermi, Enrico's granddaughter, because she is Enrico's granddaughter! *(At this point she gave one bird to Rachel and one to me. I was surprised to see that Rachel had such a bird in her hotel room at the Rome Fermi Congress.—J. O.)*

And now, I'm open for questions.

Question: I was wondering if the reason we had some of our instruments named after *Winnie the Pooh* characters was because Enrico was reading *Winnie the Pooh* to either you or Giulio? This would be in the early 1940s. Did he read those books to you?

Nella: I don't think so. I certainly read those books. It seems to me that by the time I got to reading them, I was able to read them on my own.

Question: The lack of emotional connection that you talk about is certainly a familiar story with many great scientists. Einstein's children, for example, had serious complaints, and I think we all know lots of other cases of that sort. Einstein described himself as "a horse for single harness" and complained about his inability to make that kind of emotional connection, and I wonder what the reason might be. Is it that the competitive aspect of science is so much that top-notch scientists have to spend all their time doing science?

Nella: I don't think so, I think that there are top-notch scientists that don't have to spend all their time doing science. I think that there were particular reasons in my father's case, one of which had to do with the death of a brother at a young age when they were very close. I think that had to do both with his being distant from people . . .you know, once burned, twice shy. He was close to his brother and his brother had died. The other thing had to do with that his brother was considered to be much smarter than he was, and he had to be very competitive, and it's very hard to keep up with a dead brother.

Question: Nella, I have to confess to a question of, well, there's a number of people I've been in contact with who do not know who Enrico Fermi is, and I was wondering, of course there are different places where you went where he was sort of a celebrity, but have you been conscious of the fact that you were sometimes in groups where people don't know what your father has done?

Nella: Yes, I mean, over a period of time I've been conscious of being in places where people *do* know, and it took me a long time to be able to take this in my stride, because for quite a long time I felt I was only seen as my father's daughter. By the time I got to be about 45, I decided I was a person in my own right, and then the whole thing ceased bothering me. And it didn't happen all at once, because when I was a very small child, he really wasn't all that famous. He was probably famous in physics circles, but it was really after the war, when the whole thing about the bomb came out, that he became, at least at that time (I think perhaps the memory has faded), but at that time, he was very famous, and a lot of people had heard about him, and I think that maybe now, I do live in Hyde Park, which is the University of Chicago neighborhood, so I run into a lot of people who know who he was, but I run into people who didn't, and, you know, it doesn't bother me, either way.

Question: (muffled) books, literary character. Did he read (**muffled)?**

Nella: His favorite book, or series of books, was Captain Horatio Hornblower. *(laughter)*

Question: What did the children in Los Alamos think was going on? You were there from 1944 until 1945?

Nella: I don't know what the other children thought. I know what I thought. I kept it to myself. I thought that something was going on about nuclear energy. I don't think I thought of a bomb, I thought about nuclear energy. And the idea was a fairly obvious one, I mean, there were so many nuclear physicists around. . . .And it even said in my physics book that in future years, there perhaps would be nuclear energy to power whatever we needed powered. So that was what I thought about it, and I thought that it could be used, because it was war work, I thought it might perhaps be used for ships, that would go faster, you know, airplanes, or whatever. I did not think of a nuclear explosion.

Question: Did he make you aware of a career that he had picked out for you and your brother?

Nella: He was, well, he was rather disappointed when I decided to go into art. I think both my parents were, but the attitude was more or less "not much lost, she's only a girl." I think he was anxious, though, for me to have a career. And when I wanted to go to the

Art Students' League in New York, and at that time, the Art Students' League was not offering degrees (I think it is now), my father didn't want me to go, because he thought I should get a degree. My mother didn't want me to go, because she thought I shouldn't be in the big, bad town all by myself. So, I found myself in Iowa City, which had a very good art school, but didn't have quite the ***(muffled)*** of New York. And after I got my bachelor's, he said, now you should get a master's degree, and I really didn't see much reason for it, but I did get pressured into getting a master's degree, and after I had done that… and he kept saying "just in case". . .and what I thought at the time the 'just in case' meant was that, "well, you might not find a husband". What I think he meant. . .his sister had been widowed at a relatively young age with three small children, so I think he thought, well, you know, a woman should . . .But I think it went further than that, because after I got a master's degree, he said, well, you should get a doctor's degree, just in case. Now you don't get a doctor's degree "just in case"! And I didn't until many many years later when I felt like it.

Question: What was the table talk like during the McCarthy period?

Nella: Well, I think that the table talk was very much anti-McCarthy. Obviously, the whole Oppenheimer business came up. . .I don't even remember so much about the table talk, but I know which side my father was on, even though he was obviously a much better friend of Teller's than of Oppenheimer's, but he tried to turn Edward around, but he did not side with Teller in any sense. Even though I don't think that he was close to Oppenheimer at all, he felt that Oppenheimer was in the right and should be defended, and, you know. . .and did so, so there was no question about that. But I think he was one of the few people that would still talk to Teller after the whole thing was over. . .and I think that at the end of Emilio's book, there's a bit where he says that he is. . .the name is not mentioned, but I feel convinced that Teller is being referred to. . .that says that as he was dying, he was trying to save his soul, and he said to Emilio, "what better work for a dying man than to save a soul," and I'm sure that he was referring to Teller.

Question: ***(muffled)*** grammar, spelling ***(muffled)*?**

Nella: My mother did, without much success.

Question: What about your father, what ***(muffled)*?**

Nella: It wasn't an issue for him. He wanted me to get an education, that was clearly an issue, and he was quite strong on that. And, as I said, I don't think it was the "just in case." I think he really. . .I think again that was one of the things he couldn't express, but he felt it was important to have an education, even for a girl.

Question: This is partly my memory, and partly from several sources. It's in regard to your grandfather. I don't know what his ***(muffled)*** but I ***(muffled)*** you first heard that he disappeared ***(muffled)*** very upsetting ***(muffled)***?

Nella: Well, it was certainly a very upsetting experience, particularly for my mother, but I think for both of them. Obviously particularly for my mother because he was her father. I think that when we left Italy, my parents told people that we were going away for six months, and they did that not really to deceive. . .except for their very close friends such as the Amaldis and people like that. . .but otherwise, they did not tell their family they were going away for a longer time, I think because they were. . .this was part of my father's caution. He was afraid that if he said he was leaving Italy, that he would be stopped. So, they really made it up that they were going to take a trip and visit America, and then come back, and that was the way it was stated. As I think someone else mentioned, there was a good bit of secrecy about soliciting drugs in America, and. . .I'm not sure, I'm losing track now. . .Oh yes, so I think that my mother was having a lot of guilt about leaving her father behind. My grandmother had died of natural causes some years before we left, and there were two sisters and a brother who were still in Italy, so it wasn't as if she was abandoning him altogether to himself. I'm not sure when they learned about it. I know that at some point, my mother told me that they had heard. . .we had heard before that that he had been taken by the Nazis, but then at some point, my mother had talked to someone, whom she said. . .and it was in a moment where she said. . .because I think that it might have been a way of protecting me, rather than the strict truth. . .she said that he had died on the train. Now, about three or four years ago, I talked to a man who had done some research into the subject, and he seems to have come up with some very conclusive evidence that my grandfather made it as far as Auschwitz. He was one of the first to go in the gas chambers. He was an old man, of course.

Male voice: He was a high-ranking naval officer.

Nella: Yes, and he thought that being a high-ranking naval officer. . .he was an admiral, although he only became an admiral in retirement. . .but. . .and I had the feeling he was, perhaps, kicked upstairs, but he had given his life to the service of his country, and he was a gentleman of the old school and was convinced that they would not bother him. After all, he was an admiral, and he had served his country well, and he wrote a very long diary, most of which was very dull, but the end part. . .it's not a diary, it's a memoir . . .but obviously he was working from a naval log, and it reads about like that. But the end part is a diary, and it takes it almost to the last day, and he would have had opportunities... my aunt, my father's sister, was practically running an underground railroad in her basement, and she had gone over to persuade him to come and stay with her, and he had other friends and connections who were not Jewish who he could have stayed with. . .it was really easy to hide in Rome. . .and it was the old people, typically, who got took, but not always, and it was not the kind of thing that happened in Germany, or in some of the other countries, largely because the population was simply not behind it.

Question: Do you care to say anything about your brother's attitude toward your father?

Nella: No.

Question: ***(muffled)*** any discussion of the possibility of the ***(muffled)*** your father ***(muffled)*** being caused by the amount of radioactivity that he ***(muffled)***

Nella: I don't know that there's been discussion about it, but I personally have discussed it with two doctors. One doctor was one of the doctors that was on the scene at the time, and he said absolutely not. Again, I'm always a little suspicious of these things, because doctors have way of trying to protect you from stuff that you don't really. . . mean, my father is dead, and when I asked him, he had been dead a good, long time, and it wasn't going to. . .it was only a matter of trying to satisfy my own hunch. I had sort of had a hunch at the time, and I was just trying to check it out, that's all. I wasn't going to be. . .But, that definite "no" . . .I don't know whether I believe it or not. The other doctor I talked to is my own personal doctor, who was not there at the time, and he said that he had always assumed that, in fact, that was the case. But, he also qualified it and said that there would be no way of knowing, that it would look like cancer in any case, that the only thing you could really say or speculate was, yes, this man has been exposed to a lot of radiation, and therefore, this may have been due to the radiation.

Question: ***(muffled)***

Nella: He had stomach cancer, but after his death when they did the autopsy, they found that he had another cancer as well, which was apparently unrelated to that. That, to me, strengthens the case for radiation because it would seem that there was damage all over the place.

Question: When you were a child, did your father take you to the lab and show you how the cyclotron worked?

Nella: I probably got took to the lab once or twice, I don't remember very well, I mean, I'm sure I got took at least once or twice. It's not something that made a deep impression.

Conference chairman (Carl Sagan): Nella, thank you very much for sharing all this with us.

Appendix to Chapter 26

Jay Orear

There seems to be an apparent contradiction about Fermi's shyness and personal warmth. In her talk Nella said, "And he could not relate to us on an emotional level, so it wasn't until we were old enough (and I quote from him) 'to talk to,' that he could approach us, and that he could approach us on his own level." Jane Wilson in her talk said, "I noted that Fermi was a pleasant character, who didn't seem conscious of being a great man, that he just sat on the floor, like a regular human being, and that he seemed to have a sense of humor. . . .At this first meeting, I was somewhat surprised, having known Enrico off and on for quite a while, to find, after this ebullient, extroverted man, this very serious, shy, reticent woman (Laura)." I have observed that Fermi was quite warm with Jane Wilson and a long list of others like Dick Garwin, Herb Anderson, Leona Marshall, Joan Hinton, Jay Orear, Art Rosenfeld, Harold Agnew, etc. Nella is probably correct in that he was not as good at relating to children. And as with any human being, he related better to some individuals than others.

In the first 15 chapters I discussed several examples of Enrico's warmth. The following is a list of some of these examples plus others including personal discussions.

1. Fermi would have lunch with students in the student cafeteria.
2. Fermi enjoyed square dancing, party games, tennis, swimming, ice skating, hiking, mountain climbing, and skiing with students and friends. Harold Agnew and I both conclude that Enrico and Nella worked well together in planning the square dance parties.
3. Other parties at his house with students and others. Once my wife and I had a party for the Nuclear Emulsion Group at our house including Fermi, the grad students, and scanners. On social occasions he seemed to treat professors, students, and employees as equals.
4. Fermi's office door was always open and anyone was welcome.
5. Personal discussion: Enrico's problem with insomnia.
6. Personal discussion: The encounter one day of his son with street ruffians and Enrico's reaction to it.
7. Personal discussion: When a postdoc of Bethe published a paper that had ignored my previous work on the same subject. Fermi was upset with Bethe over this and he did speak to him about it.
8. Personal discussion: When Enrico took Giulio to a real barbershop as a birthday present. Previously all his haircuts were done by his mother.
9. Personal discussions: tear gas guns and dosimeters (see Ch. 6).

Such personal discussions were infrequent, but I did have the feeling that Enrico was always available. Whenever he was around, he was the life of the party. He was not a cold person. He usually had the twinkle in his eyes and the smile shown in Figure 1.

Chapter 27
Arthur Rosenfeld, first panelist
U. of California, Berkeley

O**rear, substituting for the chairman**: These are Chicago-related people who have not had a chance to speak and who were not previously on the program. This was the philosophy in choosing the panel. Let's start with Art Rosenfeld.

Art: I have three small reminiscences. One thing which is only partly Fermi, but I think is one of my first recollections when I first went to Chicago. This was, I think, about a year and a half after the end of the war. Cars were still pretty old. Enrico came to Ryerson and Eckart (he would chain his bike up in front of Eckart every day), fairly early—about 8:15 a.m. I used to go in very early. too. Somebody said this morning that he didn't remember how early Fermi came to the lab, but it was pretty early. *(8:27 a.m. for Fermi's INS office—J. O.)* The funny scene I remember (I have a photograph, but I didn't bring it) was that Fermi would always appear on a quite beatup bike—actually, in Chicago in those days, you didn't want a very shiny bike, it would get stolen—so he would appear on a beatup black bike, and chain it to the iron link fence on the tennis court in front of Eckart. About that time, the new man in the stockroom (I can't remember his name, Fred, I think) would arrive, and he had a brand new black Chrysler. Moreover, Fermi apparently didn't go out for lunch every day. I've heard with surprise about the two- to three-hour lunches in Italy, but Fermi would usually have some bag full of lunch in his beatup parka. Fred, on the other hand, had learned that it looks good to come to work with your lunch in a briefcase, so this very distinguished man would get out of his Chrysler and get up to the door of Eckart just about the same time usually as Fermi would finish chaining up his bicycle and wander in with his old beatup navy pea coat, and a sandwich sticking out of his pocket. *(laughter.)* If you didn't know what was going on, you would expect Fermi to open the door for Fred, the stockman, to walk in, but, of course, Fred would get up to the door first and open the door for Fermi to walk in. *(laughter)* I thought it was all pretty charming.

A comment about this press conference, which is near the end of the film *The Life of Enrico Fermi*. I guess that I organized that, and I'm trying to remember the interesting things that went on. Although I told Jay that I thought it was after Fermi came back from the summer school in Italy, I now think that it was before that. *(A photo of the handout is shown in figure 18. At the top it is dated October 4, 1954. At the end it refers to Eisenhower's citation of July 8, 1954, which date was after Fermi had left for Italy. So this press conference was held the month before Fermi died.— J. O.)*

The Oppenheimer matter had boiled up, and Fermi was, in fact, beginning to be somewhat regretful of not having a lot of press conferences, and so on. Then came a book called *The Hydrogen Bomb*, by Blair and Shepley, two people that I hope you've all forgotten about since then—a very virulent book, completely black and white—there were no in-between cases: Oppenheimer was a "communist dupe" and Teller was a "great saint," and anybody who didn't want to build a H-bomb was a "dupe." Somehow I got hold of the first one or two copies—I had gone over to the University of Chicago Press, which was all of one block from the institute—the day it came out. Fermi had said to get him one, too, so I came back with a couple copies of this book. That was in the afternoon. After supper, I went back to work, as I usually did, but I decided to start reading this book. It made me so goddamned mad, I stayed there all night! I had just about finished it at 8:00 in the morning, when Enrico arrived. He asked me what I was doing there, and I told him that I had found the book so interesting and awful, that I had stayed up all night reading it. He said, "Well, that's interesting, I didn't read the whole thing, but I stayed up until about 2:00 in the morning, and I think it's a pretty rotten book." So I said that if he was willing to say that in public, why don't we have a press conference? And he said, "Jolly good idea." That was 8:00 in the morning, and I had never organized a press conference before, so I called my friend Goodwin, who was on the staff at *Newsweek*, and asked him who I should call, and how long would it take people to show up. We decided that right after lunch was a good time for a press conference. I made 10 or 15 phone calls, and Fermi went back to finish the book, saying that if we were going to have a press conference, he ought to have read the whole thing, *(laughter)* which I thought was a pretty good idea. A large number of people showed up, including TV and radio, and we got pretty good publicity, and that's how that came about. I was amazed at how quickly it happened, and what a good sport he was, at any rate. That's what I remember of that.

I guess that those are the couple things that have occurred to me during the day, so I will pass the microphone on to Maurice Glicksman.

Chapter 28
Glicksman Comment by
Maurice Glicksman, second panelist
Provost, Brown University

I'd like to tell a couple of stories that I've not heard yet today, about Fermi, and also say something about the three-half resonance work.

One I remembered in Val Telegdi's description of the courses that Fermi gave—this reminded me of the date—in the winter of '51, Fermi decided that graduate students ought to have a chance for a good graduate course in solid state. So, he and Cyril Smith offered such a course. I signed up for it. The plan for the course was that he would give one week of lectures on theory, and Cyril was going to give the following week's lectures on experiments. The first week, Enrico came, and he gave an excellent set of lectures on bringing atoms and molecules together, and building up condensed matter, and something on symmetry, and then the next week, when Cyril arrived to give his lecture, Enrico came and sat with his notebook in front, opened up his notebook, took out a sharp pencil. . . and after a few minutes, closed the book, and sat there through the lecture. Cyril was, I think, a little nervous with Enrico being at the front, and it was a disorganized lecture, and consisted of both some sort of general discussions, then launching into some phase diagrams of alloys, one after the other. An attempt, I suppose, on his part to show some examples of experiments in solid state. Enrico did not show up for the rest of the lectures that week, then showed up the next week, and said that, for some reason, Cyril was unable to continue giving lectures, and that he would complete the course, covering both theory and experiment. We never got an explanation as to what happened with Cyril.

I did run into an experience, possibly the only one you may hear today, about Enrico being absolutely at a loss. It occurred one time at the institute when we were having the annual Christmas party. Enrico and a group of us were in his office, working on some data, and someone came up and said that the whole institute was celebrating Christmas and that we had to be there, that we couldn't stay in the office just working on physics. So we came down to the room where all the people were, and they had been drinking a little bit. There was a woman, a secretary at the institute, who had a few drinks under her belt, and when Enrico walked in, she came up to him, put her arms around him, and said, "Professor Fermi, I was at a party last night, and I told everyone I worked for the greatest physicist in the world. Doesn't that deserve a kiss?" *(laughter)* Well, Fermi saw Nate Sugarman (who was known to all of us as a man who knew how to deal with ladies) *(laughter)* and he said, "Nate, can't you help out?!" *(laughter)* That was the only time I ever saw Nate at a loss. *(laughter)*

My own situation was that, after having a stint with Dick Garwin for a short time in his laboratory, Herb Anderson had asked me to join the group. I did the first problem, which was okay, and then I was supposed to choose a thesis problem. Enrico wanted me to build a magnificent cloud chamber, to enable me to do the low-energy work better than these emulsion people, and everything else that was going on, and his view was that you couldn't trust things at high energy because there were too many unknowns. You really had to look at the low energy (low energy being 100 MeV or so.) I wasn't very excited about that, but they sent me off to Michigan summer school, where Carl Anderson was giving a series of talks on how to build cloud chambers, and I designed a cloud chamber. I went to see Herb and told him that I wanted to go ahead with this, but that I didn't want to only look at the low energy, I was going to design a cloud chamber that could handle many different things, and I wanted to find some particles. I had been working on getting the pion beam up to the peak—they hadn't gotten it up to the peak—and I managed to get about 220-some odd MeV out of the cyclotron. I wanted to find a di-meson, produce some new particles with this. (There had been a report from England that there had been such a thing, but it turned out to be a false report.) Herb asked how long it would take to build the cloud chamber, and I said, "Well, if you give me the services of a graduate student and a technician, about a year and a half." He said, "Well, you're optimistic. Three years, minimum." Courteney Wright had been working something like four years already on his hydrogen diffusion chamber, and it wasn't yet working. And he said, "And it will take you another two years, that's five years. You're married, you've got a kid, and my conscience won't let you do it. You can do your experiments in other ways."

I said, "Yes, I can do them with counters." So he said that was what I should do. I asked what Enrico would think of that, and he told me not to worry about that. So I went ahead and designed an experiment to look at the 200 MeV scattering, and I started doing experiments, got the beam out, and got these huge cross sections, which was a surprise to everyone, because they expected the cross section to be going down. At this point, Herb came to me and said that Enrico was talking to him and that he was interested in the high cross section. He said, "He'd like to extend the total cross section measurements that had been done (so far up to 135 MeV) and work with you on that." I said to Herb, "Look, he didn't want me to work on high energy, so I'd gone ahead and done this, I'd found these interesting things. . . .Why should I invite him in on something that looks so exciting?" And Herb said, "Maurice, you'll never be ashamed or embarrassed about having a paper with Fermi." He was right. *(laughter)* So Enrico and a group of us worked together and did the measurements, which showed, in fact, that the total cross sections did go up. Of course, Enrico insisted that I had to have very detailed angular distributions at 217 MeV, which is where my dissertation thesis was, because he was worried about higher order phase shifts affecting d-wave, so I had to make a lot of measurements and spend a lot of time in the cyclotron, and I did that. I finished writing it up, and I knew the phase shift analysis had been done—I did a phase shift analysis on the 217 MeV data, and got the 3-3 phase shift close to 90 degrees, indicating it had probably gone through the resonance at that point. At that point, Herb disappeared from the scene, and now Enrico was in charge of my dissertation and my thesis committee, and I had to satisfy him as to what I was writing in my thesis. I wrote about these phase shifts, and how they showed the resonance, and he said to me, "Look Maurice, you and I are experimentalists, and we do an experiment, and we provide an unusual result. What happens is, the theorists look at that, and they don't know anything about experiments, so they take that data as though it's gospel, retire to a mountaintop, and they spend a year working out a brand new theory to explain your data. They come back a year later, and someone else has made the measurement in between, with a different result that what you got, and your name is Mud. M-U-D. So you have to be very careful in explaining your work, writing it out." So I put in some more weasel words and took out some of the stronger language. I didn't back off from the phase shifts, and Fermi didn't force me to take that out—it was a legitimate solution to the data. Unfortunately, as some of you know, Herb ran into that problem with the Pi-mu, Pi-e ratio some years later, many years after Fermi died, with the wrong result.

Let me close with a sad vignette, about when Enrico came back from Europe after the summer of '54. The day he came back, we were in his office. We were using his office because it had that large table to work on, and ran pion scattering data, and Herb and all of us were absolutely shocked by his appearance. We asked him what was the trouble, and he said that he just couldn't eat. What he said was that food tasted like dirt, and he couldn't get it in. And he looked at me—I was a little bit heavier than I am now—and he said, "Not like you, Maurice, you always enjoy your food. I wish I could do the same." We asked him if he'd seen a doctor, but he hadn't wanted to see a doctor in Europe. And, of course, you've heard the rest of this story. It was a very sad time for all of us.

Chapter 29
Wolfenstein Comment by

Lincoln Wolfenstein, third panelist
Carnegie Mellon University

The last people have talked about Fermi's courses, and back then he didn't teach out of books, but I remember the quantum mechanics course, where students would always ask, "Well, could you tell us where we could find that in a book?" And Fermi said, grinning, "It's in any quantum mechanics book!" He didn't know any. They would say, "Well, name one!" "Rojanski," he said, "It's in Rojanski." Well, it wasn't in Rojanski—it wasn't in any quantum mechanics book. *(laughter)*

Not too many years ago, I found myself in a sort of similar situation. I was talking about the index of refraction of solar neutrinos as you calculated from the optical theorem. I was about to say, "The optical theorem is in any book in quantum mechanics," *(laughter)* but I stopped myself and decided to go to the library and look at some books in quantum mechanics. The imaginary part of the optical theorem is in there, but the real part I couldn't find in any book on quantum mechanics. There is a very good, beautiful presentation of the real part of the index of refraction, and that is, of course, in *Fermi's Notes on Nuclear Physics*, by Orear, Rosenfeld, and Schluter. I tell people now, it's not in any quantum mechanics book, but it's in *Fermi's Notes*.

I was one of the group of 16, after the war, who took the first qualifying exam. A lot of us sat together in the top floor, the fourth floor, of Eckart, in this big office. I came into the office. There was my desk, next to it was Merv Goldberger's, there was Geoff Chew's, Rudy Sternheimer's, and I think Carl Argo was there. . .anyway, about half a dozen of us in that room, and Fermi's office was down the hall. Goldberger and Chew got up the courage and asked Fermi if they could do theoretical theses with him. Most of the rest of us ended up with Teller. Goldberger and Chew did their thesis together. I remember Merv's thesis was sort of a cascade calculation for high-energy particles going through a nucleus, an intra-nuclear cascade. I remember Fermi coming in and saying, "I'll do the first couple of examples." He drew the particle coming in, and had another coming out at this angle. . . drew all the lines, and then he did another one. . . and then he left it for Goldberger and Chew to do enough examples to write the thesis.

I didn't have much to do with Fermi, as far as my thesis went, until I came to my thesis examination. I came back from Pittsburgh to take my final orals, and there was supposed to be an experimentalist. Herb Anderson was supposed to be on the committee, but Herb couldn't come. So, I had Fermi and Teller and Wenzel, that was my committee. Fermi said, "It's all right, I'll be both the theoretician and the experimentalist on your committee." So I would start talking, and I would say, ". . .and now we look at these tensor operators. . ." and Fermi would say, "Ohhhh, the experimentalist doesn't understand that. Would you please explain that?" *(laughter)* So he was both the theorist and the experimentalist on my committee.

Chapter 30
My Life as a Physicist's Wife
Laura Fermi

Erice, 16 July 1975

(This lecture by Laura Fermi was given July 16, 1975, as part of a conference titled "New Phenomena in Subnuclear Physics" in Erice, the Science City on a romantic mountaintop in Erice, Sicily. The full conference report is published by Plenum Press, New York and London. Permissions have been granted by the conference chairman, Nino Zichichi, and Plenum Press to present the full text in this book.—J. O.)

Figure 31. Laura Fermi presenting her paper at Erice.

My life as a physicist's wife began at least one year before I got married, the day Fermi and Rasetti took me to see the crocodiles in the old Istituto di Fisica in Via Panisperna, in Rome. It was sometimes in 1927. Enrico Fermi had been called the previous fall to an especially created chair of theoretical physics, and soon afterwards Franco Rasetti had been appointed "aiuto." They were both 25 years old. As a second-year student of general science, I attended courses in the physics building, but had not been in the laboratories.

The two young men had talked so much about their crocodiles, about having to feed them and take care of them, that to this day I am not sure what I expected: I was certainly disappointed when I saw two shabby wooden spectrographs and was told that they were the crocodiles. Spectroscopy was very fashionable in those days, Rasetti was a spectroscopist, and now and then he made Fermi share in an experiment.

I liked young physicists, and some of the older ones. The year before I had taken the "matematichetta," mathematics for chemists and naturalists, which was taught by Fermi's friend Enrico Persico; and in 1927 I was attending Corbino's course of electricity for students of engineering. Senator Orso Mario Corbino was a fascinating man, short, round-headed and round-bodied, with sparkling dots for eyes. I remember him asking in class whether any really worthwhile students would like to shift from engineering to physics and be trained by the new faculty members, Fermi and Rasetti. One student only, Edoardo Amaldi, volunteered. We, the girls, teased him for the good opinion he had of himself, but the Roman school of physics was born; a few months later Emilio Segré and Ettore Majorana joined it.

I liked Persico, Rasetti, and Corbino, and I don't need to say that I liked Fermi. But I did not like physics. One day that summer I asked Fermi to quiz me and see if I was well prepared for the approaching exam on the two-year physics course. We were at Ostia, and Fermi was sitting cross-legged on the sand, in his bathing-suit, which came up almost to his neck. As he quizzed me, his usual grin faded and his lips tightened. In the end he said: "I am sorry, Miss Capon, but you don't understand a thing." What an encouragement!

The months went by and we became engaged. Fermi began to weigh on the kitchen scales the silver objects that we were getting as wedding presents. I knew that Fermi was a man of measurement, but as I handed him the objects to be weighed, I felt guilty. In my bourgeois world, the value of a gift was measured only by the giver's intentions. We were married on July 19, 1928, and we went to the Alps for our honeymoon. On a rainy day Fermi said: "I am going to teach you all the physics there is in just two years, and we'll start right now." Physics on my honeymoon did not appeal to me. But what could I do? I reminded him that I didn't understand physics, as he had told me. He replied, "There are no poor students, there are only poor teachers." But I was the exception. After days of conscientious application, we got into an argument about the validity of the Maxwell equations. What has mathematical equality to do with the equality of real phenomena? I wanted to know. And there my training ended.

But physics doesn't always come easy even to great physicists. The next winter was the coldest on record in Rome and we began talking of storm windows. Fermi pulled out his slide rule, calculated the effects of drafts on the inside temperatures, misplaced the decimal point, and we froze all winter. Fermi, however, never made the same mistake twice: he soon taught me always to estimate the order of magnitude of a result before undertaking to calculate it.

Many years later, when we were already in America, he was watching me prepare supper, one evening, with the aid of a pressure cooker. When we had bought it, giving in to his love of gadgets, he had explained to me the instructions in physical terms. Now I was holding the weight in my hand, waiting for the steam to begin to escape. Fermi asked, "Why don't you put down the weight right away?" "You told me once, I replied, that if I put the weight down while there is still air in the cooker I would be cooking at the partial pressure." Fermi said promptly, "If I said so, it must be true."

I am jumping ahead. We spent 10 years in Rome before going to America. Physics became more pleasant after Fermi and his group undertook to produce radioactive isotopes by neutron bombardment, in early 1934. Up to then Fermi had been mainly a theoretical physicist and had already completed two important papers: his statistics of the monatomic gas and the theory of beta rays.

The theory of beta rays had just caused him some bitterness. He had sent a letter announcing it to the journal *Nature*, but the editor had refused to publish it, saying, in effect, that it was crazy. Fermi was ready to give up theory for a while and take up experiments. Artificial radioactivity had just been discovered by the Joliot-Curies in a few light elements that they had bombarded with alpha particles.

Fermi thought that neutrons might be more effective than alphas and decided to try. Geiger counters were not standard laboratory equipment and he had to build his own. He and his friends prepared also the radon-beryllium sources, not an easy operation. I saw the physicists extract the radon from one gram of radium, and try to seal it inside small tubes containing the beryllium. But sometimes on the flame a tube went "pop" and broke. Segré's task was procurement: with a shopping bag and a shopping list he made the rounds of all chemist's shops in Rome and even borrowed gold from a jeweler friend.

The group began bombarding elements systematically, in the order of the periodic table. The lightest elements did not react, but from fluorine up most elements became radioactive. I began picking up bits of atomic physics listening to the men talk shop on Sunday hikes. Radioactivity, disintegration, half-life, the head of the manganese and the tail of some other element. . . .Things began falling into place. In fact, physics was comprehensible, as long as atoms were small planetary systems and discoveries could be made in goldfish ponds. . . .like the discovery of slow neutrons.

It was the fall of the same year, 1934. Two little physicists, Amaldi and Pontecorvo, had noted some inconsistency in the results of their neutron bombardment. Fermi looked into the matter. One morning he surrounded the neutron source with paraffin, and immediately the activity in the target increased greatly. Over lunchtime Fermi worked out an explanation: in going through paraffin the neutrons collided with hydrogen atoms, were slowed down, and then were more easily captured by the target atoms.

Back in the laboratory after their siesta, the group decided to test Fermi's theory using the most abundant hydrogenated substance at hand; and so they plunged neutron source and target in the goldfish pond at the back of the old physics building. Lo and behold! Fermi was right. Water too increased the radioactivity in the target by many times.

Atomic physics was so pleasant at that time that Ginestra Amaldi and I wrote a book about it. I had been pestering Fermi for suggestions of something intellectual that I might do. I had some time on my hands because a nursemaid was taking care of our little girl, Nella, and our son, Giulio, had not yet been born. At last Fermi suggested, "Why don't you write a book?" I was taken aback. "A book? What about?" The answer was one word: "Physics." What else is on a physicist's mind? As I protested that I didn't know enough physics, Fermi suggested that I write with Ginestra Amaldi. And so we did. I wrote the classical part, and when electrons began to jump out of their orbits and to claim they were both matter and wave, Ginestra took over. *Alchimia del tempo nostro* came out in 1936, a bit early for definitive atomic physics, with an introduction signed by Corbino. We sold 2,000 copies, a smash at that time for a book of popular science.

Meanwhile History began to interfere with everyday life. It had interfered in the early times of fascism; then things seemed to quiet down until they took a turn for the worse in the mid-thirties. There was the Ethiopian war and the consequent economic sanctions against Italy, which exasperated Mussolini. Up to then he had been on France's and England's side, trying to check Nazi expansionism. But now he threw himself into Hitler's outstretched arms, let him have his ways, and didn't protest even when he annexed Austria. In the summer of 1938, to emulate his friend, Mussolini promulgated anti-Semitic laws.

We decided to move to the United States, not only because I come from a Jewish family, "but also because we felt that Italy was *de facto* under German rule. Fermi wrote four letters to *four* American universities, in veiled terms, fearing censorship. But the Americans are smart. They took the hint, and Fermi got *five* invitations. He accepted the offer of Columbia University in New York.

We left Rome in early December 1938, with our two children, two and seven years old, and a nursemaid. We stopped in Stockholm to let Fermi pick up his Nobel Prize. One day, while strolling in the streets of Stockholm we ran into a mousy little woman with a tense expression. She was Lise Meitner, then a refugee from German persecution. Until the previous July she had been in Berlin where with Hahn and Strassman she had tried to solve the puzzle of element 93. Perhaps I should explain. In 1934, when Fermi and his group were bombarding uranium with neutrons, they detected an activity which they could not attribute to any elements near uranium in the periodic table. On theoretical considerations they thought that they might have created a new element, 93. They couldn't be certain, because in those early days of the art there were no established techniques to separate the extremely small amounts of substances that were produced. Only time would tell.

But Senator Corbino chose to announce the discovery at the royal session of the Accademia dei Lincei, in the presence of the King and Queen of Italy. The announcement created a great commotion inside and outside Italy. The scientific community was by and large skeptical. Fermi was terribly upset, more than I ever saw him before or after. Yet he and his group did not pursue the matter, feeling insufficiently skilled; and soon they were engrossed in the study of slow neutrons.

Research on the puzzling product of uranium disintegration was picked up by Hahn and Meitner, and later Strassmann In the following years they alternately confirmed and denied the existence of element 93. The puzzle had not been solved by the time Meitner left Germany in July 1938.

Fermi and Meitner did not talk about physics that day in the street. To me the significance of that encounter lay in Meitner's tense, almost scared look, a look that I was to see time and time again on the face of other refugees.

On December 24 we boarded the *Franconia* at Southampton and landed in New York on January 2, 1939, not without having had on the way a lesson in Anglo-Saxon habits. In the boat's elevator we had run into our first Santa Claus—we didn't know who he was; and I became uncomfortably acquainted with New Year's Eve celebrations when a tall female member of the D'Oyle Carte Company, the famous performers of Gilbert and Sullivan operas, bent almost in two to hug and kiss a passive Fermi.

My adjustment to American life was slow, but Fermi had only a couple of weeks of not knowing what to do with himself, scientifically. At Columbia he had no laboratory and he was preparing to become again a full-time theoretical physicist when news of the discovery of fission broke out. It came to America with Bohr, who had it, as he was leaving Copenhagen, from Lise Meitner and her nephew Otto Frisch. Meitner had received a letter from Hahn informing her that he and Strassmann had identified barium among the products of uranium disintegration. This meant that the uranium atom split in two almost equal parts. Something that had never been known to happen before!

Bohr arrived in New York on January 16, 1939, exactly two weeks after we did; Fermi and I went to meet him at the pier. He did not mention fission then, and from the pier he went to Princeton to be with Albert Einstein. But in a few days he was at Columbia University looking for Fermi, and so full of the news of fission that when he didn't find Fermi in his office, he unburdened his soul with a graduate student.

As a result the student, Herbert Anderson, invited Fermi to work with him and use the cyclotron for which he had built some equipment. Fermi accepted, glad to be in a laboratory again and to resume the kind of work he had done in Rome. By January 26, only 10 days after Bohr's arrival, an experiment to verify fission had been completed, and Bohr and Fermi were at the Annual Conference of Theoretical Physics in Washington, where they dropped a bombshell by revealing fission and its implications. Fermi advanced the hypothesis that neutrons might be released in the process, and so the idea of a chain reaction was launched. Soon Fermi and the Hungarian Leo Szilard were exploring the possibility of building an atomic pile.

Fermi explained fission to me, and its possible role as the "key that might unlock the great stores of energy in the atoms," as people used to say. But soon secrecy fell on the work of atomic scientists. It was self-imposed, having been suggested by Szilard, a man of numberless ideas. With the advent of secrecy I went on a long vacation from physics that was to last until the end of the Second World War. Then Fermi brought home a mimeographed copy of the so-called "Smyth Report," the scientific history of the development of atomic weapons. And slowly, with great difficulty, I caught up with six years' worth of memorable events.

So if I mention something that has to do with atomic physics, you must assume that I pieced it together later on, from the Smyth report and the stories of many friends.

I didn't miss physics at first, busy as I was keeping house and becoming Americanized. Fermi had made a deal with Anderson and in exchange for teaching him physics he got lessons in Americana. I learned more slowly, absorbing the democratic ways from our children. We had bought a house in Leonia, New Jersey, a friendly suburb of New York that seemed ideal for raising children. But Nella always asked for "more freedom," implying that I was infringing on her rights when I told her to come home after school before going to play with her friends. And four-year-old Giulio once declared, "You can't make me wash my hands. This is a free country." My "charming Italian accent," as kindly Americans called it, was a source of steady humiliations. The worst came when I ventured to order groceries on the telephone—I asked for butter and got bird-seed. Life was not always easy.

In early December 1941, two important events took place: the American government decided to push as much as possible the effort to develop atomic energy; and a couple of days later the United States entered the Second World War. I'd like to take a couple of minutes to tell you how the American government became involved in atomic energy.

In the United States of those days there were no links between government and the universities, such as the ministry of education in other countries; and virtually no channels of communications were available. So the scientists took the initiative. An early attempt by Fermi to alert the Navy produced little results—although it now seems probable that the first idea of a nuclear submarine dates back to that meeting between Fermi and Navy officers. A little later, two Hungarians, Eugene Wigner and Leo Szilard, made a more successful attempt. In the typical devious Hungarian way, they agreed with Einstein, the tallest figure in science, that they would write a letter to President Roosevelt, and he, Einstein, would sign it. By the time the letter was ready, Einstein was vacationing in some place that could be reached only by car. So Wigner and Szilard engaged a third Hungarian physicist, Edward Teller, as a chauffeur.

The letter dated August 2, 1939, started: "Some recent work by E. Fermi and L. Szilard. . .leads me to expect that the element uranium may be turned into a new and important source of energy in the immediate future. . . ." According to Teller, who told me the story, after reading and signing the letter Einstein exclaimed, "For the first time in history, men will use energy that does not come from the sun!" Not trusting the mails, Szilard gave the letter to economist Alexander Sachs, who occasionally talked to the president. Some weeks went by, war broke out in Europe, and finally President Roosevelt received the message. He set up a committee on uranium, but until December 1941, work remained limited to a couple of universities, and appropriations were exceedingly small. With the decision to push the effort, the uranium project suddenly expanded immensely, and the work on the atomic pile was moved to Chicago.

Fermi began traveling between New York and Chicago, to wind up his experiments at Columbia and start things going in Chicago. But meanwhile we had become enemy aliens, because Italy and the United States were at war. So Fermi was required to obtain a special permit each time he had to travel: his work was so secret that not even the immigration authorities could be told about it. The permit had to come from Trenton, the capital of New Jersey, where we lived.

We had thought we were settled for good in Leonia, but in the summer of 1942 we all moved to Chicago. We expected to be there for the duration of the war; instead, after two years we moved on to Los Alamos, New Mexico; and at each move we came in contact with more European scientists and stricter security measures. Besides Fermi and Szilard, the foreign scientists included James Franck, the Nobel Prize–winning chemist; Eugene Wigner; and Edward Teller.

As for security measures, at Columbia University a couple of rooms were closed to the noninitiated, but I doubt that many people know about them. In Chicago we had our first experience with fake names: the project was called Metallurgical Laboratory, or Met Lab for short, although there wasn't a single metallurgist in it (the only secret about it that I was told). And soon the Met Lab became a part of the protean Manhattan Project, also a code name. Fermi became Eugene Farmer when he was traveling, and like other key scientists he acquired a bodyguard, a big man of Italian descent, John Baudino, who looked much more impressive than Fermi himself. He knew how to bang a fist on a table, which Fermi could not do. We, the wives, were refused access to the physics building by armed guards, and in addition we had to listen to long lectures on the dangers of loose talk.

The way husbands interpreted secrecy varied. At one extreme was Arthur Compton, the director of the Met Lab: in a book that he published in 1958, he revealed that he had obtained clearance for his wife so that he could tell her the secrets—when other wives and I learned this, we were terribly incensed. At the other extreme was Fermi who was completely tight-lipped. An episode will illustrate his attitude: on the evening of December 2, 1942, we had a party for Met Lab people at our house. As they walked in, all men congratulated Fermi, but nobody would tell me why. At last a friend whispered in my ear, "He has sunk a Japanese admiral with his ship."

I felt as I had many years before about the crocodiles: I couldn't quite believe. . .but . . .perhaps he had invented death rays . . .The friend insisted, "Do you think that anything is impossible to Fermi?"

So the next morning I asked Fermi, "Did you really sink a Japanese admiral?" But Fermi put on his best poker face: "Did I?" he asked. "So you didn't," I said. "Didn't I?" he replied. There was really no point in trying to extract information from Fermi.

Of course, that day Fermi had led the experiment that achieved the first chain reaction in the first atomic pile. I am told that he gave a great demonstration of showmanship. He was very much in command of the situation, standing on a platform by the pile, selfassured but watchful; moving his gray eyes from the indicators of neutron activity to his slide rule; predicting how the pile would behave at the next step; ordering physicist George Weil on the floor below to pull out the control rod a little more, and then again a little more, and so step by step until the pile chain-reacted. By all accounts Fermi directed the experiment with the precision of a well-rehearsed show. But the show had not been rehearsed: construction of the pile had been completed only a few hours earlier, when the night shift under Herbert Anderson laid the last layer and locked all controls in place. At the end of the experiment the scientists made a silent toast with a bottle of Chianti which Eugene Wigner had bought months earlier, with great foresight, before war conditions made Chianti disappear from the Chicago market. The empty bottle, with the signatures of all who were at the experiment, is still making the rounds of museums and exhibits, and you may run into it at some time.

Los Alamos was the climax of my career as a physicist's wife. It was an experience entirely different from any I had before or after, made of elements each with its own striking individuality. There was the contact with the immensely vast wilderness of mesas, canyons, and desert, that has no comparison in Europe. There was the isolation of the town that did not exist for the rest of the world, was not on the map, was not even a part of New Mexico, so that its inhabitants could not even vote. Its code name was Site Y, its address, Post Office Box 1663, Santa Fe (Santa Fe was the nearest city, some 70 kilometers away). The town, on top of a mesa, encircled by a barbed-wire fence, was run by the army—whatever the army provided us with, for instance the blankets for our beds, was stamped USED for United States Engineering Detachment.

We were fingerprinted, given passes and badges, and assigned apartments in barrack-style buildings—the size of the apartment depended on the number of children we had, the rent on the salary of the man. Our mail was censored, as Segré discovered long before the army officially announced censorship. Once when he was away from the project on a trip he placed a strand of hair in a letter to his wife, but when she opened the letter, the strand of hair was gone. There were so many foreign-born. (General Groves, the chief of the Manhattan Project, called them "crackpots") that "it was all a big accent." Emilio Segré, Bruno Rossi, Edward Teller, Hans Bethe, and Rudi Peierls were among our old friends. Among the new friends we made were Vicky Weisskopf, Hans Staub, mathematician Stan Ulam, and Johnny Von Neumann, who came to Site Y on visit. Also on visit came Uncle Nick—he was Niels Bohr, but his true name was one of the most closely guarded secrets.

The strangeness of Site Y was magnified by the fact that for the first three weeks I was there with my children but without Fermi, who just before we left Chicago had been called to Hanford in the state of Washington. There, three large piles were being built to produce the plutonium needed for atomic bombs. When construction began, only the Chicago pile had ever operated; that was a very small and simple pile, with no shielding and very simple controls. The Hanford piles, as Fermi would have said, were different animals. So Fermi had been asked to be at hand when the first production pile would start operating, just in case something should go wrong. And something did go wrong—and remained secret for the next 14 years: the pile began to chain-react, as it was expected, but soon it shut itself down. It seemed as if the entire Hanford plant might be a failure. So Fermi had been detained to try and solve the problem. With Wigner he found that the pile was being "poisoned" by a product of fission, a Xenon gas, which absorbed large quantities of neutrons.

While Fermi was at Hanford, I had occasion to meet my first and only spy, and so add to my collection of memorable persons. I forgot to tell you about this "collection." There are in it two kings with whom I dined, King Albert of Belgium at a Solvay meeting in Brussels; and King Gustav of Sweden, at the Nobel ceremonies; there is Madame Curie whom I hardly met because she would not take notice of insignificant wives like me; Rutherford, and Einstein, and other sacred objects of the distant past. In Los Alamos I added Klaus Fuchs the spy. The first Sunday I was there a group of friends organized a picnic in a canyon. Our car was needed, but I wouldn't drive in that unknown, wild territory. So the Peierls asked Fuchs, their friend and protégé, to drive my car. He was an attractive young man, German-born, with a quiet look through round eyeglasses, who answered sparingly to my questions. Even as he spoke to me he was leading a double life, that of a competent physicist appreciated by his colleagues, and that of spy. As he was to confess in 1950, he was giving secret information to the Russians on the progress of the atomic bomb. Fermi was to say Fuchs had made it possible for the Russians to make an atomic bomb five to ten years earlier than they would have otherwise.

There is no time to describe the peculiar features of life in Los Alamos. As for the secret work that went on inside the Technical Area, it led to two events of great momentum: the first atomic explosion at Trinity, the code name for Alamogordo in southern New Mexico; and the dropping of the atomic bomb on Hiroshima and Nagasaki.

In the first part of July 1945, men began to disappear from the mesa to go to Trinity, without telling us why. But soon we learned, somehow, that at Trinity there was going to be a test of some kind.

On the morning of July 16 the news spread that a sleepless patient at the Los Alamos hospital had seen a strange light in the early morning. We thought that the test must have been successful.

Late the same evening Fermi and a few other men came back. He looked dried out and shrunken, baked by the heat of the southern desert. He was dead tired—for the first time in his life he had felt unsafe for him to drive his car and had let Sam Allison take the wheel. That was all he had to say. But only a few weeks later, when I could ask questions and get answers, I learned from Fermi that he had seen the dazzling light of the explosion (like one thousand suns), but had not heard the sound, a sound that was described as "a strong sustained, awesome roar." His attention had been concentrated on little pieces of paper that he let fall from his hands. The air blast of the explosion dragged them along and they fell at some distance. Fermi, who always liked simple experiments, measured that distance counting his steps, and so he calculated the power of the explosion. His figures were remarkably close to those obtained with precision instruments and complex calculations. Then he explored the site of the explosion: a depressed area 400 yards in radius was glazed with a green, glasslike substance, the sand that had melted and solidified again.

Three weeks after the test at Trinity the bomb was dropped over Hiroshima. In Los Alamos, President Truman's announcement of the bomb was transmitted over the paging system of the Technical Area. There were no telephones in our homes, and the news spread by way of mouth, as soon as the first husband went home to tell his wife. I was informed by Gennia Peierls, who lived in the apartment below ours, and rushed upstairs shouting in her thick Russian accent, "Our stuff was dropped over Japan!" She said "our stuff" because not even by the morning after Hiroshima did we fully realize that Los Alamos was making atomic bombs.

Hiroshima and the end of secrecy set off an explosion of words and feelings in Los Alamos. Children became suddenly very proud of their fathers. Wives asked all the questions that had found no answer since the beginning of the uranium project. And the men appeared altogether changed. I had never heard them mention the atomic bomb, and now they talked of nothing else. They had been absorbed in their research in the protective isolation of the Technical Area, and at least at home they had shown no signs of emotion. Now they were troubled and bewildered, and their concern extended to the whole world. They talked of international control of atomic energy, they posed moral questions.

Perhaps they were not emotionally prepared for the absence of that time interval which usually separates scientific discoveries from their applications. Anyhow, driven to action, the men called meetings, exchanged views, formed associations, and made plans to explain atomic energy and its implications to the public. They felt that if everybody understood the issues, atomic power would not be used again in a war—it would indeed become a deterrent and prevent wars.

Were they right? It is 30 years since the first explosion at Trinity. In about four weeks it will be 30 years since the end of the Second World War—between the first and second World War there were only 21 years.

This is a good point to put an end to my rambling recollections. By comparison with Los Alamos the eight years in Chicago after the war appear grayish, despite the construction of the Chicago synchrocyclotron and other events.

But it is usual to draw a conclusion to a talk. Here is mine. Some physicists' wives believe that their husbands like physics better than they do their wives. And they may have a point. After work in the evening, when a wife is expecting a word of endearment, like "I couldn't live without you," the husband is most likely utterly silent, absorbed in scribbling numbers and symbols on the margins of the evening paper. When she would like to go to the movies, he has a date with an experiment that cannot wait. There are other complaints, some more justified than others. But all in all, life with a physicist is well worth living.

Chapter 31
Enrico Fermi

Chen Ning Yang
The Chinese University of Hong Kong
and
Tsinghua University, Beijing

Figure 32.
Frank Yang

Contribution to the Centennial Celebration in 2001 of the One-Hundredth Birthday of Enrico Fermi on September 29, 1901

(This paper was sent to the Rome conference in September 2001. Yang was not present and it was not part of the program. Part of it is presented here. See end of Chapter 18 for other comments by Yang.—J. O.)

Enrico Fermi was, of all the great physicists of the twentieth century, among the most respected and admired. He was respected and admired because of his contributions to both theoretical and experimental physics, because of his leadership in discovering for mankind a powerful new source of energy, and above all, because of his personal character: He was always reliable and trustworthy. He had both of his feet on the ground all the time. He had great strength but never threw his weight around. He did not play to the gallery. He did not practice one-upmanship. He exemplified, I always believe, the perfect Confucian gentleman.

Fermi's earliest interests in physics seem to be in general relativity. Starting from around 1923 he began to think deeply about the "Gibbs paradox" and the "absolute entropy constant" in statistical mechanics. This research led to his first monumental work and to the "Fermi distribution," "Fermi sphere," "Fermi liquid," "Fermi statistics," "Fermions," etc.

It was characteristic of Fermi's style in research that he should follow this abstract contribution with an application to the heavy atom, leading to what is now known as the Thomas-Fermi method. The differential equation involved in this method was solved by Fermi numerically with a small and primitive hand calculator. This numerical work took him probably a week. E. Majorana, who was a lightning-fast calculator and a very skeptical man, decided to check the numerical work. He did this by transforming the equation into a Riccato equation and solving the latter numerically. The result agreed exactly with the one obtained by Fermi. Fermi's love of the use of computers. small and large, which we graduate students at Chicago observed and admired, began evidently early in his career and lasted throughout his entire life.

Fermi's next major contribution was in quantum electrodynamics, where he succeeded in eliminating the longitudinal field to arrive at the Coulomb interaction. Fermi was very proud of this work as his students at the University of Chicago in the years 1946 to 1951 knew. (But it seems today that few theorists under the age of 65 know about this contribution of Fermi's.) It again was characteristic of Fermi's style that in this work he saw through complicated formalisms to arrive at the basics, in this case a collection of harmonic oscillators, and to proceed to solve a simple Schrodinger-like equation. The work was first presented in April 1929 in Paris and later at the famous Summer School at Ann Arbor in the summer of 1930. G. Uhlenbeck told me in the late 1950s that before this work of Fermi nobody really understood the quantum theory of radiation and that this work had established Fermi as among the few top field theorists in the world.

I shall skip describing his beautiful contribution in 1930 to the theory of hyperfine structure, and come to the theory of beta-decay. According to Segré, Fermi had considered, throughout his life, that this theory was his most important contribution to theoretical physics. I had read Segré's remarks in this regard, but was puzzled. One day in the 1970s, I had the following conversation with Eugene Wigner in the cafeteria of Rockefeller University:

Yang: What do you think was Fermi's most important contribution to theoretical physics?

Wigner: beta-decay theory.

Yang: How could that be? It is being replaced by more fundamental ideas. Of course it was a very important contribution which had sustained the whole field for some 40 years: Fermi had characteristically swept what was unknowable at that time under the rug and focused on what can be calculated. It was beautiful and agreed with experiment. But it was not permanent. In contrast, the Fermi distribution is permanent.

Wigner: No, no, you do not understand the impact it produced at the time. Von Neumann and I had been thinking about beta-decay for a long time as did everybody else. We simply did not know how to create an electron in a nucleus.

Yang: Fermi knew how to do that by using a second quantized psi?

Wigner: Yes.

Yang: But it was you and Jordan who had first invented the second quantized psi.

Wigner: Yes, yes. But we never dreamed that it could be used in real physics.

I shall not go into Fermi's later contributions nor into his relations with students. I shall only add a couple of stories about Fermi.

One of Fermi's assistants at Los Alamos during the war was Joan Hinton, who became a graduate student at the University of Chicago after the war. When I began working in late 1946 for Sam Allison, she was a fellow graduate student in the same laboratory. In the spring of 1948 she went to China and married her boyfriend, Sid Engst, and settled down in China permanently to do agricultural work. (Hers was a very interesting story that should be written down. I hope she will do it soon.) In the summer of 1971 during my first visit to the New China, half a year before Nixon, I accidentally met her in a hostel in Da-zhai, then a model agricultural commune in the County of Xi-Yang. Surprised and delighted, we reminisced about the Chicago days: how I was awkward in the laboratory, how I almost accidentally electrocuted her, how I had taught her a few sentences of Chinese, how I had borrowed a car and had driven her to the La Salle station to embark on her long trip to China, etc., etc. She asked me whether I remembered the farewell party that the Fermi's had given her before she left. I did. Did I remember the camera that they had given her that evening? No, I did not. After a pause, she said she had felt, a few days before that farewell party, that she should tell Fermi about her plan to go to the Communist-controlled area of China. So she did. And what did Fermi say? He did not object. Joan said, "For that I am eternally grateful" (If she had tried to leave later she might have been denied permission.) I (Yang) considered this such an important statement that after coming back to Stony Brook, I called Mrs. Fermi in Chicago and reported to her my whole encounter with Joan in Da-zhai. A few years later, Joan visited Chicago herself and had the opportunity to visit with Mrs. Fermi and her daughter, Nella Fermi. *(Joan made a later visit to Chicago at the time of the second Fermi student reunion. She gave a one-hour talk about her experiences in China and is shown in Figure 2 just after giving the talk.—J. O.)*

In 1983 Yang had also written the following: "Fermi was deeply respected by all as a physicist and as a person. The quality about him that commands respect is, I believe, solidity. There was nothing about him that did not radiate this fundamental strength of character. One day in the early 1950s, J. R. Oppenheimer, who was the Chairman of the important General Advisory Committee (GAC) of the Atomic Energy Commission (AEC) told me that he had tried to persuade Fermi to stay on the GAC when Fermi's term was up. Fermi was reluctant. He pressed, and finally Fermi said, 'You know, I don't always trust my opinions about these political matters.'"

Chapter 32
Fermi Centennial Comments

Leon Lederman
Former Director, Fermilab

Figure 33.
Leon Lederman and Bob Wilson singing their song.

I send this note instead of attending this meeting because of the disruptions to all of our lives by the tragic events of September 11. I had been looking forward to coming to Rome, celebrating Enrico Fermi's centennial and seeing many old friends. The scientific community, which so reveres Fermi's contributions, both in science but also in style, must now maintain our faith in rationality, which is threatened on all sides. It is my personal belief that we must understand and act wisely on the root causes of terrorism. But now let me make a few remarks relevant to the centennial. My assignment, for the Fermi centennial, was to discuss the early period of pion and muon physics. I was among the first post-WW II graduate students to get a Ph.D. at Columbia University's NEVIS Cyclotron Laboratory. The date was 1951. My thesis was on the lifetime of the pion and the mass of the muon. My adviser was visiting professor Gilberto Bernardini. Through Gilberto, I met Fermi several times. Our involvement with pions was essentially simultaneous; Fermi's Chicago period included a new collection of awesome students that Fermi seemed to attract. It was quintessential Fermi, with an almost seamless mix of theory and experiment. I recall being delighted that the great Fermi was working on the same things as I. NEVIS came online a few years before Chicago. John Tinlot and I had discovered how to get beams of pions out of the accelerator, focused by the fringing field of the cyclotron magnet. We had "hot and cold" pion beams! Our Berkeley competitors were not so lucky. Our negative beams went out to ~150 MeV, but positive pions (obtained from backwards emission in proton-target collisions) died at about 60 MeV.

We worked on lifetimes of pions on scattering of pions from a carbon plate in a Wilson Chamber, on mass of muons, and the properties of the neutrino. Fermi's group concentrated on pion-proton scattering. I still recall the excitement of Fermi's "Rochester Conference" presentation of his negative pion scattering. The cross section was large, definitively establishing the strong interaction of pions after some disturbing cosmic ray results.

When Fermi's group turned to positive pions, the results were even more spectacular. The cross section rose dramatically. When it was last seen, it was at about 135 MeV, heading steeply upward. The suspicion was a resonance but it took several years to establish the "3-3" resonance, although Fermi, on the basis of a glance at a paper written by Keith Bruekner, predicted the famous ratio of the three pion-proton cross sections (pi plus to pi minus; pi zero to pi minus; pi minus to pi minus) as 9:2:1.

Fermi led a reduction of the data via a phase shift analysis. Again it was only after Fermi's death that the correct *phase shifts* were established and the 3-3 resonance firmly established. I will never forget the first Rochester Conference around 1950. I was the only graduate student present and found myself standing next to Enrico on the lunch line. Desperate to show my deep knowledge, I asked him, "Professor, what do you think of the evidence for the V-zero-two which we just heard?" He looked at me and gave a response that became famous: "Young man, if I could remember the names of these particles, I would have been a botanist." *(Actually, by this time Fermi felt that all these new mesons were different decay modes of the same particle. However, he did not live to see that two of these "decay modes" were of different parity.—J. O.)*

We were in a new field that emerged from the fields of cosmic rays and nuclear physics. The beginnings in the accelerators of Chicago, Berkeley, and Columbia are the clear progenitors of a field that has led to our current understanding of the Standard Model of Fundamental Particles and its essential coupling to the astrophysics of the origins and evolution of the universe.

It is clear that Enrico Fermi's personal leadership, his scientific style, and his influence on students was a major force in the establishment of physics in the United States. My personal contact with Fermi in visits to Chicago, in Rochester conferences, in his early summer visit to Brookhaven just months before his illness, was a seminal experience. I was later honored to become director of the Fermi National Accelerator Lab (Fermilab) and to receive the Enrico Fermi Medal at the hands of President Bill Clinton in 1993. I now send my warmest greetings to the Centennial assembly convinced that the pursuit of our efforts to understand the world and to insist that this knowledge be applied compassionately, is the highest form of tribute to the memory of Enrico Fermi.

Index

A-bomb, 54
Agnew, 3, 4, 10, 11, 52, 54, 67, 101, 102, 103, 104, 105, 122, 129, 137
agnostic, 57, 62
Albert, 82, 83, 149, 151
Allison, 6, 91, 152, 157
alpha particle, 80, 147
alpha particles, 70, 79, 80, 81, 147
alternate gradient principle, 47
Alvarez, 21, 118
Amaldi, 70, 77, 78, 94, 126, 134, 146, 147
American Physical Society, 7, 20, 27, 37, 38, 82, 99
Anderson, 27, 45, 83, 90, 92, 93, 94, 101, 127, 141, 149, 151
Ansatz, 78
antineutrino, 31
antiproton, 6, 31
anti-Semitism, 81
APS, 3, 37, 38
Argonne, 7, 38, 47, 83, 85, 86, 90, 91, 126
Atoms in the family, 32, 145
Austria, 81, 118, 148
Backus, 20
Baudino, 110, 150
Bayes Theorem, 20
Belgium, 50, 151
Berkeley, 31, 50, 70, 92, 102, 114, 139, 160
Berkeley Bevatron, 31
Bernardini, 160
Bethe, 3, 4, 5, 6, 27, 35, 50, 52, 54, 67, 69, 74, 75, 78, 80, 82, 83, 89, 98, 121, 137, 151
Bevatron, 31, 47
Bikini, 11, 29
birth of a new age, 84
Bohr, 64, 82, 83, 119, 148, 149, 151
bongo drum, 35
bosons, 31, 42, 46
Bothe, 79
Brazilians, 35
Breit interaction, 79
Brookhaven, 28, 47, 48, 160
Bruekner, 160
bubble chambers, 28
Butler, 50
Byers, 10
Cambridge, 7, 47, 80
cancer, 27, 45, 90, 135, 136
Carl Sagan, 4, 1, 3, 51, 67, 71, 97, 116, 136
Caton, 59
CERN, 4, 47, 89
Chadwick, 80, 119
chain reaction, 33, 70, 79, 81, 82, 83, 90, 127, 149, 150
Chamberlain, 6, 9, 31, 102, 103
Chandrasekhar, 6, 32, 58, 95, 97
Chew, 12, 97, 99, 103, 143
Chicago, 3, 4, 1, 3, 4, 5, 6, 9, 10, 11, 12, 13, 16, 17, 25, 26, 28, 31, 33, 35, 41, 42, 45, 54, 57, 58, 65, 67, 73, 77, 81, 84, 85, 87, 89, 90, 91, 93, 94, 95, 96, 97, 101, 102, 103, 104, 115, 119, 120, 121, 122, 125, 126, 127, 139, 150, 151, 153, 156, 157, 160
Chicago cyclotron, 45
Chimbidis, 4
Christopher Columbus, 72, 74
Chwolson, 48, 64
cloud chamber, 29, 141
Cobalt Bomb, 51
Collected Papers of E. Fermi, 45
Columbia, 3, 4, 1, 3, 21, 28, 38, 42, 45, 49, 50, 51, 67, 73, 81, 83, 86, 90, 92, 94, 101, 108, 114, 125, 126, 148, 149, 150, 160
Columbia University, 1, 7, 21, 28, 38, 45, 49, 51, 73, 83, 90, 108, 125, 148, 149, 150, 160
Compton, 7, 70, 72, 84, 150
Conant, 7, 72, 84
Corbino, 77, 126, 146, 147, 148
Cornell symposium, 1, 7, 16, 35, 45, 58, 67
Corson, 3, 4, 5, 67, 69, 78
cosmic ray particle, 23
Cosmotron, 28, 45, 47
Courant, 47
CP1, 84, 126
Creativity, 3, 1, 7, 63, 65
Cronin, 6, 10, 29, 97
Curie, 80, 151
cyclotron, 25, 45, 46, 92, 94, 99, 102, 111, 122, 136, 142, 149, 160
cyclotrons, 42, 111

d'Agostino, 80
Dalitz, 21, 97
Dancing, 11
Dec. 2, 1942, 84
declassification, 85
demonstration explosion, 54
DePino, 4
Dick Garwin, 4, 1, 3, 6, 9, 10, 12, 25, 28, 52, 54, 67, 73, 89, 91, 104, 121, 123, 137, 141
Dirac, 64, 72
Dresselhaus, 6
Edward R. Murrow, 72
Einstein, 64, 82, 98, 133, 149, 150, 152
electrodynamics, 5, 78, 79
elementary particles, 38, 72, 93
Embassy, 50
Emilio, 101
Emilio Segre, 1, 3, 6, 50, 59, 99, 102, 146, 151
Engst, 157
Enrico, 2, 3, 4, 1, 3, 4, 5, 6, 7, 11, 12, 20, 26, 27, 32, 47, 49, 51, 52, 57, 58, 59, 60, 61, 64, 66, 67, 72, 73, 74, 75, 77, 81, 84, 85, 86, 89, 90, 94, 96, 98, 99, 101, 102, 103, 104, 108, 110, 115, 118, 119, 121, 122, 123, 125, 129, 132, 133, 137, 139, 141, 142, 146, 155, 156, 159, 160
Enrico Fermi, 11, 32, 49, 57, 60, 64, 66, 72, 73, 84, 85, 86, 101, 115, 121, 123, 125, 146, 160
Erice, 5, 1, 7, 49, 51, 145
Fano, 50, 77
Farwell, 9, 10, 102
Fascist Italy, 83
Federation of American Scientists, 54
Fermi, 2, 3, 4, 5, 1, 3, 4, 5, 6, 7, 9, 10, 11, 12, 13, 16, 17, 19, 20, 21, 22, 24, 25, 26, 27, 28, 29, 31, 32, 33, 35, 37, 38, 39, 40, 41, 42, 43, 45, 46, 47, 48, 49, 50, 51, 52, 53, 54, 57, 58, 59, 60, 63, 64, 65, 67, 69, 70, 72, 73, 74, 75, 77, 78, 79, 80, 81, 82, 83, 84, 85, 86, 87, 89, 90, 91, 92, 93, 94, 95, 96, 97, 98, 99, 101, 102, 103, 104, 105, 107, 108, 109, 110, 113, 114, 115, 116, 117, 118, 120, 121, 122, 123, 125, 126, 127, 129, 132, 133, 137, 139, 141, 142, 143, 145, 146, 147, 148, 149, 150, 151, 152, 155, 156, 157, 159, 160
Fermi pile, 79
Fermi Remembered, 10
Fermi's most notable accomplishments, 63
Fermilab, 2, 3, 5, 24, 40, 72, 107, 125, 126, 159, 160
Fermi-Metropolis phase shifts, 45
Fermi-Metropolis solution, 45, 46
Feynman, 3, 24, 35, 41, 64, 69, 95
Fischer, 21
Fourier analysis, 78
Franck, 150
Friedman, 4, 6, 9, 10, 45, 97
Frisch, 80, 81, 148
Fundamental Physics, 20
Galileo, 54, 57, 63, 64, 96
Garwin, 3, 4, 5, 10, 12, 25, 33, 52, 65, 73, 91, 97, 99, 104, 121, 123
Gell-Mann, 6, 10, 12, 31, 97
General Groves, 151
Gentile, 77
Germany, 33, 54, 79, 81, 135, 148
Giulio, 28, 60, 131, 132, 137, 147, 149
Glicksman, 4, 5, 9, 10, 67, 94, 140, 141
Glickstein, 3
Goldberger, 10, 12, 25, 89, 97, 99, 103, 143
Golden Rule Number Two, 16
gun assembly, 54, 102
Haberscheim, 4, 9
Hahn, 80, 81, 83, 148
H-bomb, 51, 52, 54, 91, 104, 123
Heilbrun, 64
Heisenberg, 64, 77, 78, 79
Heitler, 50
Herb Anderson, 3, 27, 45, 91, 94, 101, 102, 137, 141, 143
Hildebrand, 9, 10
Hinton, 9, 10, 137, 157
His Excellency Fermi, 35, 78, 81
Hitler, 54, 58, 73, 132, 148
Holton, 7, 32, 57, 59
Hyde Park High School, 11
impurities, 83
insomnia, 26, 137
Institute of Nuclear Studies, 33
Intuition, 3, 1, 7, 31, 32, 39, 42, 45, 48, 63, 86, 122, 123
Iron Curtain, 22
Ishbar Fraser, 59, 60
isotopic spin, 16, 94
Italian fascism, 49
Italian Navigators, 33
Jewish, 49, 61, 81, 135, 148

jig-saw, 48
Joliot, 80, 147
kaon, 16, 21, 22, 31
King, 3, 4, 148, 151
King Gustav, 151
Klaus Fuchs, 152
Kolb, 4
Kuiper, 73
Lach, 5
Landau, 48, 97
Laura, 3, 4, 5, 1, 3, 7, 11, 29, 32, 51, 57, 59, 60, 61, 67, 72, 73, 78, 80, 81, 89, 94, 98, 99, 101, 102, 103, 104, 117, 118, 119, 120, 123, 137, 145
Laura Fermi, 1, 3, 32, 51, 59, 72, 73, 78
Lederman, 4, 5, 28, 67, 159
Lee, 3, 6, 12, 16, 31, 33, 34, 49, 63, 64, 65, 97, 99, 103
Leona Marshall, 3, 7, 26, 86, 92, 121, 137
Livingston, 47
Longmire, 52
Los Alamos, 2, 4, 5, 3, 4, 11, 35, 52, 53, 54, 67, 69, 70, 75, 90, 91, 92, 93, 94, 95, 96, 99, 101, 102, 104, 107, 109, 110, 112, 113, 114, 116, 119, 121, 123, 133, 150, 151, 152, 153, 157
Majorana, 77, 146, 156
Manhattan Project, 114, 150, 151
Martin, 4, 5, 94, 121
maximum likelihood method, 21
Maxwell, 63, 64, 146
Mayer, 6, 52, 95
McDaniel, 3, 75
McEvoy, 3, 4
Meitner, 80, 81, 148
Metropolis, 45, 46, 94
MIT, 4, 5, 21, 92, 121
Monte Carlo, 20
Morrison, 20, 69
Moszkowski, 10
Mueller, 79
muon, 6, 16, 92, 121, 160
Mussolini, 58, 77, 132, 148
My Life As A Physicist's Wife, 51
Nagle, 4, 10, 91, 94, 99
Nazi, 61, 81, 148
Nella, 3, 4, 11, 32, 45, 57, 58, 59, 60, 61, 67, 78, 102, 103, 104, 129, 132, 133, 134, 135, 136, 137, 147, 149, 157
neutrino, 6, 31, 46, 63, 125, 160
Neutron, 4, 67, 81, 82, 85, 86, 90
Nevis cyclotron, 45
Newton, 57, 58, 63, 64
Nino Zichichi, 1, 145
Nixon, 157
Nobel, 3, 5, 6, 12, 21, 24, 25, 31, 34, 48, 49, 65, 81, 82, 83, 89, 95, 103, 118, 148, 150, 151
Nodnick, 32
non-conservation of parity, 6, 21, 22, 33, 34, 42
Nuclear, 4, 16, 21, 26, 28, 33, 67, 83, 91, 92, 93, 95, 96, 117, 137, 143
nuclear chain reaction, 7, 9, 54, 79, 83, 89
nuclear emulsion, 23, 28, 29, 31, 45
nuclear fission, 32
nuclear magic numbers, 6
Nuclear Physics, 13
nuclear pile, 4, 67, 83
numerical, 48, 73, 78, 87, 94, 98, 99, 122, 156
odd parity, 22, 31, 46
Oppenheimer, 21, 51, 54, 58, 64, 72, 98, 107, 108, 112, 113, 114, 115, 134, 139, 157
Oppie, 58, 108, 109, 110, 111
Orear, 1, 3, 4, 9, 13, 19, 20, 21, 28, 45, 52, 59, 70, 72, 73, 87, 93, 99, 125, 126, 137, 139, 143, 145
Panofsky, 20
paraffin, 31, 32, 81, 122, 147
patents, 64, 83, 125, 126, 127
Pauli Notes, 31
Pegram, 3, 42, 49, 50, 51
Peierls, 118, 119, 151, 152
penetration barrier, 33
Peoples, 4, 3, 4, 67, 125
Persico, 57, 59, 77, 146
Physical Review, 16, 26, 47, 48, 85
Physicist's Wife, 4, 7, 67, 145
Physics Today, 38
Pincherle, 50
pion, 6, 16, 20, 21, 22, 23, 26, 27, 28, 31, 42, 46, 64, 92, 93, 94, 141, 142, 160
Placzek, 78, 79
Plenum Press, 145
Politics, 49
Pontecorvo, 32, 77, 126, 147
President Bill Clinton, 160
President Roosevelt, 149, 150
Princeton, 21, 47, 149

proton, 20, 21, 24, 26, 27, 28, 29, 31, 39, 40, 41, 45, 46, 47, 64, 80, 93, 94, 95, 160
prototype, 48
Pupin Laboratories, 42
quantum electrodynamics, 16, 17, 63, 78, 79, 156
quantum mechanics, 11, 16, 64, 82, 85, 86, 96, 97, 119, 143
quark, 6, 31, 46, 93
Rabi, 5, 51, 52, 54, 67, 72, 92, 117
Racah, 50, 77
Rachel Fermi, 1, 59, 60
racial, 50, 51
racism, 49
Radiation Lab, 21
radium, 80, 147
radon, 80, 147
Rasetti, 50, 77, 78, 80, 126, 146
Religion, 3, 57
resonance, 28, 45, 48, 82, 94, 141, 142, 160
Ritson, 21
Rojansky, 16
Rome, 1, 3, 4, 7, 10, 32, 59, 67, 70, 74, 75, 77, 80, 81, 89, 92, 97, 102, 105, 119, 126, 129, 132, 135, 146, 147, 148, 149, 156, 159
Rome Congress, 1, 7, 32, 102
Rosenbluth, 12, 52, 103
Rosenfeld, 3, 4, 3, 4, 5, 9, 10, 13, 16, 17, 20, 21, 26, 28, 45, 51, 53, 67, 87, 93, 137, 139, 143
Rossi, 50, 118, 151
Royal Academy, 35, 77
Rutherford, 64, 80, 83, 97, 151
Sachs, 150
Sagan, 3, 4, 51, 71, 82
Schein, 29
Schluter, 3, 4, 9, 10, 13, 16, 17, 20, 21, 87, 93, 99, 143
Schrodinger, 64, 97, 156
security clearance, 51, 58
See it now, 7
Segre, 1, 31, 32, 38, 50, 51, 57, 59, 61, 63, 77, 78, 79, 81, 82, 89, 99, 102, 118, 126, 147, 151, 156
self-sustaining, 72, 73, 83, 90, 91
Shoesmith, 65
skating, 12, 35, 102, 137
Slater, 10, 28, 45
slow neutrons, 31, 147, 148
Snyder, 47
Solmitz, 20, 21
Somerfeld, 78
Sommerfeld, 5, 77, 97
SSC, 21
Stadler, 27
Stagg Field, 72, 90, 126
Stalin, 54
statistics, 12, 20, 21, 22, 24, 27, 63, 66, 72, 77, 89, 93, 95, 147, 156
Staub, 151
Steinberger, 6, 7, 9, 10, 12, 20, 97, 99, 103, 121
strange particles, 29, 41, 46
Strassman, 80, 81, 148
strong focusing, 47
Sua Excellencia Fermi, 77
Swanson, 28, 45
symmetry, 24, 25, 94, 141
Szilard, 7, 42, 54, 83, 91, 115, 126, 149, 150
Taft, 28, 45
Talman, 48
Tau Meson, 20, 21, 22, 23
tau-theta puzzle, 22
Telegdi, 4, 3, 4, 5, 9, 16, 17, 45, 64, 67, 89, 91, 94, 97, 99, 141
Teller, 47, 51, 52, 53, 58, 72, 91, 92, 93, 97, 104, 118, 122, 123, 134, 139, 143, 149, 150, 151
The Life And Times Of Enrico Fermi, 10
Tinlot, 160
Trabachi, 80
Tramm, 49
transuranic, 32, 33, 63
Trinity, 109, 110, 112, 113, 152, 153
trolley car, 3, 25
Truman, 54, 116, 152
U.S., 20, 33, 50, 51, 54, 72, 94
UCLA, 1, 10, 35
Uhlenbeck, 156
Ulam, 52, 91, 95, 123, 151
Ultimate Accelerator, 38
University of Chicago, 1, 9, 11, 17, 28, 58, 63, 72, 73, 83, 84, 90, 91, 96, 98, 120, 121, 123, 133, 139, 156, 157
uranium, 32, 42, 70, 72, 79, 81, 83, 90, 101, 110, 111, 148, 150, 152
Urey, 73, 91
visa, 50

Von Neumann, 151, 157
Wattenberg, 3, 4, 9, 10, 67, 83, 86, 87, 101
wedding, 60, 61, 146
Weisskopf, 5, 82, 92, 95, 118, 151
Wick, 77
Wigner, 149, 150, 151, 156
Wilson, 3, 4, 5, 3, 4, 7, 24, 28, 48, 51, 54, 67, 69, 75, 76, 107, 115, 117, 125, 137, 159, 160
Wolfenstein, 4, 3, 4, 6, 10, 12, 16, 67, 97, 99, 143
wood, 81, 131
Wright, 10, 47, 142
Yang, 4, 5, 6, 7, 12, 16, 31, 34, 63, 64, 65, 67, 86, 87, 93, 97, 99, 103, 155, 156, 157
Yodh, 9, 10, 94, 97
Yukawa, 38, 92
Zeitschrift fur Physik, 63