

WORKING WITH FERMI AT CHICAGO AND LOS ALAMOS

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Abstract:

I discuss my experience with Enrico Fermi as student and fellow faculty member at Chicago 1947-1952 and with him as consultants to the Los Alamos Scientific Laboratory in 1950-1952. The talk shares observations about this great physicist and exemplary human being.

Enrico Fermi: *The Master Scientist*

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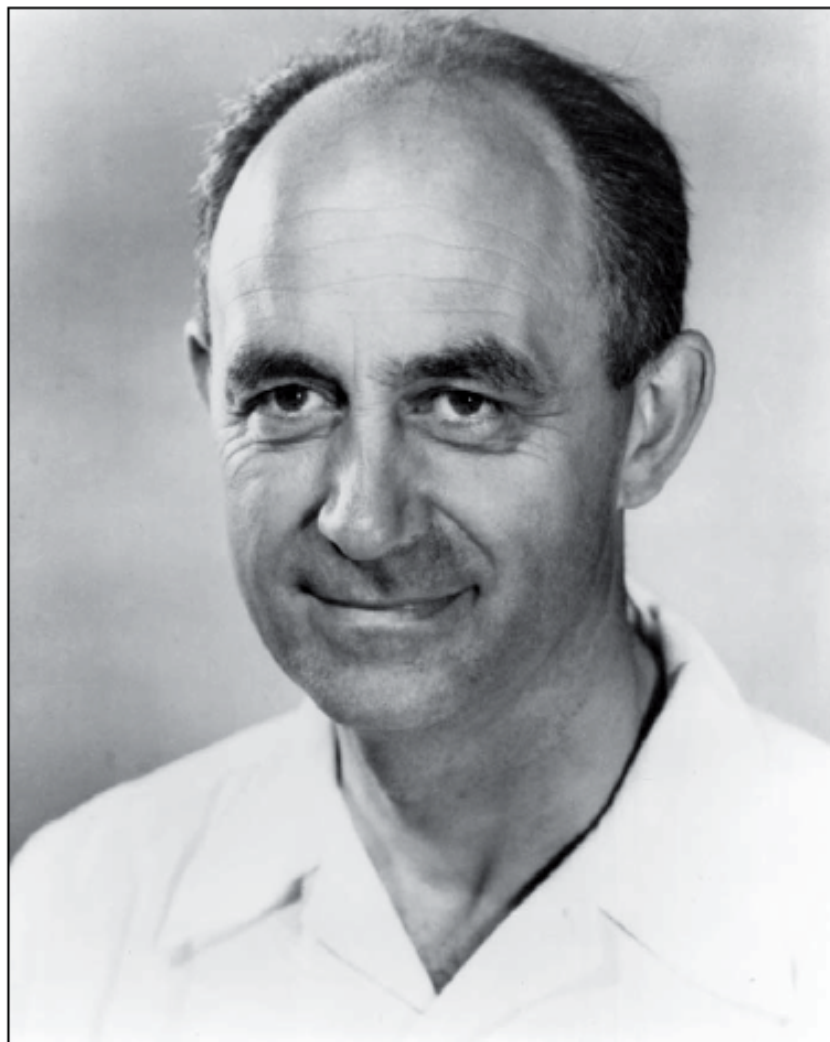


Figure 1.
Photo of Enrico Fermi while at Los Alamos. Note the smile and twinkle in his eyes, which were typical. It is as if we can tell what this man is like on the inside just from this photo. Courtesy Fermilab history of accelerator physics office.

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Enrico Fermi's work speaks for itself, but you probably haven't read it all and won't, and neither have I. So I will try to provide some firsthand recollections from a very imperfect observer. I have posted two of my talks from the 2001 centennial year of Fermi's birth; here modified and with a few pictures, courtesy of my friends and colleagues Harold Agnew and Jay Orear.

I came to Chicago for graduate work in Physics in 1947 with my wife Lois, and after some months of courses decided that I needed to do some experimental work, which is my interest and my strength. So I got up my courage to see Prof. Fermi and volunteered to help in his lab, which turned out to be a good thing to have done. I discovered that he had in his laboratory a machine shop --his own lathe, cut-off saw, and the like. Fermi had great respect for the ability of the central shops at Chicago, but felt they were too fastidious, and if you put something there you would get it back ten times as accurate and ten times as long delayed as it should be. So he made a lot of his own equipment and this was my tendency, too.

In his laboratory I found also Leona Woods Marshall-- Fermi and Leona had just done some work on electron-neutron interaction--and Jack Steinberger, who was in one corner with trays of long brass Geiger tubes and piles of graphite and lead, looking at cosmic-ray muon decay. Fermi and Marshall were doing an experiment on positron annihilation, using glass Geiger tubes; they sealed the tubes themselves, incorporating a cotton thread soaked with 12.4-hr half-life Na-22 radioactive material, yielding a positron which would then annihilate. But Martin Deutsch at MIT was studying too and he, pretty soon, scooped them because he had access to scintillation counters with 2-inch-diameter developmental end-window phototubes from RCA. Eventually Fermi got such phototubes, and we decided we would put them to work.

The electronics at that time was rather primitive, using Rossi coincidence circuits from the 1930s which had microsecond resolving time and Geiger tubes with hundred microsecond dead time. It was time to invest in building things that could be used more efficiently for the future. I looked into these matters and built and published some fast pulsers and coincidence circuits which were in the few nanoseconds rather than the microsecond range, using what would now be called vacuum-tube current-switch technology and a self-adapting diode clamp. Those were widely used for a decade or more at Chicago and elsewhere.

I remember how sad I felt when Fermi's technician left for another job. The great physicist, still in his prime at 47, was dependent to some extent upon the work of this young man. And yet Fermi realized that the young person had to move on in order to have a career, and Fermi had to start again with somebody else and show him how to do the work.

Fermi's propensity for building his own equipment was shown when the 450-MeV cyclotron was put into operation. First in the construction of the synchrocyclotron, for which a lot of the problems with high-power radio frequencies come in spades; when you build such a thing which has the requirement to provide an electric field over a large area, you put it together and you find that all of the power leaks out someplace else, something burns up. This is even more of a problem when the frequency is swept over a substantial range --as is required by the mass of the proton increasing from 938 MeV at rest to $(938 + 450)$ MeV when it has been accelerated. The frequency changes in a reciprocal fashion.

Fermi would meet every morning at 7:30 with the engineers--Leroy Schwartz and colleagues--listen to their report on the problems and prospects of the cyclotron activity; talk to them about the analyses he had made of the progress of the previous day; and then he would go about his other work and hear from them either later that evening or the next morning. Fermi's advice was invaluable in conquering parasitic oscillations of the rf system.

When the cyclotron started to work, there were beams to be brought out and targets to be positioned within the cyclotron. Rather than having a large number of targets mounted on probes penetrating the vacuum chamber that could be plunged into position, Fermi conceived of a movable target-- a trolley that would move along the

rim of the cyclotron. He took advantage of all of the characteristics of the cyclotron-- it has an intense magnetic field so to build a motor you don't need to provide for a magnetic field, it's there. It's a stronger field than in any motor. The cyclotron had a circumferentially ridged pole and so Fermi could build a cart that would move along the pole in a circumferential manner. The cart had a little copper or carbon target that could be flipped into the beam or not by switching current to a coil on the cart.. After the trolley was made, he decided to put a thermocouple on the target to measure the power dissipated there in ionization and nuclear interactions.

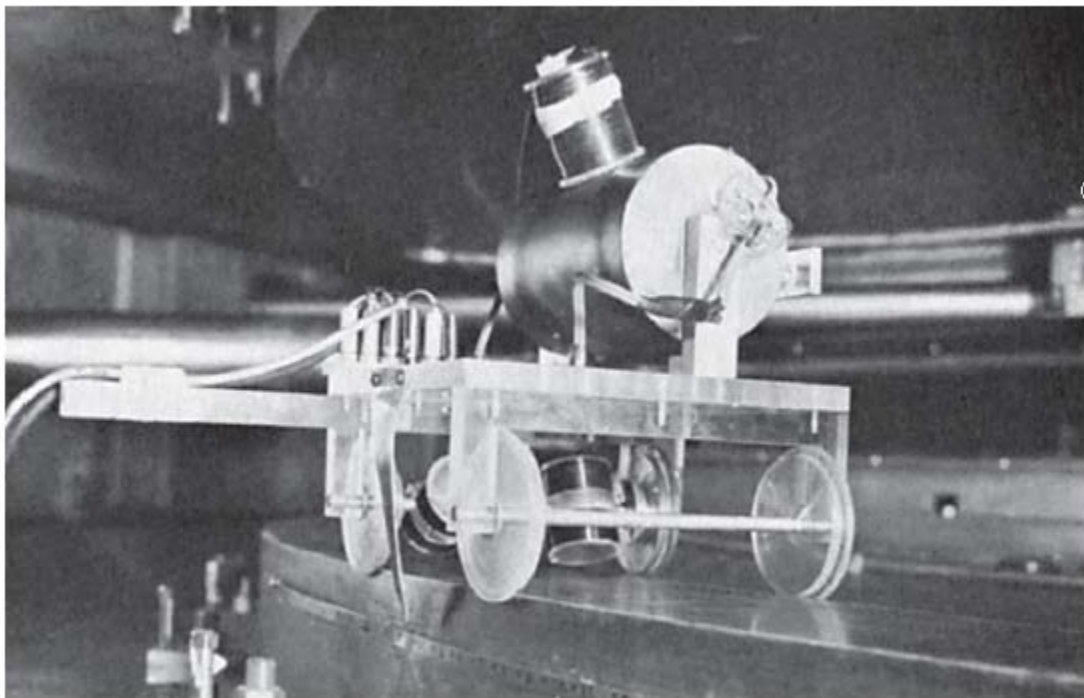


Figure 9.
Fermi's famous trolley car. It held the target of the Chicago cyclotron. The two-dimensional position and energy dissipation were measured remotely. It was designed and constructed by Fermi himself.

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(Jay Orear)

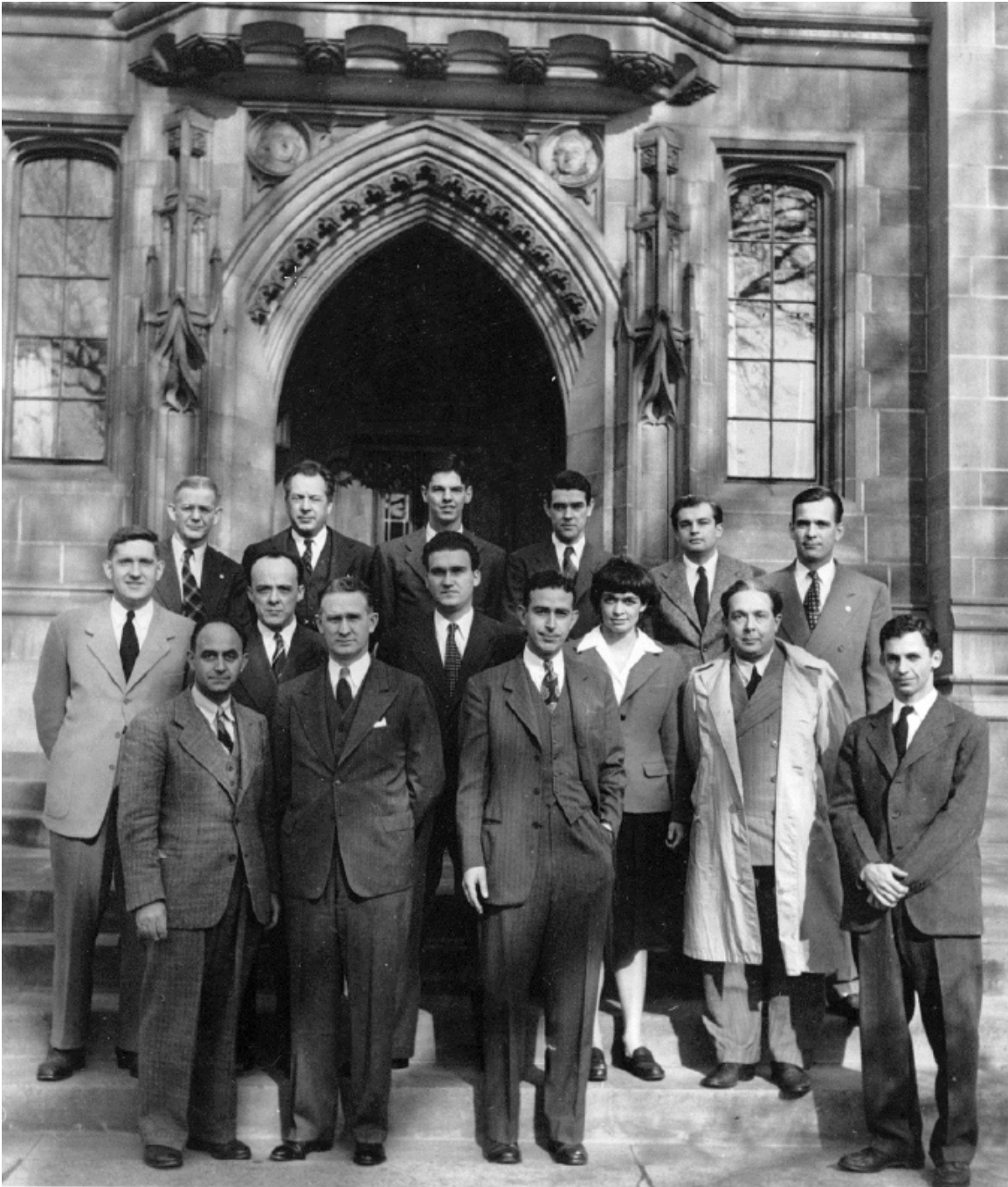
I remember that about 35 watts of target dissipation was pretty good behavior of the cyclotron for awhile. Although Fermi could control the cyclotron very well, less capable people were bothered by the time lag between changing the beam incident on the target and the target’s assumption of a new temperature as measured by the thermocouple. So I built a little circuit to anticipate the result. You could control the cyclotron and instead of having, I guess, a 90-second time lag there was an irreducible couple of second lag to perceive the result of the adjustment.

Fermi was interested in calculating the wave functions of nucleons in peculiar nuclear potentials-- there were Feenberg models, there were Jastrow models of the nuclear potential, among others. One day, I came into Fermi's lab in Ryerson Hall, where he had a coil (or maybe it was in this case a bar magnet) suspended as a torsion pendulum on a lab bench. He was going to make an analog computer. The analog of the spatial distribution of a one dimensional particle wave function was going to be the time behavior of this torsion pendulum. And the analog of the prescribed potential would be the current fed to the coil as a restoring torque coefficient. It's a perfect analogy, for small angles.

Now if we were going to do this, we would need to record the position of the needle with respect to time, and to give it the current as a function of time. I suggested that this was probably not the best approach; that one could have an all-electronic analog computer which used just operational amplifiers. Having suggested it, of course, now I had to build this thing. Fermi used it some. It was a whole rack of equipment in those days with special operational amplifiers I designed, that used plus and minus 600-volt supplies, and had a curve follower so that you could input the potential— $V(x)$; after Fermi's death, Clyde Hutchinson used it but nobody else, to my knowledge.

Among my regrets is the failure to respond to one of Fermi's suggestions. When I received my Ph.D. in December 1949 and joined the faculty of the Physics Department, I had my own laboratory in the Institute of Nuclear Studies. I was busy doing experiments on the 100-MeV betatron; planning experiments for the cyclotron; getting ideas where I could--including scintillation counter ideas from Gaurang Yodh and others. Fermi came in one day and in his typical fashion he asked "whether I had thought about" including spin-orbit interaction in the calculation on nuclear wave functions and energy levels. So I thought about that. Two weeks later he came back and asked what progress I had made and I told him none. So he said he would talk to Maria Mayer about it. I had simply lacked the courage to put down what I was doing to take up a new challenge, where in fact I would probably not have done nearly so well as Maria.

In those days, Chicago paid faculty nine-month salaries. They could either starve or get government contracts for the other three months, but I was unwilling to do either. Fermi suggested that I could be a consultant to Los Alamos. Rumor has it, although I don't recall, that I had made some suggestions to him about nuclear weapons and he said that the place to discuss such things was Los Alamos. So in 1950, my wife Lois and I and our 6-month-old son Jeffrey, went to Los Alamos for three months. Fermi and his wife Laura were there, and, in fact, I shared an office with Enrico Fermi which was a good experience. People would come in and talk to him. During the war he had gone to Los Alamos, not when the laboratory was formed in March 1943, because he was busy in Chicago helping to design the plutonium production reactor at Hanford, after his achievement of the first self-sustaining nuclear chain reaction at Chicago December 2, 1942.



Most of Fermi's team, the first to achieve a self-sustaining nuclear chain reaction. (Photo from University of Chicago, 1946)

Fermi arrived at Los Alamos in the Fall of 1944 and worked there through 1945. He was not in charge of any development group, although there was an "F-Division," but he was a treasured consultant, known as "the Pope." Anyone needing the result of a calculation or experiment could ask Enrico and one way or another he would either show them how to estimate or, in extremis, provide an answer. Notably, Edward Teller and his couple of assistants working toward thermonuclear weapons were moved to F Division to pursue their work toward thermonuclear weapons.

Fermi was also accessible as a friend. At Chicago and at Los Alamos, as had been the case in Italy, Fermi was proud of his acute vision, his stamina and physical achievements. With his dog-paddle stroke, in Lake Michigan he was able to forge ahead of collegiate swimmers and return to encourage their progress. His steady step up mountains left much younger sprinters in the dust. He loved to ski. But he was not a good fisherman and opined to Emilio Segré that it would be more sporting to use a line with a hook on either end, the fisherman holding one in his mouth as he sought his quarry.



Fermi with Maria Martinez, renowned potter, and one of her grandchildren. (Photo from San Ildefonso Pueblo open house, via Harold M. Agnew)

Into our office at Los Alamos came Fred Reines in 1950, suggesting that maybe with all of these nuclear explosions at the Nevada test site, he could put a detector underground and detect the (anti)neutrinos from the beta decay of the moles of fission products. Fermi pointed out that a nuclear reactor-- one of the modern reactors, anyhow-- burns about three kilograms of uranium a day and that fission of one kilogram of uranium in a nuclear weapon gives 17 kilotons. Lots more neutrinos are available from a reactor and you can get closer to it, so that Fermi's suggestion to Fred Reines led him to do the more feasible continuous experiment at reactors, for which Fred in 1995 received the Nobel Prize.

Of course, physicists, especially experimental physicists, have a lab notebook, and mine at Los Alamos, which I obtained on my first day at Los Alamos in 1950, was classified Secret, Restricted Data; it went into my safe whenever I was not actually using it. Fermi preferred to keep his work unclassified, so he occasionally wrote in my notebook either for my edification or because the work was truly classified, or because it was uncertain whether it was or not. Here is the first of seven pages in Fermi's own hand, of the near- and far-field disturbance from a (nuclear) explosion in an underground cavity. I would characterize his calculation as earthshaking, literally but not figuratively.

Explosion in underground cavity

(E. Fermi in R.L. Garwin's notebook
LANB 3616)

$$\text{Total energy} = 5 \times 10^{21} \text{ ergs} = W$$

$$\text{Initial radius } R = 33 \text{ m}$$

$$\text{Initial volume } \frac{4\pi}{3} R^3 = 1.25 \times 10^5 \text{ m}^3$$

$$p = \frac{W}{V} (\gamma - 1) = \frac{5 \times 10^{21}}{1.25 \times 10^{11}} \cdot \frac{2}{3} = 2.7 \times 10^{10}$$

From p. 6

Assume equation of state of rock

$$E = \frac{1}{2} k (v_0 - v)^2 = \text{ergs per cc}$$

$$p = (v_0 - v) k$$

$$c = \sqrt{k v_0^2}$$

$$v_0 = .4 \quad c = 5 \times 10^5 \quad k = 1.57 \times 10^{12}$$

From 3rd Hugoniot

$$\frac{1}{2} k (v_0 - v_1)^2 = \frac{1}{2} p (v_0 - v_1)$$

$$v_0 - v_1 = \frac{p}{k} = \frac{2.7 \times 10^{10}}{1.57 \times 10^{12}} = .0172$$

$$v_0 = .4000$$

$$v_1 = .3828$$

07/00/50

"Explosion in underground cavity," seven pages by Enrico Fermi in R.L. Garwin's Los Alamos notebook (LANB 3616), calculating the radiated wave from an explosion in an underground cavity of initial radius 33 m, and total energy 100 kt. (070050..EF)

Those of you who know something about the extensive efforts on detection of distant underground nuclear explosions by seismic signals (and the flipside of that coin, the possibility of reducing the seismic signal by using a large enough cavity so that the rock remains in the elastic range) will recognize from the first few lines that Fermi has taken a 100 kiloton explosive in a cavity of 33-meter radius—just a bit larger than the cavity radius that would “fully decouple” a mere 1-kt explosion. I won’t go into any detail about this calculation except to observe that most of the elisions are not to correct mistakes but to cancel like terms on two sides of the equation or between numerator and denominator. On page 4 Fermi concludes that “5% of energy is elastic radiation.”

Fermi and Stanislaw Ulam would work together at Fermi's desk, on calculations for the burning of a cylinder of deuterium (the "classical super")--which had been Fermi's original suggestion in 1941 to Edward Teller--that had caught fire with Teller but not in reality. The classical super turned out to be very difficult, maybe barely feasible now. I won't detail the difficulties. Fermi would start with an accountant's spreadsheet, a Marchant electrically driven desk calculator, and a slide rule. Having converted the partial differential equations that were involved to first-order differential equations; he would fill in the first few rows of the spreadsheet-- time would march down the spreadsheet; and he and Stan would talk about the parameters. And then their computer would take over, which was Miriam Caldwell. Overnight she would complete the spreadsheet that Fermi had begun and would return the next morning with the completed product. Fermi and Ulam would graph the results and give the computer the next problem. Their results were published in the classified document LA-1158 of 1950.

Part of my own work that summer of 1950 was to begin an experiment to measure the d-d and d-t cross-sections which had been measured ten years previously at the University of Texas. I thought it was a pretty weak reed to lean on in deciding to build hydrogen bombs or not, when you didn't know what the cross-sections were. So I devised and began to build the equipment. When I had to leave at the end of the summer to return to my responsibilities at Chicago, Fermi encouraged Physics Division leader Jerry Kellogg and the laboratory director, Norris Bradbury, to import Jim Tuck from England-- he had been at Los Alamos during the war-- to continue this work, which was then published in 1954.

I worked also on diagnostics of nuclear explosions. The first couple of weeks that I was at Los Alamos in 1950, I spent in the classified report library reading the weekly reports of all of the groups from the Lab's beginning in 1943.

In 1951 I was back for the summer. I didn't share an office with Fermi. The Physics Division decided I would be better off as a consultant to the Theoretical Division and that's where I was ever after in my summers at Los Alamos. Fermi was concerned that summer with a large number of things, including Taylor instability. If you have a stable interface like this glass of water, you perturb it and it ripples, and everybody knows that there are waves that run on the surface of the water. Most children know also to put a card over a full glass of water and turn it over, and the water stays in the glass-- it is stable. But it's meta-stable. If you take the card off, the interface is still supported by the air pressure but the water pretty soon falls out. This is a very important phenomenon in nuclear weaponry and had been plaguing the people at Los Alamos ever since they considered implosion weapons in 1942, but they unexpectedly had actually to make them in order to use plutonium in 1944.

Fermi had schematized the problem on his blackboard. For the initial stages of Taylor instability you assume a ripple on the surface, and instead of behaving sinusoidally in time it behaves exponentially in time with the same time behavior except it's imaginary instead of real or vice versa. So there is a time in which the amplitude doubles; the next interval it doubles again; the next interval it gets to be eight times as large as the initial disturbance. And pretty soon, of course, this cannot go on because the energy in the instability exceeds the energy that was driving it; the velocity exceeds the velocity of light. And so the question is what happens at large amplitudes? So Fermi said, let me make a 2-D model; I'll have a broad tongue which moves into the dense material; I'll have a narrow tongue that moves away from it and I'll just solve this numerically. So he did some of that but he wasn't quite satisfied with the solution. One afternoon around 4:50 p.m., John von Neumann came by and saw what Fermi had on the blackboard and asked what he was doing. So Enrico told him and John von Neumann said "That's very interesting." He came back about 15 minutes later and showed him how to approach the problem. Fermi leaned against his doorpost and told me, "You know that man makes me feel I know no mathematics at all."

Fermi's crude approximation of September 1, 1951 is published as No. 244 in *The Collected Papers of Enrico Fermi*. It shows the narrow tongue proceeding into vacuum with a large-amplitude uniform acceleration of $8/7$

g. The more rigorous analytical calculation of the model (with von Neumann) is No. 245; the limiting acceleration is still $8/7 g$. If the problem is the Taylor unstable interface between two incompressible fluids, the result is, naturally, multiplied by the fractional difference in density.

Fermi had often done hand calculations such as those with Ulam on the classical super, so he was open to the wondrous advances of the first electronic computers at Los Alamos—the Maniac, a copy of the Princeton Institute of Advanced Studies “Johniac” built by John von Neumann and Herman Goldstine. Fermi was eventually to do an important calculation on the Maniac regarding the phase-shift analysis of the energy dependence of the measured cross sections of the scattering of negative and positive pions by protons. The first calculation was performed by Fermi and Nick Metropolis and appears as No. 256 in *The Collected Papers of Enrico Fermi*, with a foreword by Metropolis. The paper indicates, incidentally, the speed of the Maniac—a hand calculation that takes 20 minutes by Marchant and slide rule takes only 0.4 seconds on the Maniac. The Maniac could perform about 10,000 fixed point operations per second, to be compared with the billions of operation per second on your few-hundred-dollar laptop.

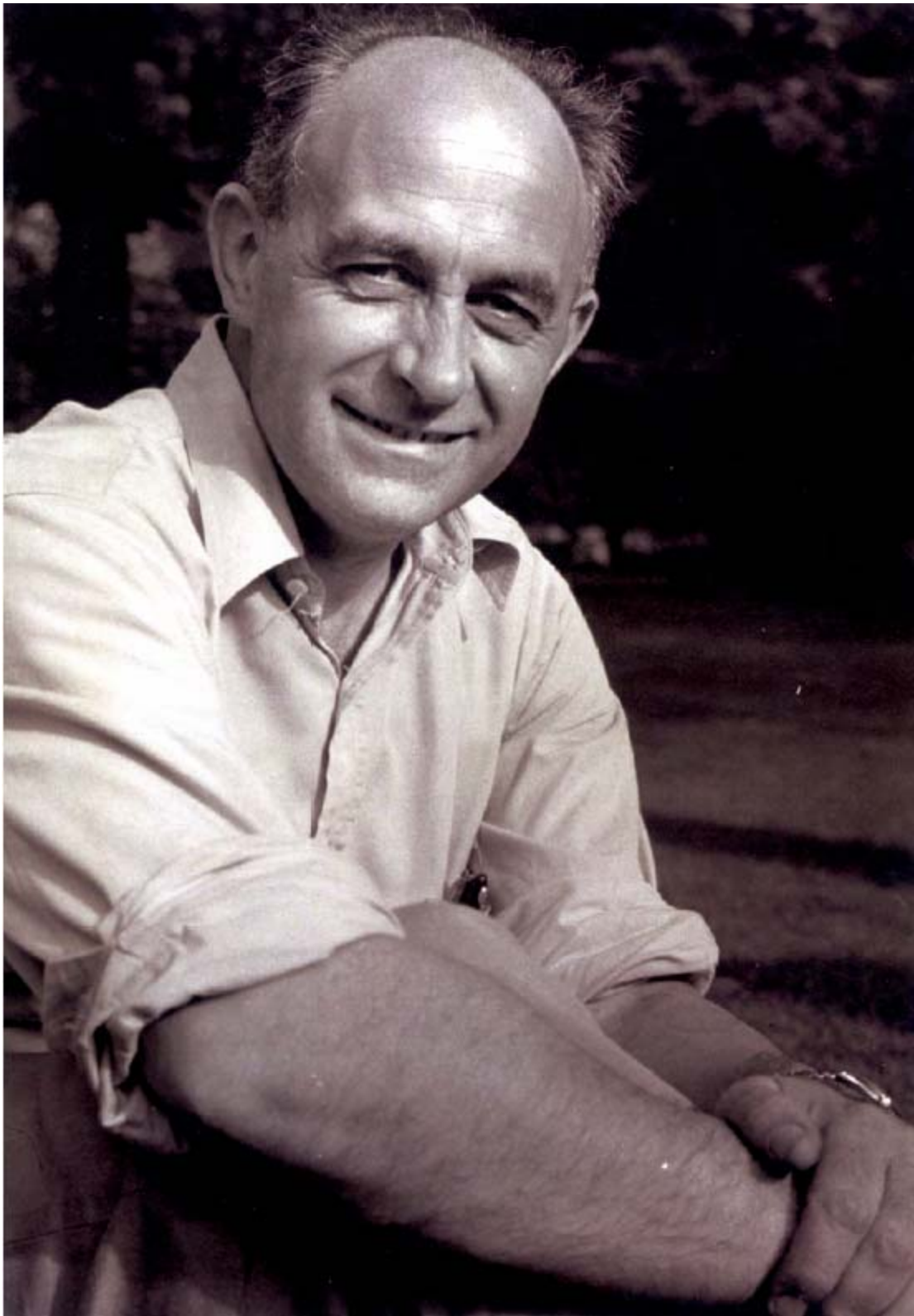
Fermi had always been interested in the ergodic theorem and in understanding the approach to thermal equilibrium or to random distribution. He had the idea of using the Maniac to calculate the behavior of a chain of loosely coupled harmonic mass-spring oscillators, for which the coupling energy between adjacent masses had not only the usual quadratic form in displacement difference but also a small cubic term. It is easy to estimate the time required for the damping of the fundamental mode by transfer of the energy to higher modes in this conservative system. Paper No. 266 (1955—published after Fermi’s death) summarizes this work; a foreword by Ulam notes Fermi’s surprise at the results. After thousands of computing cycles and what appears to be approaching equipartition among modes of oscillations, almost all the energy returns to the initial mode. With further “great cycles” the energy in the initial mode is reduced by, say, 1% per great cycle, but after 16 great cycles the energy in the initial mode returns within 1%.

There was another time at Chicago where our colleague, Edward Teller, who was also on the faculty, came by and told Fermi of his most recent enthusiasm. After Teller left, Fermi commented to me, "That's the one monomaniac I know with more than one mania."

I left the University of Chicago in December 1952 for the IBM Watson Scientific Laboratory at Columbia University, to change my focus from particle physics to condensed matter physics. I didn't like having to tell people six weeks in advance what I wanted to do with the cyclotron or to work with a team of six people. Now it's 60 weeks in advance and 600 people, so I think I made the right choice. But I certainly admire what has been done in particle physics since. I did work at IBM on superconductors, and liquid and solid helium, and I continued to work summers at Los Alamos. I didn't realize how much I would miss the daily contact with Fermi

By 1954 I had worked for a year (sort of half-time) on air defense of the United States and had made contact with people in Washington outside the nuclear weapons community. Hearing of Fermi's illness I returned to Chicago, I think, in October and saw him in his house. He had an inoperable cancer of the lining of the stomach. We talked for an hour or so. He regretted not having been more involved with public policy about nuclear weapons, for example. I was, of course, terribly saddened that Enrico Fermi was taken from us at the age of 53; I just imagine his joy had he been around to see the evolution of computers and the development of physics, and the role that he might have played had he lived another 30 years to be as old as I am now.

As one of his students put it in 2001, “We were all smarter when Fermi was around.”



Enrico Fermi in Los Alamos, 1952. (Photo by Harold M. Agnew)