FRANCO RASETTI
(1901-2001)

INTERVIEWED BY
JUDITH R. GOODSTEIN

February 4, 1982

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Physics, paleontology, botany

Abstract

Along with Enrico Fermi, Franco Rasetti played a key role in the rebirth of Italian physics in the 1920s and 1930s. In this interview he talks about experiments at Caltech on the Raman effect in 1928-1929, mountain climbing, his passion for bugs, fossils and flowers, and doing physics in Florence, Rome, Berlin-Dahlem and Quebec. Rasetti also reminisces about the Rome school of mathematics and other scientists he has known and worked with in Europe and in North America, including Robert and Glenn Millikan, Lise Meitner, and O. M. Corbino.

Administrative information

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The interview is unrestricted.

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Preferred citation

Rasetti, Franco. Interview by Judith R. Goodstein. Waremme, Belgium, February 4, 1982. Oral History Project, California Institute of Technology Archives. Created from revised version published in *Physics in Perspective*, 3 (2001), 271-313. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Rasetti_F

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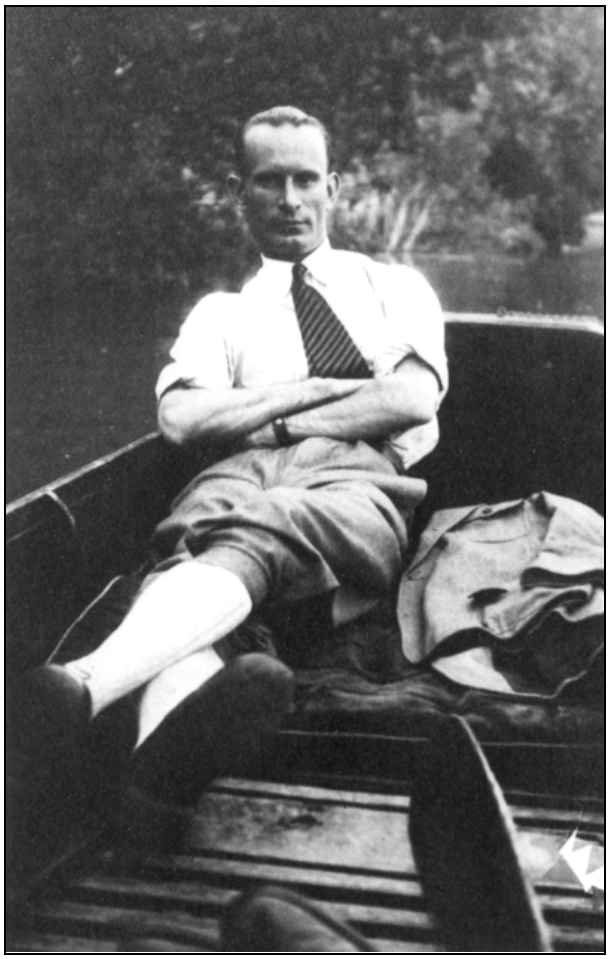

Lounging in a punt in Cambridge, England during the summer of 1932.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH FRANCO RASETTI

BY JUDITH R. GOODSTEIN

WAREMME, BELGIUM

Caltech Archives, 2002

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INTRODUCTION TO FRANCO RASETTI ORAL HISTORY

by

JUDITH R. GOODSTEIN

University Archivist

The following interview was conducted on February 4, 1982, with Professor Franco Rasetti, member of the Rome school of physics between 1927 and 1938. Rasetti was born in Castiglione del Lago, Italy, on August 10, 1901. After receiving his doctorate in physics from the University of Pisa in 1922, Rasetti moved to Florence, where he held the position of lecturer to Antonio Garbasso, director of the Physics Institute of the University of Florence in Arcetri. Five years later, in 1927, Rasetti transferred to Rome, where he became first assistant to Orso Mario Corbino, Professor of Experimental Physics and Director of the Physics Institute of the University of Rome, located on Via Panisperna. Rasetti spent the 1928–29 academic year in Pasadena, at Caltech, in R.A. Millikan's laboratory, where he did important work on the Raman effect. Within a few years of his return to Rome, he had gained an international reputation, mainly through his work on the Raman effect. He became professor of physics at Rome in 1934. Five years later, shortly before the outbreak of World War II, Rasetti was invited to join the faculty of Laval University in Quebec, where he taught, did research on cosmic rays, and founded the university's first department of physics. A born naturalist and outdoorsman, Rasetti took up geology and paleontology while in Quebec. In 1947, he accepted a position as professor of physics at Johns Hopkins University, where he remained until 1970, returning afterwards to Italy. He started on his botanical studies (Alpine flowers and Italian orchids) while he was at Johns Hopkins.

The interview was conducted in Waremme, Belgium, where Franco Rasetti and his wife Marie Madeleine Rasetti now live, as part of the Caltech Archives' Oral History Project. The interviewer is Judith Goodstein, University Archivist.

Acknowledgments

I thank Professor and Mrs. Rasetti for permission to publish this oral history and colleagues Sara Lippincott and Roger H. Stuewer for various helpful suggestions. This work was supported by a grant from the John Randolph Haynes and Dora Haynes Foundation.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with Franco Rasetti **By Judith R. Goodstein**

Waremme, Belgium

February 4, 1982

Begin Tape 1, Side 1

GOODSTEIN: What made you choose Caltech in 1928?

RASETTI: Well, [Robert A.] Millikan's name was very well known in Rome. So I came partly for that reason and partly because I wanted to see California.

GOODSTEIN: You spent one year there.

RASETTI: Yes. Well, about nine months.

GOODSTEIN: Then you went to Hawaii.

RASETTI: Hawaii. From Hawaii, I took a British ship to Vancouver. I climbed five or six peaks in the Canadian Rockies, then I took the train to New York, and then back to Europe.

GOODSTEIN: Whose laboratory did you come to Caltech to work in?

RASETTI: Well, it was Millikan, in the physics department. Millikan was at the same time, of course, head of the institute and chairman of the department.

Now, the first person I met, who came to meet me at the station, was I. S. Bowen—I didn't know him personally, but his name was very well known to me because he had worked in atomic spectra, which had been my field up to that time in Italy—in Florence and later in Rome.

So Bowen very kindly came to meet me at the station and also found me lodging in the home of Millikan's secretary.

GOODSTEIN: Inga Howard?

RASETTI: Yes, exactly. And another guest at that home was Theodosius Dobzhansky, and we became very good friends. Of course, he was a famous geneticist, but he had a secondary interest in beetles—in the systematics of lady beetles. And as beetles had been my specialty since childhood, we had quite a lot in common to talk about.[1]

Also, I wanted immediately to buy a car. In fact, the day after my arrival I bought a car, which surprised everybody, because Europeans were not supposed to be car-minded, like Americans. But I couldn't exist without a car. So I bought a secondhand Chevrolet, which worked very well for the time I was there.

Now, my work happened to have nothing to do with the work that was being done in the Caltech physics department.

GOODSTEIN: That's one of the things I wanted to know.

RASETTI: It so happened that a month after I got to Pasadena, [Chandrasekhara V.] Raman discovered the Raman effect, and it was announced by a letter in *Nature*.[2] Raman observed this effect in organic liquids, like benzene, carbon tetrachloride, and similar things. Some Russians independently, just a little after Raman, had observed it on transparent colorless crystals, like quartz, fluorite, calcite, and the like. Then I thought, Well, it might be interesting to study the

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[1] On Rasetti see Thelma Nason, ''A Man for All Sciences,'' *Johns Hopkins Magazine* **17** (1966),12–17 and 25–27; Edoardo Amaldi, ''The Case of Physics,'' in Giovanni Battimelli and Giovanni Paoloni, eds., 20*th Century Physics*: *Essays and Recollections: A Selection of Historical Writings by Edoardo Amaldi* (Singapore: World Scientific Publishing, 1998), pp.168–190, esp. 175–178, and Emilio Segrè, *Enrico Fermi*: *Physicist* (Chicago: University of Chicago Press, 1970), chapter 3.

[2] C. V. Raman and K. S. Krishnan, ''A New Type of Secondary Radiation,'' *Nature* **121** (1928), 501. This issue is dated March 31, 1928. Rasetti may be remembering a further letter on the discovery (''The Raman Effect in Highly Viscous Liquids'') which also was published in *Nature*, by S. Venkateswaran, on October 6, 1928.

Raman effect in gases, and I was the first to do that. And also, for studying it in gases, I thought it would be convenient to use as a source not visible light—which had been done by Raman and the Russians—but ultraviolet light, with a mercury arc. The setup was very simple. All the apparatus was on a table [laughter]; at that time, you could make discoveries with very simple means. It was amazing that the Raman effect was not discovered a century or half a century before, because all that was needed was the most common equipment to be found in any physics lab, and it took an hour to set it up. But somehow it had been missed, because it was sort of an axiom that the light scattered by a transparent colorless medium would have the same wavelength as the incident light.

Fig. 1. Franco Rasetti, Ensenada, Baja California, Thanksgiving, 1928, photograph by Glenn A. Millikan. In his photo album, Glenn Millikan wrote the following caption: ‘‘Lovely uninhabited beaches with wonderful kelp. Greatest discovery of the trip: Franco.’’ Caltech Archives, R.A. Millikan Collection, Box 92.

Instead, Raman and others found that when a solid or liquid transparent medium was irradiated with monochromatic light, the scattered light contained, in addition to light of unchanged frequency, pairs of lines, symmetrical on the frequency scale with respect to the incident frequency, shifted by plus or minus the frequency of a characteristic vibration of the atoms in the crystal or molecule.

In gases, of course, the effect would be extremely weak, because a gas is thousands of times less dense than a liquid or a solid. So I took just an ordinary quartz lamp, a mercury vapor lamp with a quartz wall, water-cooled, in order to obtain a strong emission of the resonance line of the mercury atom, of 253.6 nm wavelength, the one that generated all the Raman lines I studied. I built some tubes, which I filled with high pressure gases—several atmospheres of oxygen, nitrogen, hydrogen; the most common diatomic gases—to get a stronger effect. And I immediately photographed the Raman effect of gases.

These observations were important, because one understood all about diatomic molecules in the gas form, whereas solids and liquids are very complicated systems.[3] So it was easy to observe the frequencies in a crystal, but it was hard to relate them to theory. In diatomic gases, there is just one vibrational frequency, hence just one pair of Raman lines corresponding to it was found. There is, however, another type of motion that may generate Raman lines: the rotation of the two atoms about an axis passing through the center of gravity of the molecule and perpendicular to the line joining the two atoms. This rotation of the molecule is, of course, quantized, and as it does not involve a large amount of energy, many rotational quantum states are excited even at room temperature. Quantum theory predicted that in Raman scattering, the rotational quantum number should jump by two units, such as from zero to two, one to three, two to four, and so on, or vice versa—the intensities of the Raman lines first increasing, then decreasing with increasing quantum numbers, according to a regular curve. This is true for molecules made of two different atoms, as I observed in carbon monoxide. For homonuclear molecules, however, the intensities of even- and odd-numbered lines should fall on two separate curves, alternating intensities, the intensity ratio being (I+1)/I, where I is the spin of the nucleus, measured in quantum units of h-bar. For hydrogen (spin ½, ratio of intensities 3) and oxygen

[3] On the history of nuclear physics before the discovery of the neutron and the reception of Rasetti's measurements among European physicists, see Roger H. Stuewer, ''The Nuclear Electron Hypothesis,'' in W.A. Shea, ed., *Otto Hahn and the Rise of Nuclear Physics* (Dordrecht: D. Reidel, 1983), pp. 19– 67, esp. 35–37.

(spin zero, ratio of intensities infinite—that is, alternating lines missing), the results confirmed expectations. For nitrogen, known to have spin 1, the ratio was 2 as expected, but the lines expected to be weak were strong, and vice versa. The expectation had derived from the assumption that the nitrogen nucleus consists of fourteen protons and seven electrons, hence a total of twenty-one particles, an odd number. [Walter] Heitler and [Gerhard] Herzberg, two theoreticians particularly expert in molecular spectra, pointed out this apparent contradiction, which was resolved only several years later with the discovery of the neutron and the conclusion that the nitrogen nucleus consists of seven protons and seven neutrons.

I must say that nobody else was interested in the Raman effect at that time at Caltech, or anywhere else, for it was something new. So my work had nothing to do with the other activities that were going on in the department. And perhaps my results were understood but not entirely appreciated, because nobody was familiar with molecular spectra.

Now, fortunately for me, it happened that just a month after I had gotten all of these results on these diatomic gases, there was a meeting of the American Physical Society in Berkeley. And the chairman of the physics department at Berkeley, Dr. Raymond T. Birge, was also the president of the American Physical Society at that time. Birge was also a specialist in molecular spectra. So we went up to Berkeley to attend this meeting, and Birge gave a talk about the most important results in physics in that year. He devoted part of his talk to my work on the Raman effect.

GOODSTEIN: How did he know what you had done?

RASETTI: It was published already.

GOODSTEIN: So you published it very fast.

RASETTI: Yes, it was published very fast, first in *Nature* and then in *PNAS* [*Proceedings of the National Academy of Sciences*], so he knew of these results.[4] So that made a great impression on

[4] Rasetti, ‘‘Raman Effect in Gases,’’ *Nature* **123** (1929), 205 and ‘‘On the Raman Effect in Diatomic Gases,’’ *PNAS* **15** (1929), 234–237, and ‘‘II,’’ 515–519.

Millikan and all the Caltech physicists, because they hadn't realized that my results were so important. But Birge talked of them as the highlight of the year—at least in the field of spectroscopy.

Then I got down to work on more complicated molecules. I did some work on polyatomic molecules, and I continued it later in Rome [Physics Institute of the University of Rome]. So that's about what I did at Caltech.

We had some interesting visitors: both [Arnold] Sommerfeld and Werner Heisenberg came to visit Caltech. I don't remember which one of them came first, but they both gave lectures on quantum mechanics. I became very good friends with them—especially with Heisenberg, who was exactly my age. He liked mountain climbing, so one day I took him out to Idyllwild and we climbed San Gorgonio—which, of course, is very easy but nevertheless over 11,000 feet.

GOODSTEIN: Had you known Heisenberg before he came to Caltech?

RASETTI: No, no. I met him there. Then I saw him again in Berlin for a year, but that comes much later.

GOODSTEIN: Do you remember what sort of impact his lectures had on the physicists at Caltech? That is, were the physicists at Caltech able to follow all of his lectures on quantum mechanics?

RASETTI: Yes; at least many of them, yes. Both Sommerfeld's and Heisenberg's.

GOODSTEIN: Were these talks well attended?

RASETTI: Yes, yes. They were attended by most of the physicists. And then, of course, I followed what was going on in nuclear physics. But at that time it was a little too early for the big discoveries in nuclear physics. I followed what [Charles C.] Lauritsen was doing. They had built up a sort of Van de Graaff accelerator and they were trying to obtain nuclear disintegrations and transformations by accelerated particles. But they used electrons, and of course they didn't

observe anything, because it was later found that you had to use protons or deuterons—well, the deuteron had not been discovered at that time, of course. But you had to use heavy particles.

GOODSTEIN: I wanted to ask you whether the information that Sommerfeld and Heisenberg brought was new to the Caltech physicists?

RASETTI: Yes, but it had already been published.

GOODSTEIN: Was it new to you?

RASETTI: No, no, it was not new to me, because I had studied it in Rome already. No, it was nothing new, because [Enrico] Fermi had taught on that already, before I went to Caltech.

GOODSTEIN: There was a Caltech physicist, a theoretician, named Paul Epstein. Did he take any interest in your work?

RASETTI: No. I don't remember much about what Epstein was doing, but certainly we did not have any particular interest in common. [Richard Chace] Tolman was another theoretician.

GOODSTEIN: What about Tolman? Did he have any interest?

RASETTI: No, not much relation to my work. I'll tell you about other people whom I met and with whom I made friends. First, as I told you, Dobzhansky, then Fritz Zwicky. Zwicky and I had mostly in common not physics but mountain climbing. And we did the first winter climb of Mount Whitney on the first and second of March in 1929. Apparently it had never been climbed in winter before—at least Zwicky said so. I was very good friends with Zwicky, who was a very original person. Although his main interest was in crystals, as you probably know, he was the one who invented supernovas.

GOODSTEIN: Had much attention been paid to that by the people at Caltech?

RASETTI: Yes—by the astronomers, of course. And though it wasn't my field, I visited the Mount Wilson Observatory; at that time, Mount Palomar hadn't been built.

GOODSTEIN: Did you meet [Edwin] Hubble?

RASETTI: I don't remember meeting Hubble.

GOODSTEIN: Did you ever run across George Ellery Hale?

RASETTI: Yes, yes. But I didn't meet Hubble, or at least I don't recall it. The Paulings were very kind; they sometimes invited me. I liked [Linus Pauling] very much. If you have a chance to see him, give him my regards.

GOODSTEIN: I will do that. Then there was Roscoe Dickinson.

RASETTI: Oh, yes. We worked together on the Raman effect on polyatomic molecules.

Fig. 2. Glenn Allan Millikan, with camping gear, Scotland, 1930. Caltech Archives.

GOODSTEIN: How did that come about—that both of you worked together?

RASETTI: He provided the chemicals and I provided the spectroscope, and we got along very well. We published a couple of papers. Actually, I think the papers on diatomic molecules were mine, and then Dickinson, R. T. Dillon, and I were the three authors on the polyatomic paper.[5]

[5] R.G. Dickinson, R. T. Dillon, and F. Rasetti, ''Raman Spectra of Polyatomic Gases,'' *Physical Review* **34** (1929), 582–587.

Dickinson and Dillon were physical chemists. Another person who was very interesting was Theodore von Kármán.

GOODSTEIN: Did you have much to do with von Kármán?

Fig. 3. In front of Theodore von Kármán's car, 1931. Left to Right: J.D. Burgers, J. M. Burgers, P. Knapp, T. von Kármán. Caltech Archives.

RASETTI: Well, I knew absolutely nothing about his subject, and he didn't know about mine, but we just sympathized. He was a very, very nice and interesting person. As you know, he was a world authority on fluid dynamics, but he was especially known in the department for his car and for his way of driving. He bought a secondhand car that had belonged to a student, and on it was painted the name "Cactus Kate." [Laughter] And so this Cactus Kate and the way Kármán drove it became quite a legend in the department. One time—he told us this story—he was driving on a dirt road that had a grade crossing. At the grade crossing, for some reason, he didn't continue on the road but turned onto the railroad tracks instead, bouncing on the ties. [Laughter] And some people there had to help him get the car back onto the road. He was so absentminded!

GOODSTEIN: Where did you meet other people at Caltech? Was it usually at lunch?

RASETTI: At lunch, at the faculty club. That was, of course, a new experience for me, because in Italian universities there is no faculty club and no common lunch. So I found it a very nice institution, because it was a way of meeting people from other departments. Then, of course, the concept of a campus was new to me, too, because in Europe—at least at that time—there was no such thing. Each department was in a separate building, and often even closely related departments were in different parts of town. For instance, in Florence, where I had my first employment, the physics department was on top of a hill, three or four kilometers out of town. It was near the astrophysical observatory, but chemistry was three miles away, in the center of town. So the concepts of a campus and of a faculty club were all new to me. Now, of course, even in Europe this exists. For instance, in Rome in 1937, the university [departments], which had been scattered all over town, were united into a kind of campus. But still, the concept of a faculty club and meeting at lunch is not a European concept.

GOODSTEIN: Did you have a good command of English when you went to Caltech?

RASETTI: Yes. I had not the slightest difficulty. Italian, French, English, and German I've known since my very young days.

GOODSTEIN: When you were speaking to von Kármán, which language did you prefer?

RASETTI: Oh, when we were there, we always spoke in English, even with the foreigners.

GOODSTEIN: So that was the common language?

RASETTI: Yes.

GOODSTEIN: Do you remember anything about the first physics colloquium you gave at Caltech? I noticed the first talk was by you alone, and then later on you gave a couple with Benedict

Cassen; they were called "The Raman Spectra and Nuclear Spin." And on one of those days, at least, Heisenberg was lecturing also.

RASETTI: I forgot all about this. Cassen, yes....I very often sat with him while they were doing these experiments with the Van de Graaff accelerator.

GOODSTEIN: He was in C. C. Lauritsen's group.

RASETTI: Yes, exactly.

GOODSTEIN: Was there a lot of visiting among the different laboratories?

RASETTI: Well, yes. But this one particularly, because they were working in the evening. So from, let's say, eight to ten I always chatted with Lauritsen and Cassen.

GOODSTEIN: Was [J. Robert] Oppenheimer at Caltech when you were there?

RASETTI: Well, he was there on and off. He was teaching both at Berkeley and at Caltech. I met him there, of course, and I saw more of him later, when I was in Berkeley, in the summer of '35.

GOODSTEIN: How is it that you never came back to Caltech?

RASETTI: Well, I went to other places, since later I was attracted to nuclear physics; in nuclear physics, there was not much going on at Caltech. I went to [Otto] Hahn and [Lise] Meitner's laboratory in 1931-1932, and again in 1933-1934.

The discovery of the neutron, of course, is attributed to [James] Chadwick, who was the first to interpret the results as due to neutrons. But the experiments were done by [Irène] Curie and [Frédéric] Joliot. They had a hydrogen cloud chamber, and they irradiated with a polonium beryllium source, and they observed the proton recoils. But they didn't interpret them as due to a neutron. Chadwick said that those [recoils] are produced not by gamma rays but by neutrons.

And the day after the issue of *Nature* arrived in Dahlem, I was already operating a cloud chamber and already had the polonium beryllium source.[6] So the next day I had reproduced the experiments of Curie and Joliot. So I happened to be the first in Germany to observe the effect of the neutron.

GOODSTEIN: In Germany, there was a great deal of interest in that.

RASETTI: Yes, it was very exciting.

Fig. 4. Lise Meitner and Otto Hahn in their laboratory in the research Institut für Chemie in Berlin-Dahlem, 1913, shortly after Hahn became head of the Institute's department of radioactivity. Courtesy of the American Institute of Physics Emilio Segrè Visual Archives.

GOODSTEIN: Was it more exciting intellectually to work in Lise Meitner's lab?

RASETTI: Well, it was a very different sort of institute, because it was exclusively devoted to radioactivity. Hahn took care of the chemistry, and Lise Meitner took care of the physics. It was quite different from a department like Caltech's—or even in Rome—where more or less all fields of physics were of interest. Their laboratory was concentrated on radioactivity. Both Meitner and Hahn were very pleasant persons, extremely friendly and helpful, and they let me do whatever I wanted to do.

GOODSTEIN: What about Millikan? Wasn't he friendly and helpful?

[6] J. Chadwick, ''Possible Existence of a Neutron,'' *Nature* **129** (1932), 312.

RASETTI: Yes, but he was not watching the actual experiments of individual groups. He was very attentive to the whole thing, but from a distance, so to speak. But Meitner was different. Meitner told me, "Now you take this...." She taught me how to prepare the polonium, how to extract and separate the polonium from radium, how to evaporate it on the beryllium foil, and so on. She actually followed all my technical steps of the experiment. Millikan was sort of—how would you say it?—a directing general; he hired people, and except for the cosmic rays he was not directly concerned with the experiments of an individual or particular groups. And then, of course, he was the head of Caltech; he had a lot of administrative work to do. Whereas Hahn and Meitner did not have much administrative work and were actually working with their own hands. And Millikan wasn't, at least not at that time.

GOODSTEIN: You said earlier that Millikan's name was well known in Rome, before you got to Caltech. Was that because he was a grand personality in physics, or was it because of his work with the oil-drop apparatus?

RASETTI: Yes, mostly for that work; he was mostly known for the measurement of the electron charge.

GOODSTEIN: So when you eventually came back to the United States, it was to places that were doing nuclear physics?

RASETTI: Yes, but I never worked in the United States again until I settled in Baltimore [at Johns Hopkins; 1947], and that was after the war. Before the war, my only return trip to the States was in 1935-1936, with a friend of mine, Lorenzo Emo [Capodilista]; he died very young. On this trip, we left in June, went to New York, then spent a few days in Ann Arbor, where Fermi and [Samuel A.] Goudsmit were teaching at the summer session [of the University of Michigan]. Then we bought a secondhand car in Ann Arbor and drove all over the West, through all the national parks—through the Grand Canyon, which I knew already anyway, and through Zion Canyon and Bryce Canyon, Yellowstone Park, then wound up in Berkeley and spent a month there. Actually, my friend Emo remained there for a year. And there I saw again,

of course, Oppenheimer and [Ernest O.] Lawrence and [Edwin M.] McMillan. I knew all of them from before, but I had closer relations with them in ’35.

GOODSTEIN: Did you pick up any sense of rivalry between Berkeley and Caltech in ’35?

RASETTI: Well, no, I didn’t particularly notice it. For one thing, their main interests were in different fields.

I haven’t talked enough about Millikan’s work[7]. You know, Millikan was completely absorbed in the work on cosmic rays. He measured the absorption, the decrease in intensity with depth, in lakes and in the atmosphere and so on. Well, I must say, in that area he was stubborn beyond reason, because he still believed in his idea that cosmic rays originated by four hydrogen atoms uniting to form a helium atom, and twelve to form carbon, and fourteen to form nitrogen. When I was at Caltech, he was perhaps beginning a little bit to lose faith in his original idea. But he still believed that the primary cosmic rays were gamma rays, even when nobody else in the world believed it anymore. It was becoming so obvious that primary cosmic rays, before they entered the atmosphere, were positively charged particles—nuclei of atoms and not gamma rays. Actually, he claimed that from the absorption of gamma rays with depth in lakes, he could distinguish the energy of the gamma rays that would be produced when helium was formed and those that originated from the formation of boron, carbon, and nitrogen—which was all, of course, completely absurd.

GOODSTEIN: Did you hear him talk about this much during your stay at Caltech?

RASETTI: Yes, he was still giving talks and giving interviews to newspaper men. He talked about the “birth cry” of atoms. I remember he used that phrase. [Laughter]

GOODSTEIN: What about his own research students?

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[7] On Millikan’s cosmic-ray research, see Robert H. Kargon, ‘‘Birth Cries of the Elements: Theory and Experiment along Millikan’s route to cosmic rays,’’ in Harry Woolf, ed., *The Analytic Spirit*: *Essays in the History of Science in Honor of Henry Guerlac* (Ithaca: Cornell University Press, 1981), pp. 309– 325, and Daniel J. Kevles, ‘‘Physicists and the Revolt against Science in the 1930s,’’ *Physics Today* **31** (February 1978), 23–30.

RASETTI: I don't know if his research students believed it.

GOODSTEIN: Carl Anderson was one of them. And I think at that time Ira Bowen was one of his students.

RASETTI: But Bowen was working mostly on atomic spectra, not on cosmic rays.

GOODSTEIN: And there was Victor Neher.

RASETTI: Oh, yes, Victor Neher, yes. Well, I don't know if they believed that. But Millikan, certainly, I remember, was still saying in interviews in 1929 that the primary cosmic rays were gamma rays and corresponded to the formation of successive atoms.

GOODSTEIN: I read recently that at the nuclear physics meeting in Rome in 1931, Bruno Rossi criticized Millikan for this point of view—and Millikan was there, of course. Do you remember any of the dialogue between Rossi and Millikan?[8]

RASETTI: No, I don't. I don't even know if they had a dialogue. Well, of course, at that time it was already well established that primary cosmic rays were charged particles.

GOODSTEIN: So Rossi was not being all that new in criticizing him?

RASETTI: No. Because, of course, it had been observed: first, the equatorial dip, and second, that there were more coming from the west than from the east—which showed totally that they were charged particles, and positively charged.

[8] See also Bruno Rossi's comments in Bruno Rossi, *Moments in the Life of a Scientist* (Cambridge: Cambridge University Press, 1990), pp. 17–19.

GOODSTEIN: Throughout the thirties, Caltech never hired any of the scientists who had to leave Europe. They never took any of the émigré scientists. Now, they could have offered Rossi a position, since he was in cosmic rays. Do you think that the fact that Rossi disagreed so strongly with Millikan made a difference?

RASETTI: I have no idea about that; I don't know.

GOODSTEIN: Had you ever heard that Rossi would have come to Caltech if he'd been asked?

RASETTI: No, I don't know. I just don't know.

GOODSTEIN: OK. They never took anybody. Basically, after 1930 nobody new came to Caltech.

RASETTI: Is that so?

GOODSTEIN: Caltech made use of its own students—who got their PhDs, stayed on, and rose very, very slowly through the academic ranks. Nobody new came, and that was true for the whole decade. [Walter M.] Elsasser, the geophysicist, came for a time in the late thirties, but he's the only one—and then that was only a temporary appointment. Did you, when you were there in '28-'29, have the sense that Caltech was becoming very closed?

RASETTI: I have no elements of comparison, of course, because I didn't know many other American universities. I have no way of comparing it.

GOODSTEIN: Was it a very exciting place to be, Caltech in 1928-1929?

RASETTI: I think it was.

GOODSTEIN: Did you have the feeling that everybody was participating in something very new?

RASETTI: Yes, yes. And it was especially nice to have so many people with different interests—like Pauling, who became interested in molecular biology.

GOODSTEIN: So you went to hear Pauling's various lectures on the chemical bond?

RASETTI: Yes, yes.

GOODSTEIN: Now, was it well appreciated how significant that work was?

RASETTI: I don't know. I thought it very interesting, but I don't know how well it was appreciated in general.

GOODSTEIN: It's always easy, after the fact, to look back and say, "This was very important, and everyone understood that." But I was wondering if you, at the time, picked up any such feeling.

RASETTI: No, not really. I had no feeling for it.

GOODSTEIN: When you were at Caltech, did you attend any classes? Did you follow anyone's lectures?

RASETTI: No, except for Heisenberg's and Sommerfeld's lectures.

GOODSTEIN: Do you remember when you first met Glenn Millikan?

RASETTI: Oh, yes. I wanted to talk to you about Glenn Millikan.[9] We became very good friends.

GOODSTEIN: Did you meet him at Caltech?

[9] On Glenn Millikan see G. B. B. M. Sutherland, ''Glenn Allan Millikan, 1906–1947,'' *Cambridge Review*, November 22, 1947.

RASETTI: Yes, and we immediately became friends. We both liked to go traveling in the desert. I remember a trip to Death Valley, the Panamint Mountains, all those deserts—Arizona, Phoenix, Tucson, the Superstition Mountains, all sorts of places. Whenever we had a vacation, I went around with Glenn Millikan or Louis Henry.

Henry was a Belgian; he was in physics but I don't know what he was doing. I remember only that [R. A.] Millikan once told me, "Ah, these fellows that the Belgian foundation sends us are never worth much." I don't recall on what he was working, but I remember this remark of Millikan's.

In Europe I went skiing with Glenn Millikan twice, which must have been in the winter of '32 and again in '33, in Switzerland. And we also once went on a driving trip through parts of England; I remember going through Bath and Stoke-on-Trent. We went in his car, in which he had built a very original piece of equipment for taking a shower when he was camping. He had a big water tank that was heated by the engine, and then he could set up a shower on a pole and stand underneath and take a shower. He demonstrated how it worked—not taking a shower, of course—in the center of London. [Laughter] It worked beautifully. But he didn't use it on this trip, because we were staying in places—we were not camping. With us that time was an American couple, whose name I've forgotten; they were biologists.

Fig. 5. Franco Rasetti, Swiss Alps, 1932, photograph by Glenn A. Millikan. Caltech Archives.

We were both very fond of skiing. On one of those ski trips, we were based in Andermatt. We made some very, very long and tough climbs. We climbed Pizzo Centrale, which was twelve hours at least of skiing. At that time, of course, these ski lifts and so on were unheard of. You climbed for about

five or six hours, and went down in twenty minutes. [Laughter] But I remember the Oberalp Pass was one excursion, and another was this Pizzo Centrale. And then, in a different season, we were together in Zermatt. We climbed a peak of 3,800 meters on skis. That was also quite a climb. Oh, we were very good friends! We liked doing the same things. He died very young, unfortunately, you know, in an accident in Virginia, just practicing rock climbing. It's a shame.

GOODSTEIN: He was different from his father.

RASETTI: Yes, completely different. His father was an extremely conservative Republican—an admirer of Herbert Hoover. Glenn Millikan had a much more open mind.

GOODSTEIN: Did he ever talk about his father?

RASETTI: No, I cannot remember ever having discussed his father with him. I knew Clark Millikan,[10] of course, also, but not as well as Glenn.

GOODSTEIN: What was Clark Millikan's personality like? Was it closer to the father's?

RASETTI: No, I would say intermediate. Glenn was a little more bohemian, let's say, and Clark was not as stiff a Republican or as conservative as his father, but halfway, perhaps. But of course I knew Clark much less than I knew Glenn.

GOODSTEIN: So the skiing trips with Glenn in the thirties were when he was at Cambridge?

RASETTI: Yes, when he was at Cambridge.

GOODSTEIN: Why was [R. A.] Millikan trying to get you to go to China?

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[10] On Clark Millikan see E. E. Sechler, ''Clark Blanchard Millikan, 1903–1966,'' *Biographical Memoirs of the National Academy of Sciences* **40** (1969), 211–225.

RASETTI: Well, Millikan had generally a rather poor opinion of Europe, especially with regard to the political situation—perhaps excepting England, of which he had a very high opinion. He insisted on sending me to China, finding a job for me in China, which I thought was crazy. Because that was just the worst period of the civil war between the Kuomintang and the warlords. So I didn't understand how there could be more stability in China than in European countries.

GOODSTEIN: Did Caltech have any foreign students from China at that time?

RASETTI: I don't remember any Chinese students at Caltech. In Chicago, there were [C. N.] Yang and [T. D.] Lee. The only Chinese with whom I made friends in a physics department was a Chinese that was working in Hahn and Meitner's laboratory at the Kaiser Wilhelm Institute in Berlin. And I read later in the newspapers that he was one of those who worked on atomic energy projects in China after the war. I remember that Lise Meitner always used to scold him, because he was always accused of contaminating the laboratory with radioactivity. Lise Meitner was extremely strict in this respect; and she was perfectly right in doing so, because work would become impossible if the laboratory became contaminated with radioactivity. So we always had Geiger counters to monitor the activity. And I don't know whether rightly or wrongly, but this poor fellow was always accused of contaminating.

GOODSTEIN: Were you as careful when you came back to Rome, when you were working with radioactive materials?

RASETTI: Yes, yes, we were. But I believe that the total amount of radioactivity that we absorbed in our bodies—the two years' work with neutrons in Rome—was certainly at least a hundred times or perhaps more than what is considered safe by present-day standards. And yet we didn't seem to suffer any harm. I believe that the official standards are far beyond what is really necessary.

GOODSTEIN: Do you think Fermi suffered, though?

RASETTI: No, no. I don't believe at all that Fermi's cancer was tied to radioactivity.

GOODSTEIN: None of the rest of you got it.

RASETTI: We used to hold a source of between 100 and 200, perhaps even 300, millicuries of radon at the end of a Pyrex glass rod about 50 centimeters long, and of course holding it so it would be as far as possible from our bodies. But we did that several times a day for a couple of years.

GOODSTEIN: Did you continue to work on the Raman effect in Rome, after you got back from Caltech?

RASETTI: Yes, I certainly did, immediately. The first thing I did was to set up and repeat the experiment that I had done in Pasadena, with improved apparatus—with a spectrograph with a higher resolution—and I obtained much better results and some new results. And then, of course, others also continued these experiments while I was in the States—while I was at Columbia [1936], or before, perhaps: [Edoardo] Amaldi, who was one of Fermi's students in Rome, and George Placzek. Actually, Placzek was the one who made the more general theory on the Raman effect in polyatomic molecules. He published a paper that was fundamental.[11]

GOODSTEIN: So this really was a continuation of the work you began in Pasadena. Was Fermi interested in your experiences at Caltech?

RASETTI: Yes, very much.

GOODSTEIN: Were there certain things he wanted to know in particular?

[11] George Placzek, ''Rayleigh-Streuung und Raman-Effekt,'' *Handbuch der Radiologie* **6** (1934), Pt II, 205–374.

RASETTI: Well, he was especially interested because, as I said, I repeated all the experiments and perfected them, extended them, in Rome—and Fermi was following them very closely, of course.

GOODSTEIN: Was he at all interested in the organization of physics at Caltech?

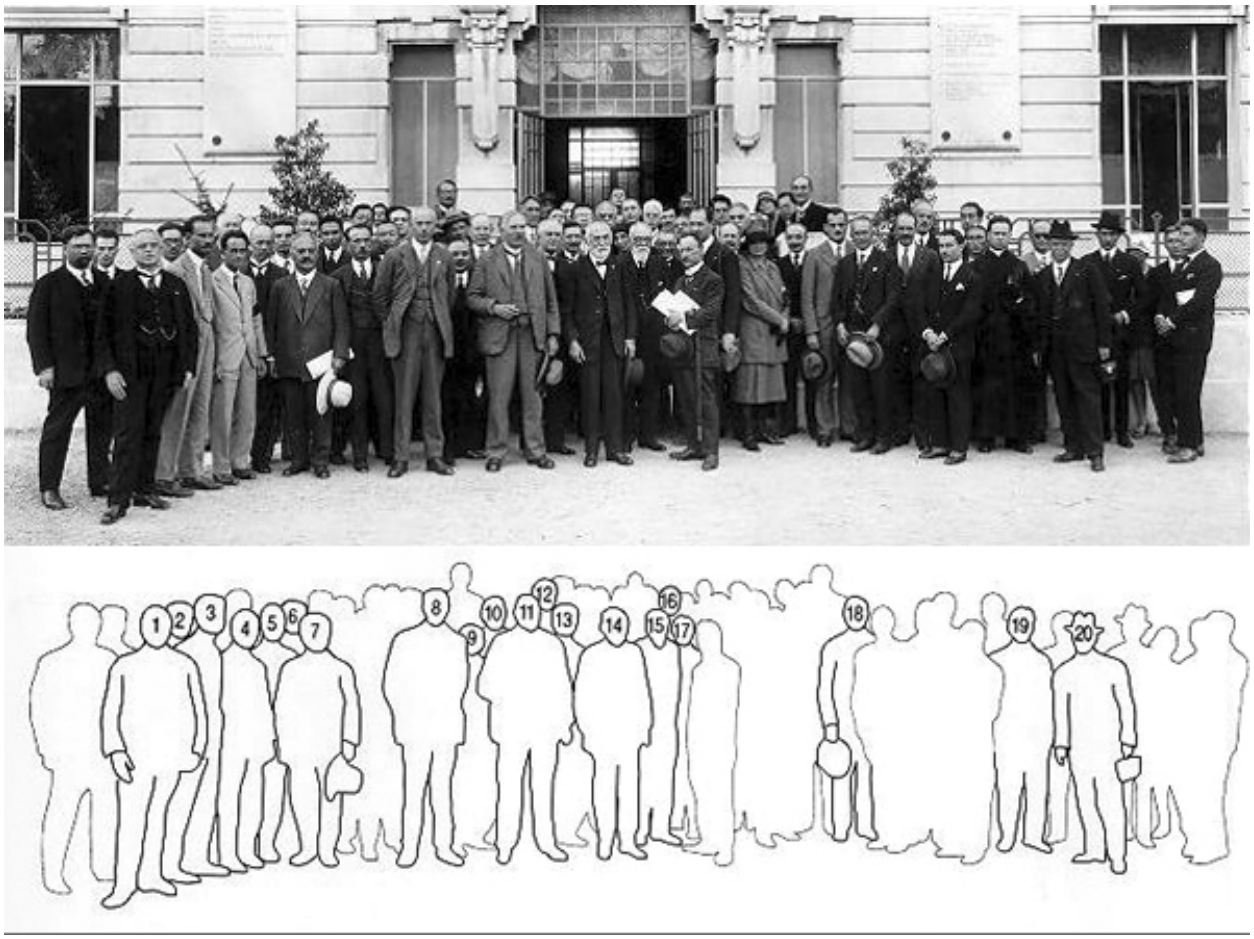

Fig. 6. Volta Physics Conference, Como, Italy, September 1927. Caltech Archives. The participants included: 1, Giovanni Giorgi; 2, Emilio Segrè; 3, Franco Rasetti; 4, Enrico Fermi; 5, Owen W. Richardson; 6, Theodore von Kármán; 7, Arnold Sommerfeld; 8, Francis W. Aston; 9, Orso M. Corbino; 10, Edwin H. Hall; 11, Ernest Rutherford; 12, Robert W. Wood; 13, Robert A. Millikan; 14, Hendrik A. Lorentz; 15, Marcel Brillouin; 16, Pieter Zeeman; 17, Tullio Levi-Civita; 18, Arthur H. Compton; 19, Giuseppe Gianfranceschi; 20, Quirino Majorana. A historian once asked Rasetti if the Como conference had a direct impact on the development of Italian science. Rasetti replied: ''Enormous, because it demonstrated to all the Italian people in the universities how important Fermi was. It was absolutely the revelation of Fermi to the Italians.'' Quoted in ''Franco Rasetti and Enrico Persico,'' interview by Thomas Kuhn, April 8, 1963, p. 2, Archives for History of Quantum Physics, Office for History of Science and Technology, University of California, Berkeley.

RASETTI: Well, not particularly. You know, Fermi was a theoretician, and theoreticians can do everything with a piece of paper and a pencil, or with a blackboard and chalk.

Then, of course, Fermi and I had worked on experiments also in Florence, because we were together in Florence for two years, in ’25 and ’26. We were all day together then, because the physics building was, as I mentioned before, on a hill out of town. We spent practically all day together, and he taught me a lot of theoretical physics.

GOODSTEIN: Did you teach him a lot of experimental techniques?

RASETTI: Yes.

GOODSTEIN: You’ve been described as being a very elegant experimentalist. When you were at Caltech, how did you find the other experimentalists?

RASETTI: I can’t tell you very much, because, as I said, I worked on the Raman effect and everybody else worked on other things. I was the only one working on the Raman effect. In fact, I continued that kind of work until the year I spent with Hahn and Meitner in Dahlem, and then I introduced the nuclear techniques in Rome.

GOODSTEIN: At least one historian has suggested that the chair of spectroscopy, which you got in 1930 was never intended to be used to do spectroscopy.[12] Is there any truth to this suggestion? The inference is that [Orso Mario] Corbino knew that Fermi was going to take his group into nuclear physics, but he [pressed for] a chair of spectroscopy anyway, even though he knew there would be no more spectroscopy work.

RASETTI: No, no. The work in nuclear physics really started in Rome after I came back from Dahlem, which means that it was in the fall of ’32. Until then, all the work was in spectroscopy: the Raman effect and also the experimental and theoretical work of Fermi on the states of atoms

[12] Gerald Holton, ‘‘Striking Gold in Science: Fermi’s Group and the Recapture of Italy’s Place in Physics,’’ *Minerva* **12** (1974), 159–198, on p. 180.

with very high quantum numbers—that is, with extremely large orbits. No, really, until I got back from Dahlem there was nothing done in nuclear physics—not even talk about switching to nuclear physics.

GOODSTEIN: There wasn't talk before that?

RASETTI: Well, there was very vague talk, yes, but new apparatus wasn't built until I got back from Dahlem.

GOODSTEIN: When you got back, was there then a discussion about what direction the group should take?

RASETTI: Yes, yes, we all agreed. Well, not all. [Emilio] Segrè wanted to continue with spectroscopy. He was against going in for nuclear physics.

GOODSTEIN: Did he have good reasons?

RASETTI: Well, I don't know. He was satisfied with spectroscopy, and he said we should continue in spectroscopy. But Fermi and I were both for shifting our interest. We were both convinced that spectroscopy was—not exhausted, but at least that the fundamental things had already been done and there was nothing startlingly new to come out of it anymore.

GOODSTEIN: When did Placzek do his work?

RASETTI: I remember that Placzek and [Hans] Bethe were both in Rome at the same time [1932]. And Bethe wrote his famous one- and two-electron systems article for the *Handbuch der Physik*. And Placzek was writing on the theory of the Raman effect in the most general form of molecules, for the *Handbuch der Radiologie*. And they were sitting side by side, each one at his own desk. And Bethe sat up stiff like this and was writing without stopping, without erasing one line. He wrote the whole monograph—such a thick book!—like that, without one erasure, practically. And Placzek was sitting next to him, and he was writing a page and then crumpling

it, throwing it into the wastepaper basket, then rewriting. [Laughter] And he was mad that Bethe could do that work without interruption, while he had to write it ten times before it pleased him.

GOODSTEIN: Did Fermi have that technique of being able to write almost perfectly?

RASETTI: Almost perfectly, but not quite as much as Bethe. I have never seen anybody write papers like Bethe—hundreds of pages without changing one line.

GOODSTEIN: All formed in the head, before he started.

RASETTI: Yes.

GOODSTEIN: Would you mind if I asked you some questions about Italian mathematicians?

RASETTI: No, I certainly don't mind.

GOODSTEIN: I was interested, for example, in a description—I think it's Laura Fermi who gives the description—of the mathematicians and the physicists who used to meet at [Guido] Castelnuovo's house, and that [Vito] Volterra used to come there.[13]

RASETTI: Volterra? No, no. [Tullio] Levi-Civita, but not Volterra. I never met him outside the university.

GOODSTEIN: Is it because he was a different kind of person, that he did not come to these receptions?

RASETTI: I don't know. Really, I know very little about him.

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[13] Laura Fermi, *Atoms in the Family*: *My Life with Enrico Fermi* (Chicago: University of Chicago Press, 1954), p. 42.

Fig. 7. Vito Volterra (1860-1940), world-renowned mathematician, who developed powerful mathematical language and theories that affected everything from physics and applied mathematics to biology and economics. Courtesy of the Bancroft Library, University of California, Berkeley.

GOODSTEIN: But he did not take part in these evenings and afternoons of conversation. By choice—I assume that he was invited.

RASETTI: Well, he certainly would have been welcome.

GOODSTEIN: Did he take an interest in Fermi's physics group?

RASETTI: No.

GOODSTEIN: He must have known about it.

RASETTI: He must have known about it. But, you see, his interests were so far from physics.

GOODSTEIN: And yet he was more than just a mathematician. That is, he was a politician; he had a certain stature in the country.

RASETTI: Yes. Castelnuovo was quite a different man; he was much more sociable. Although in mathematics he was farther from physics than Volterra's work was.

GOODSTEIN: Right, and yet he seems to have had more interest in the physics.

RASETTI: Yes—or at least if not in the physics, then in the physicists. He was very friendly with Fermi and with me.

GOODSTEIN: Volterra was not?

RASETTI: No, no. Volterra was not so approachable. He was a very kind man, but we were not familiar with him.

GOODSTEIN: I think I read someplace, though, that it was his letter of recommendation that succeeded in getting Fermi a fellowship.

RASETTI: Yes.

GOODSTEIN: To put it another way, would it have made a difference to the physics then, if Volterra had been interested in the physicists?

RASETTI: I don't think it would have made much difference. His fields of mathematics—integral equations, functionals—were not used in our fields of physics.

GOODSTEIN: Yet he played a role in keeping the Italian Physical Society alive when there weren't too many physicists in Italy. So that he seems to have cared to a certain extent.

RASETTI: Yes, it was a fact that before Fermi there was practically no physics in Italy, whereas mathematics was at a high level.

GOODSTEIN: I think Levi-Civita took more of an interest.

RASETTI: Well, of course, he developed the kind of mathematics—differential calculus—that is useful in general relativity. He and [Gregorio] Ricci-Curbastro developed the tensor calculus.

GOODSTEIN: After coming under the influence of Fermi, did you ever give any thought to becoming a theoretician instead?

RASETTI: No. I definitely do not have the power of abstraction to be a good theoretician—and much less a mathematician. No, I have always been good at working with my hands. As Laura Fermi says in her book, I was already very good at working with my hands when I was ten or twelve years old and preparing microscopic insects. I knew hundreds of Latin names of insects when I was six years old.

GOODSTEIN: That's interesting. You knew that before you knew any physics.

RASETTI: I knew nothing about physics. Now, strangely enough, before going to the university—while I was still in high school—I understood thermodynamics, which is strange, because it's a very abstract field. And I understood the concept of entropy. But, for instance, I knew nothing about even the most elementary notions of electricity and magnetism. For some strange reason I knew chemistry very well. In fact, between the ages of fifteen and sixteen I learned so much, both in inorganic and organic chemistry, that just from what I remembered, at the age of twenty or twenty-one, I passed the exams in inorganic and organic chemistry. I passed with the highest marks in both.

GOODSTEIN: Was your chemistry self-taught?

RASETTI: Just self-taught. I had two books by [A. F.] Holleman—translated from the German. I knew all about the most complicated organic compounds. And all I knew of physics was thermodynamics. [Laughter] Teaching thermodynamics, for instance, is much harder than teaching mechanics or teaching electricity and magnetism or teaching optics, but somehow I knew thermodynamics.

GOODSTEIN: So that when you went to the University of Pisa—

RASETTI: I registered in engineering, because I never dreamed I could ever become a physicist. My interests had all been in zoology and botany. I registered in engineering because my father thought that there would be no good jobs for zoologists or botanists, and that it would be better for me to be an engineer. Since the first two years were common to physics and engineering and other things, that meant that I could still decide at the beginning of the third year. And I don't think I would have shifted to physics if I hadn't met Fermi. We became friends from the first day—from the first year at the university. So we had to get along with general courses.

GOODSTEIN: Did you become a spectroscopist because of [Luigi] Puccianti?

RASETTI: Well, yes, probably. Puccianti knew spectroscopy, but he didn't know anything about the quantum theory or about relativity. And he used to say to Fermi, "Now, please, give us a lecture on something." And Fermi would give a little course in relativity. Anybody could learn special relativity in a few days, but general relativity is quite another business, because general relativity requires difficult mathematics. And relativity, at that time, was considered for physicists very difficult mathematics. So it's amazing that Fermi at nineteen published an original paper on general relativity.

GOODSTEIN: Was he at Pisa at this time?[14]

RASETTI: Yes, at Pisa.

GOODSTEIN: He did this without any encouragement from Puccianti?

RASETTI: He did it by just learning, by reading the theory from Einstein's papers.

GOODSTEIN: Now, did Puccianti try and read that paper?

RASETTI: No, no. He couldn't have understood it—nor could I. No, there were very few people who could understand general relativity in those years—few people anywhere. Puccianti knew the techniques of spectroscopy, knew the classical results of spectroscopy.

Certainly I might have become a physicist without knowing Fermi. But it's more likely that I decided because I was impressed by Fermi. Well, he was really an extraordinary student, who at the age of nineteen knew more than all of his professors.

GOODSTEIN: Puccianti didn't mind this, either?

RASETTI: No, no. He was an extraordinary person, because he didn't mind at all Fermi knowing more physics than he did. He admitted it openly and asked Fermi to give lectures. Fermi gave us private talks—to me and [Nello] Carrara, who was the third student that year in physics. So Fermi gave courses on relativity, and especially on quantum theory. He explained the Bohr atom—nobody in Italy knew anything about it.

GOODSTEIN: What did Puccianti do, if he didn't do physics anymore?

[14] E. Fermi, ''Sull'elettrostatica di un campo gravitazionale uniforme e sul peso delle masse electromagnetiche,'' *Nuovo Cimento* **22** (1921), 176–188.

RASETTI: I don't know. He read. He should have been in the humanities; he knew more about literature and poetry than physics. He was, I think, misplaced as a physicist. It was just laziness, because he certainly was intelligent enough; but he just didn't read *Annalen der Physik.* All physics came from Germany, or countries like Holland, Denmark, more or less of the German culture. And the founders of the quantum theory were either German or from Switzerland, Denmark, or Holland.

GOODSTEIN: Did you live in a boarding house when you were in Pisa?

RASETTI: Oh, no. My family was in Pisa.

GOODSTEIN: So you just went home at night.

RASETTI: I went home during the day, for lunch. There is no concept in Italy of not going home for lunch; from 12:30 to 2:30 one doesn't do anything, one goes home to eat. Fermi ate at the Scuola Normale, at a place where they gave meals, but they were so bad that he very often came to have lunch or dinner with us. And my mother gave him a good meal.

GOODSTEIN: Then you went to Florence.

RASETTI: My first job was in Florence, and I worked on atomic spectroscopy. The physics building was on a hill near where Galileo lived the last years of his life—in the Arcetri hills. The physics building had been built there and was extremely inconvenient for students, because all the other courses were given in various places in town and this was three kilometers out of town, and also at least 150 meters above the city level. So one had to take a streetcar, and after that it was still a fairly long walk to get up there. And on this Arcetri hill, the university owned a fairly large estate, on which the physics building was built, very beautiful architecturally but very impractical—the most impractical place because it was impossible to heat. It was built like an abbey: it was a rectangular building with a vast garden and lawn in the middle. It had a ground floor and a first floor, and on each of these a corridor ran all around this central square, and there was only one row of laboratories. So there was a tremendous surface exposed to the

outside, and it would have been impossible to heat this building. It had a heating plant, which was never operated, so the temperature of the building was whatever the atmospheric conditions produced. [Laughter] And in January and February, the average temperature was between three and five degrees centigrade. Now, on top of the hill was the astrophysical observatory, with a sun tower with a mirror for observing the sun that was an exact copy of the one in Pasadena.

GOODSTEIN: That's right, because [Giorgio] Abetti worked in Pasadena.

RASETTI: Yes, Abetti had been at Caltech and studied with Hale, and he came back and built an exact copy of the sun tower at the astrophysical observatory in Pasadena.

Fig. 8. Antonio G. Garbasso (1871-1933). *Source*: A. G. Garbasso, *Scienza e Poesia* (Florence: Felice le Monnier, 1934)

And at the side of the physics building was a little building that was completely unoccupied and unfurnished. There, I had only one room, furnished with a bed and a little oil stove to keep warm, so to speak, to some extent. At least it raised the temperature some five or six degrees above the outdoor temperature at night. And I lived alone there, with only the company of some scorpions. [Laughter] If you had something hanging on the wall, like a picture, when you turned it, behind it was a scorpion and a few spiders. So I lived there for two years. The third year I was there, Fermi came [at the end of 1924], and we kept company for two years. But in these two years he took my room in this little building, which later became the first building of the National Institute of

Optics. My father had died in ’24 and so my mother came to live in Florence. We bought an apartment in town, and I lived with my mother there. So every day I went up the hill, stayed there for lunch, but went back at night and had dinner at home.

GOODSTEIN: Did you like working in Florence?

RASETTI: Yes, it was very pleasant. The equipment was pretty good for those times—especially for spectroscopy, which was my field. They had a very good spectrograph and spectroscope; we had an excellent Rowland grating in the Rowland mounting. And I didn’t have much teaching to do, because [Antonio] Garbasso gave the physics course.

GOODSTEIN: How good a physicist was he?

RASETTI: Garbasso had been a good physicist, but when I knew him he was only interested in politics. He was the mayor of Florence. He gave his course in elementary physics and he was quite intelligent at it. And later Fermi explained to him what we were doing and he understood, because he was intelligent. I mean, he knew the classical theory—he didn’t know much about the quantum theory, because that had come after he lost direct interest in physics. But he followed what we were doing, and he was a very pleasant person. Like Corbino, he died at a relatively young age.

GOODSTEIN: Was he responsible for building up the physics department in Florence?

RASETTI: Well, I really don’t know, because that happened before my time. Who decided on the location of this physics building I’m not sure. Perhaps it was Garbasso. But I’m not sure, because it was several years before I went to work there.

GOODSTEIN: Was Garbasso a very outspoken Fascist?

RASETTI: No. He was very moderate, in fact. He was supposedly a Fascist, but I don't even know if he belonged to the party. Certainly he was very moderate, and I'm sure that, had he lived longer, he would have become disgusted with Fascism. But you know, in the first few years, in 1922, Fascism didn't seem very bad. In fact, a large class of Italians welcomed it, because the Communists were very powerful and practically disorganized all industrial production, disorganized the railway traffic. So at that time Mussolini seemed a fairly reasonable dictator. The first act that really disgusted the more reasonable people was the [Giacomo] Matteotti murder, which happened in 1924. But in 1922 it still looked as if it would become a reasonable dictatorship; after 1924, people lost hope in that. Still, even in the States, there was a lot of admiration for Mussolini.

GOODSTEIN: When you came to the United States, did you discover that?

RASETTI: Yes. There were perhaps more admirers of Mussolini in the States than in Italy. For instance, when [Italo] Balbo made his flight with those seaplanes from Italy to New York, there was a tremendous enthusiasm in the States for Mussolini.

GOODSTEIN: Maybe it was seen that this was what you could do under Fascism. Did you ever pick up any of that enthusiasm at Caltech?

RASETTI: No. No, I don't think at Caltech there was much interest, I would say, one way or the other. Events in Italy did not seem of sufficient importance. I remember one strongly anti-Fascist Italian—[Gaetano] Salvemini. He came to Caltech and gave a talk that I listened to. He was against Fascism, but I found him very reasonable. I don't know what impression he made; I sympathized with him.

GOODSTEIN: I didn't know he came to Caltech.

RASETTI: Yes, he talked at Caltech. I don't remember exactly what the subject was, but it must have been political. I didn't know him before that, and this was the only time I met him. I don't know what happened to him; he probably emigrated.

GOODSTEIN: So, after your years in Florence, what made you leave that city?

RASETTI: Oh, that I had a better job in Rome.

GOODSTEIN: Was that due to Fermi?

RASETTI: Yes, of course. Fermi talked Corbino into giving me a job in Rome. And also, of course, there was the fact of being again with Fermi.

GOODSTEIN: Was Corbino able to do most of the things that Fermi wanted? Was he able to get a lot of appointments?

RASETTI: Well, yes. You know, the departments in Italy were very small. There were very few positions in each department. But he could fill all the available positions with good people, like Segrè and Amaldi.

GOODSTEIN: How conversant was Corbino with quantum mechanics and relativity?

RASETTI: Not much.

GOODSTEIN: Then he depended on Fermi, basically, for his information?

RASETTI: Yes, yes. No, I don't think he knew quantum mechanics. Relativity, perhaps; I think Corbino knew special relativity, because special relativity came much earlier. Corbino was still in his active days when special relativity was discovered by Einstein, but he was twenty years older at the beginning of quantum theory.

GOODSTEIN: Why did Corbino stop being an active physicist?

RASETTI: That I don't know.

GOODSTEIN: When you came to the University of Rome, did Corbino do any teaching?

RASETTI: Oh, he always did; until his death in 1937, he was teaching. He always taught the general physics course.

GOODSTEIN: Was he a good lecturer?

RASETTI: Yes, an excellent lecturer, and he would do demonstrations and experiments in his lectures. And he took much care in preparing his lectures. He was a first-class lecturer, and he always taught general physics. He never taught the advanced courses, although if he had wanted to he could have.

GOODSTEIN: Which courses did you teach?

RASETTI: In Rome I taught spectroscopy, quantum mechanics. I never taught the general physics course for the first- and second-year students.

GOODSTEIN: Did you ever hear about Fermi's memo, in which he tried to get a separate physics institute set up?

RASETTI: No, what is that?

GOODSTEIN: Well, in 1931 he tried to have a separate physics institute set up. And in this he was unsuccessful and was turned down. And I was wondering if you'd heard anything about that at the time.

RASETTI: No. You said when?

GOODSTEIN: In 1931. He wanted something separate from the university—at least not part of the university in a way that the physics department was organized at Rome.

RASETTI: That surprises me, because I never heard it either from Fermi or from anybody else.

GOODSTEIN: His petition was turned down by the minister of education, who said that it was a very bad precedent if they started a physics institute. Then the people in chemistry would want their chemistry institute, and soon enough, you'd have no university left, but many institutes.

RASETTI: All this is new to me. It is strange, because we more or less discussed everything that concerned physics, with Fermi. But this is all new to me.

GOODSTEIN: Let me ask you something else. I have been studying the Academy of Italy; and I know that in the first years when Fermi was a member, he was a member of a committee that sat to decide prizes.[15] And I read the transcript. And I know that Fermi had tried to get one of the famous mathematicians selected, and he was overturned on that. And finally Giuseppe Levi, the biologist, was selected by the committee. And when his name was put forward, Mussolini said, "Never." And so, over the vote of the committee, somebody else was chosen. Did you ever hear about that?

RASETTI: No. What was this committee?

GOODSTEIN: It was a committee within the Academy, set up to decide who should receive the first Mussolini Prize in science. Fermi was the only physicist on this particular committee. When the committee's decision was sent to Mussolini, Mussolini overrode the committee's decision, because Levi was an anti-Fascist. Somebody else was chosen—an explorer by the name of Filippo de Filippi—to receive the science prize. So I was wondering if any of this ever leaked out.

[15] On mathematics during this era see J. R. *Goodstein*, ''The Rise and Fall of Vito Volterra's World,'' *J. History of Ideas* **45** (1984), 607–618

RASETTI: I didn't know about this. Because, you know, Fermi was extremely careful about talking about anything that was not supposed to be talked about. He was extremely cautious. He never told us the background of decisions in the Academy.

GOODSTEIN: I was just wondering if he ever did discuss these things.

RASETTI: No, there has never been a person more discreet than Fermi. And he would never tell what was discussed in the Academy of Italy. I received the Mussolini Prize several years later. Anyway, when the turn came to give it to a physicist, I received it, in '37.

GOODSTEIN: That's right. The year before that, I think, it went to Corbino.

RASETTI: I don't remember that; I confess I don't remember to whom the various Mussolini Prizes went. That Giuseppe Levi was anti-Fascist, there is no question [laughter]; he couldn't be more so. I practically grew up with that family.[16]

GOODSTEIN: How did you know them?

RASETTI: My uncle—my mother's brother—was a professor of pathology and had been a great friend of Levi's since the university days; they had studied together. So I got to know the Levi family when I was, I think, six years old in Florence, because my grandparents lived in Florence. And so when I was six years old, I was a friend of Levi's eldest son, Gino. We were both six years old, and we kept on seeing each other as we grew up. We climbed mountains in the Alps, from the age of sixteen to twenty-eight, perhaps; we climbed all the highest peaks of the Alps together. We still see each other once in a while. He lives in Ivrea now; we spent a day with them at their home in Ivrea last April. I knew less about the other members of the Levi family. The second child, Paola, married Adriano Olivetti, who was the son of the founder of the

[16] On Giuseppe Levi see Natalia Ginzburg, *Family Sayings*, trans. D. M. Low (New York: Dutton, 1967), and Rita Levi-Montalcini, *In Praise of Imperfection* : *My Life and Work*, trans. L. Attardi (New York: Basic Books, 1988).

typewriter company. I also knew the younger ones, but I was less close to them, because of the difference in age.

GOODSTEIN: Did you ever argue politics with Giuseppe Levi?

RASETTI: No, no. I have hardly ever argued about politics with anybody. I knew that he was strongly anti-Fascist, and he knew that I was mildly anti-Fascist—at least up to a certain time. I never liked to discuss politics. In general, I never felt very strongly about controversial subjects. I always preferred to talk about things like physics or the natural sciences, on which there can be no different opinions.

GOODSTEIN: Do you think that's true of physicists in general?

RASETTI: That's true of physicists. I knew very few physicists who felt strongly about political issues.

GOODSTEIN: Fermi was that way, too?

RASETTI: His position and mine were exactly alike. And so were those of most physicists and most other scientists I have known—except those, of course, in Germany who were persecuted. But many scientists were politically more or less indifferent, unless they were really strongly affected in their own personal lives. For instance, I have known most of the important physicists, and I think most of them felt more or less that way. I don't know, perhaps Heisenberg and [Erwin] Schrödinger.... Anyway, when we got together, we never discussed political subjects. For instance, when I was in Berlin, Hahn and Meitner and I did not talk about politics.

GOODSTEIN: Were you aware of the political things going on?

RASETTI: Yes, of course we were aware. Actually, I have always been aware, way in advance, of what was going to happen.

GOODSTEIN: But was it a convention, or just—

RASETTI: No, it just came naturally to us—just to stick to our own sciences and not meddle in other things.

GOODSTEIN: Were you very surprised when Ettore Majorana disappeared [1938]?

RASETTI: Yes, absolutely. That is a mystery, and I don't think there exists a person, living or dead, who knew what that was all about. How he disappeared, and why, still remain a mystery. Some people say that he landed in Sicily; some say that he stayed on the ship and came back to Naples. Even now his disappearance is controversial. The writer [Leonardo Sciascia] who thinks that Majorana disappeared for political reasons—I think he's crazy. I don't think Majorana had any strong political opinions. At least if he had, he never expressed them. Just like Fermi, like myself, we never talked about politics in our department. And Majorana never did.

Fig. 9. Ettore Majorana (1906–1938), mathematical prodigy and theoretical physicist, nicknamed by Fermi's group the "Grand Inquisitor," disappeared in March 1938 while on a boat trip from Palermo to Naples. Rasetti was known as the "Cardinal Vicar." Picture copyright of Erasmo Recami and E. Majoana Jr., a reference copy is held by the American Institute of Physics Emilio Segrè Visual Archives, Erasmo Recami and E. Majorana Jr. Photograph Collection.

GOODSTEIN: What about [Bruno] Pontecorvo?

RASETTI: Ah, that's another mystery. I don't know. We were very good friends. Perhaps you don't know that Pontecorvo was for a year my assistant, when I was chairman of the physics department at Laval, in Canada.

GOODSTEIN: You brought him from Rome?

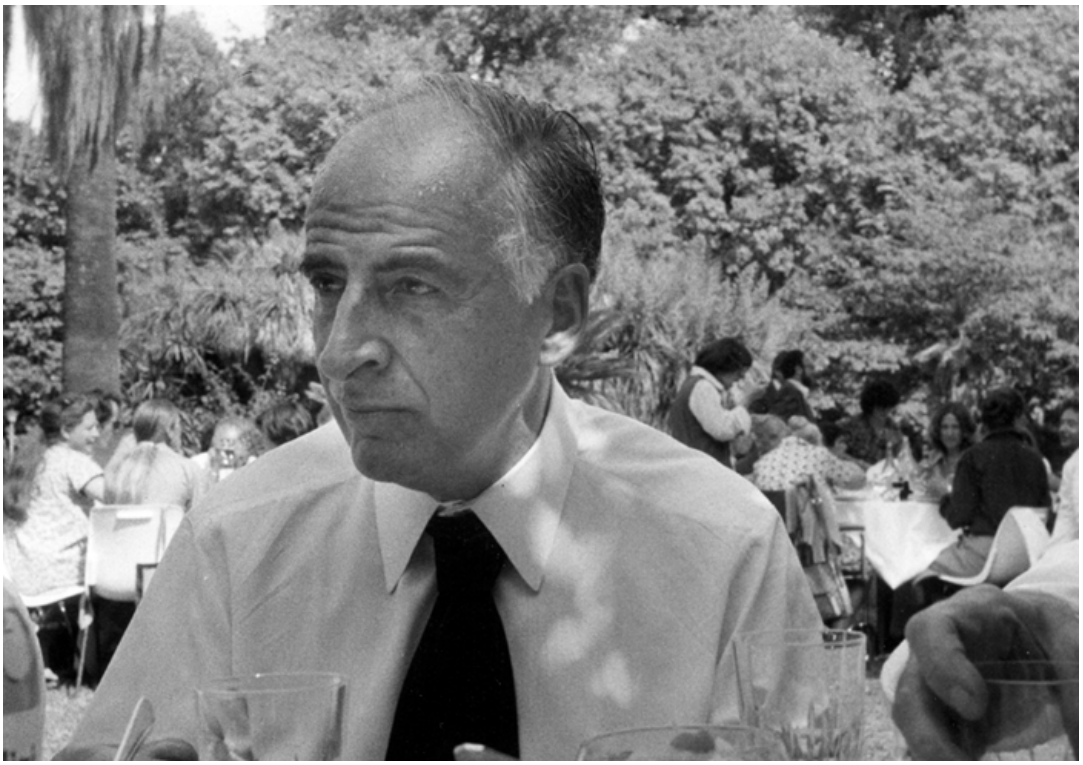

Fig. 10. Bruno Pontcorvo (1913-1993) switched from engineering at the University of Pisa to physics at Rome, graduated in 1934, and then joined Fermi's research group. Courtesy of the American Institute of Physics Emilio Segrè Visual Archives.

RASETTI: No, from the United States; he was working for an oil company. He had emigrated to the States—I can't tell you exactly what year. But after my second year at Laval, which would be in 1940, he came to Canada, and they—the university authorities, the chancellor, the dean of the faculty, I mean—they were extremely pleased with him. He made good friends with my young collaborators. Then, when he saw that Italy was close to entering the war, he left and went to Montreal, and went to work for the English group that was working on a sort of mini-Manhattan Project. For security reasons—England was under steady bombing—all of the English group had been moved to Montreal. And Bruno Pontecorvo left Laval—which we all

regretted, because we all liked him very much—and went to work for this group. And I never saw him afterwards. Because then, after the end of the war, he went to England to a place called—

GOODSTEIN: Harwell?

RASETTI: Harwell. And then one day from Harwell he disappeared to Scandinavia, and then to Russia.

GOODSTEIN: And that surprised you?

RASETTI: That surprised me, because—well, we never talked about politics. I knew he was anti-Fascist—but, more or less, we all were. But when he was in Canada, he never expressed any political views. People liked him very much at Laval. I knew his wife and his first child. I remember I used to tease his first child when he was, I think, four. We were together in the summer in the Gaspé Peninsula, and I used to tease his child—I told him that when children were five, they had to change their first name. [Laughter] And he got mad because of that. So I invented a new name for him.

The Pontecorvos—there were eight of them, eight brothers and sisters. The eldest, Guido, became a very famous geneticist—a fellow of the Royal Society and lives in London. Paul was the second. He had a career in the States; he was for many, many years with the Raytheon Company. Another of the young physicists in Rome was Eugenio Fubini. They were in Rome at the same time—Bruno Pontecorvo and Eugenio Fubini.

GOODSTEIN: Did you know both of the Fubini brothers?

RASETTI: No, I only knew Eugenio Fubini. Eugenio Fubini was the son of the mathematician [Guido Fubini]. He was an assistant US secretary of defense in the 1960s. And then he was a vice president of the IBM Corporation.

GOODSTEIN: He's now an advisor to the Jet Propulsion Laboratory.

RASETTI: Oh, he is?

GOODSTEIN: He is one of their outside advisors.

RASETTI: Oh. I had completely lost track of him.

GOODSTEIN: He certainly became well connected politically in the United States. Was he very political in Italy?

RASETTI: No. We used to go skiing together. He was called Il Fubinetto, which means "little Fubini," because he was short. Usually Amaldi invented these names. Ugo Fano was Il Fanaccio. [Laughter] Now, Ugo Fano was another of the boys in our group; he, too, was the son of a mathematician. We used to see each other a lot when he was in Washington at the Bureau of Standards. We saw the Fanos quite often, but then we lost contact with them.

GOODSTEIN: It's interesting, I've noticed that several of the sons of Italian mathematicians seldom chose mathematics if they went into science. They went into physics.

RASETTI: Well, that is a fact. I don't know if the number of examples is sufficient to establish a rule, because there are only two or three cases, but anyway, it so happened. Physics certainly was an attractive science in those days.

GOODSTEIN: Did you know Mauro Picone?

RASETTI: I never thought much of him. I didn't know him really well, but he didn't impress me at all as a particularly intelligent person.

GOODSTEIN: I don't think he was, but he had political influence. He was able to reach Mussolini and get money for his kind of applied mathematics. The older generation of mathematicians had no political influence with Mussolini.

RASETTI: No, because they were all anti-Fascist. But Fermi and I and our group didn't think much of Picone.

GOODSTEIN: No, I don't think he was a good mathematician. The only other mathematician who did very well under Fascism was [Francesco] Severi.

RASETTI: I told you about how vain he was. He wanted to ship me—I can never remember whether it was Severi or another, [P. G.] Bordoni—who wanted to ship me to Argentina.

GOODSTEIN: This was in the thirties?

RASETTI: Yes, yes.

GOODSTEIN: And you resisted. Did you know that Fermi was not going to come back to Italy when he went to pick up his Nobel Prize [1938]?

RASETTI: Oh, yes. Yes, he told me. The Amaldis, my mother, and I were the only persons who knew that he wasn't coming back—and the only persons who went to the station when he took the train for Stockholm.

GOODSTEIN: Laura Fermi confirms that; Laura Fermi has a page where she describes the scene at the train station. Do you think Amaldi had any regrets about not leaving Italy the way everybody else did?

RASETTI: Well, I don't know. No, I think he was glad of staying. His wife absolutely wanted to stay.

GOODSTEIN: How come, since you were very close to Fermi, you didn't see him more often after you were both in this country, after World War II? Was it anything personal?

RASETTI: No, nothing personal. We saw the Fermis once after the war. Then, I don't know, he was in Chicago and I was in Baltimore. I think that when he came after the war to Italy to give some lectures, I was in the Canadian Rockies. And of course, nobody expected him to die at that age. We always thought we could have time to see each other.

GOODSTEIN: Were you good friends with Emilio Segrè in Italy?

RASETTI: Yes. We climbed mountains—the Matterhorn, among others—always, of course, without guides. Oh, we climbed a lot of mountains together. We climbed the Matterhorn from a difficult side, not from the usual side, which is quite easy. From the Italian side it is much more difficult. We went up the Italian side and down the Swiss side. The rocks were covered with ice, so we had to dig steps with the ice ax. Fortunately, we got to a hut before night, the Solvay hut.

Fermi was not a mountain climber. He was extremely sturdy, very strong. He had a lot of endurance, but he was afraid of steep slopes. Anything steep scared him. He could walk thirty or forty kilometers in a day, or bicycle long distances. He liked the mountains but he wouldn't climb them. Segrè and I and Amaldi were all good climbers. We climbed practically everything in Italy, Switzerland, and Austria.

GOODSTEIN: When you were doing physics then, in the thirties, did you have the sense that you were all making history?

RASETTI: No, not at all. Well, we certainly had the sense that it was important for physics. But we never thought that it would become important to mankind, outside of scientists. We never thought it would become of more importance to mankind than, let's say, the discovery of deuterium or new isotopes or something of that sort, or something new in spectroscopy.

GOODSTEIN: When you were at Laval during the war, did you become involved in any secret war work?

RASETTI: No, no. Actually, if I had wanted, I could have been involved with the English group in Montreal, of which I spoke before. But I didn't want to.

GOODSTEIN: Then there was no pressure on you.

RASETTI: No, there was no pressure. For one thing, it was completely independent; this group was not even under the Canadian government—it took its direction from the British government. I knew some of the people who were working there. The chairman of this group was Hans von Halban, an Austrian nuclear physicist. The one I knew best in the group was George Placzek. They asked me if I wanted to work with them. I said no, I didn't want to, and that was that. They didn't put any pressure on me.

GOODSTEIN: What made you leave Laval at the end of the war?

RASETTI: Well, Laval was a little bit too far from centers of learning, and of physics in particular. It was an excellent place to be during the war, because one wasn't under any pressure from either side. But after the war, one felt a little bit too isolated. I was offered, simultaneously, several jobs. I was offered a job by George Washington University, a job at Washington University in Saint Louis, and the one at Johns Hopkins. And I chose Hopkins, because I liked to be in the East. The chairman at Washington University in Saint Louis was very insistent that I go there. I had given a course on nuclear physics there, in the fall of 1947. They liked this course enormously, and I had a great success. So the chairman of the department, Professor [Arthur L.] Hughes, insisted that I accept a place there. But I didn't like the idea of being in the Midwest.

GOODSTEIN: Did your cosmic ray research involve balloons, as Millikan's did?

RASETTI: No, it was at sea level.

GOODSTEIN: How did you pick cosmic rays as a research interest?

RASETTI: Because cosmic rays are free and everywhere. I did some work on neutrons, too. The department bought a radium-plus-beryllium source and I did this work with my collaborators. We published several papers on radioactivity produced by neutrons, which was an extension of precisely the type of work and methods that we had used in Rome—selective absorption with slow neutrons in different elements. And then I also worked on the muons and was one of the first to observe the decay of the muon in the laboratory.

GOODSTEIN: Let's return to Italy in the thirties. I want to ask you something a little different from what we've discussed already. Did the physicists ever take much notice of [Giovanni] Gentile and [Benedetto] Croce and their philosophies?

RASETTI: None whatsoever. We in the physics group in Rome had the deepest contempt for philosophy, and especially for Gentile. We had equal contempt for Gentile, who was a Fascist, and for Croce, who was an anti-Fascist, because we had a very poor opinion of philosophers regardless of their political opinions. I still think that philosophy is all nonsense.

GOODSTEIN: All of it, without exception?

RASETTI: At least the little I've read; it is all just nonsense. When I try to read something about the work of some philosopher, I have the impression that a philosopher stands for this principle: that you have to discover the meaning of a word. That in a word there is something intrinsic, so to speak, which is different from the use of it—which of course is nonsense. As if words—it's difficult for me to express this—as if words have a sort of mystic content in themselves and you have to study what this meaning is. But words are only what they are used for, and there is nothing else in words. No, we physicists are absolutely incompatible with philosophers. The mentality of any physicist—and when I say "physicist," I mean also biologist, chemist, geologist, any student of the natural sciences—is incompatible with the mentality of philosophers.

GOODSTEIN: Were you particularly aware that Gentile was hostile to science in some sense?

RASETTI: I don't know. His son [Giovanni Gentile Jr.] was a physicist and died young, during the war. I confess that I haven't read Gentile's writing. It may not be very rational to disapprove of something one hasn't read. But I've read some other philosophers—for instance, Descartes—and I never could see any head or tail of it.

GOODSTEIN: Well, actually, I think Descartes was a greater philosopher.

RASETTI: You think so? Well, maybe as philosophers go. But did he get anywhere? Did any philosopher ever establish something that was not—ever say something about which some other philosopher had not said exactly the opposite? [Laughter]

GOODSTEIN: There are some scholars who argue that science in Italy was set back because the philosophers in Italy were hostile to science.

RASETTI: That may be; I don't know.

GOODSTEIN: But then the question is, Did you ever experience that yourself?

RASETTI: No. I don't think that philosophers in Italy bothered about physicists. I don't think they either opposed them or approved of them, because they practically didn't know that physics existed.

GOODSTEIN: Let's talk about your experiences as a paleobiologist.

RASETTI: Yes. When I went to Quebec, to Laval, I looked around to see if there was anything interesting in the natural science in that area. Insects presented no interest whatsoever—except that you have billions of mosquitoes and black flies—because of the extreme uniformity of the environment. For thousands of miles all through Quebec and Ontario, they have the same maple forests, and farther north spruce forests. And so there is nothing; the fauna is extremely small in numbers. There are very few species of insects, and each species is found over millions of square kilometers. So there was no interest whatsoever in insects. Then, just browsing around

in the library of the geology department, I found out that there were extremely interesting fossils described in the 1860s from Lévis, which is just opposite Quebec City, across the Saint Lawrence River. And I said, "Well, perhaps I might get interested in fossils." So I read and read and read, absorbed books and books on these fossils. They were fossils of the Cambrian period, which is the first period in which you find well-identifiable and well-preserved fossils—from 570 million to 500 million years old. They are not the oldest fossils, but they are the oldest fossils that show any structure at all. Older than that, there are only unicellular organisms, which are just little blobs without much significance. I found that Elkanah Billings, a paleontologist who was actually by profession a lawyer, had discovered in the 1860s a great number of new species just from that very area. So one day I took the ferry across the Saint Lawrence with a hammer and a chisel, and found immediately—with the first hit of the hammer—fossils that later proved to be a new species, unknown to science. So I continued collecting for about two years. By that time, I had collected a couple of thousand specimens, which were all extraordinarily well preserved—preserved in the finest details. Well, I tried to identify these fossils, and I could identify some. For others, I found, in the whole literature that I read, that they were not described. They seemed to be new kinds. But I didn't trust myself; I didn't know enough. I said, "Perhaps I am too ignorant."

Well, then an extraordinary chance helped me. In July of 1941, I took a trip to British Columbia, with one of my collaborators in the physics department at Laval, just for mountain climbing. We took the train and went to Lake Louise, in Alberta, and to Lake O'Hara in British Columbia. While we were at Wapta Lodge camp, I saw a truck with a name painted on its side: "Smithsonian Institution." So I said, "This is surely Charles E. Resser," because Resser was the foremost specialist on Cambrian trilobites, and I knew that this was an area of Cambrian rock—that all those mountains were formed of Cambrian rock. So I said, "If somebody from the Smithsonian is here, it must be Resser." So in the evening, when these people came back, I found out it was in fact Resser [laughter], and he was with another young man named Maxey, who was helping him to collect fossils. So I introduced myself and I told him of the fossils that I had collected around Quebec City. He showed me some hundred trilobites that he had collected, and I instantly named them all. And he was absolutely amazed. He said, "Well, how is it possible that you know all these?" I said, "I know because I have read all of Walcott's work"—[C. D.] Walcott had been, at the beginning of the century, the foremost expert on Cambrian

trilobites—"and I've read practically everything that has been published on Cambrian trilobites in the States and in Canada." And I told him that I had found fossils that I couldn't identify. "Oh," he said, "there's nothing strange about that, because we know so little about Cambrian trilobites that it's quite possible that you have found a new species, and even new genera." So he said I should publish my results and that I should keep in touch with the Smithsonian Institution.

Well, that was the beginning of my interest in trilobites—for eight years in Quebec, and then for twenty years in at Johns Hopkins, in Baltimore.

GOODSTEIN: You went out on field trips looking for them?

RASETTI: On field trips. I worked for two more summers, in '47 and '48, in Alberta and British Columbia. Then I went to New Brunswick, then to the Gaspé Peninsula of Quebec, then around Quebec City. And from Baltimore, when I was at Hopkins, I worked mainly in Tennessee but also in Maryland and Pennsylvania, and I discovered hundreds and hundreds of new trilobites, developed new classifications of trilobites. And corresponded with all the trilobite students in the world—in Europe, in the Soviet Union, in Japan, in China, in Australia. I knew them all. So that has given me a lot of pleasure. I even became [1964] an honorary research associate in paleobiology of the Smithsonian Institution.

GOODSTEIN: What did you do with all your specimens?

RASETTI: They were all given to museums. I have not kept any. Most of my fossils are in the Smithsonian. Some of those found in Canada are in the museum of the Geological Survey of Canada. Later, after I moved back to Rome, I worked in Sardinia, and the trilobites from Sardinia are in the Museum of the Geological Service in Rome.

GOODSTEIN: The trilobites you found in Sardinia, were they like the ones you found in British Columbia?

RASETTI: No, no. They are all completely different.

GOODSTEIN: Are they like beetles?

RASETTI: They are arthropods, but they are not crustaceans. Out of more than 50,000 I collected, I kept only one—from Mount Stephen, in British Columbia. Usually you find only the head and tail; it is very rare to find one like this one, of the whole animal.

GOODSTEIN: Did you do this along with physics?

RASETTI: Yes, when I was at Laval and at Hopkins.

GOODSTEIN: Do you ever look for them around here?

RASETTI: There is no Cambrian around here. Cambrian in Europe exists in some very limited areas in southern France, on the island of Sardinia, in Bohemia, in some areas of Sweden and Spain.

GOODSTEIN: And the trilobites belong only to the Cambrian period?

RASETTI: No, they existed throughout the whole Paleozoic era—that is, in the Cambrian, Ordovician, Silurian, Devonian, and Permian. But they get scarcer and scarcer as you go toward the end of the Paleozoic. They become completely extinct at the end of the Paleozoic. They became extinct about 300 million or 350 million years ago. The beginning of the Cambrian is more or less defined; it is when we begin to find developed organisms that you can really identify. You find sponges. At about the same time, there appear brachiopods, which are bivalve shells but not related to mollusks, and then trilobites, and then some other groups of organisms that are no longer in existence. Brachiopods are still living today—different from those of the Cambrian, of course.

GOODSTEIN: Did you ever teach any of the paleontology you've learned?

RASETTI: No, I gave lectures but I never taught.

GOODSTEIN: Which part of Sardinia did you go to?

RASETTI: I was in the southwestern part of Sardinia.

GOODSTEIN: Had much work been done there?

RASETTI: There was work done in the 1870s and 1880s, so it was all old work and needed to be redone. The fossils are older than any of those I have from British Columbia.

GOODSTEIN: Now, how do you know that they're older? Do you do carbon dating?

RASETTI: No, the Cambrian is much too old for carbon dating. You can date only igneous rocks by radioactive methods; the dating of sedimentary rocks is done indirectly. That is, you can look for a region, let's say, where there is igneous rock, then some sediments, and then more igneous rock. Then you measure radioactivity of the underlying rock and the overlying rock; and you know that the sediment came later than the lower igneous rocks and before the upper. So you bracket its age. So the dating is indirect, and in many regions, of course, you do not find this situation. But then you can correlate them by the fossils: by the organisms, you know what the rocks are. Although the fauna are different in different areas. For instance, those of Sardinia resemble some in northern Spain and some in Morocco, but are quite unlike those of North America of a comparable age. There were different organisms living at the same time in different seas—just as, let's say, the faunas are different in the Atlantic and the Pacific.

GOODSTEIN: But now you are working almost exclusively on orchids.

RASETTI: Yes, I'm preparing to go and photograph orchids in Central Italy and the Alps.

GOODSTEIN: Has there ever been a book on all of Italy's orchids?

RASETTI: No. Well, there are books on orchids. There are excellent books—but in German—that tell of the orchids of all Europe. But unfortunately the books are out of print and we can't even find them in specialist shops. For instance, there is in Amsterdam a bookstore that specializes in books that are out of print, and even there you can't get them. And the firm that published the German book doesn't reprint them. So you cannot buy a book on European orchids—I mean, a book that is illustrated with color pictures.

GOODSTEIN: The orchids also have the same growing season, from May through the summer months?

RASETTI: Flowers at high altitude bloom in June, July. Well, orchids—again, it depends on the altitude. Let's say in Sardinia or Sicily or southern Italy they begin to bloom in March; around Rome in April; and in the Alps in June or July.

GOODSTEIN: Now, you will have to go into the Alps for these orchid species?

RASETTI: Some of them, yes.

GOODSTEIN: How much equipment do you take with you?

RASETTI: Nothing special. I take the minimum of equipment, especially in the Alps, where I have to climb; I'm too old to take too much weight.

Goodstein: Thank you for sharing your memories with me today.

Some months later, in August 1982, Rasetti wrote to me. He had read and made a few changes to the text of our interview, mostly correcting misspelled names. The only substantial change to the transcript involved his experiments at Caltech on the Raman effect in diatomic molecules. As he put it, ''I found [rewriting] it was the only way to clearly explain what my results were. I understand that this is a technical point that only a physicist can straighten out.'' Concerning

his botanical studies, Rasetti told me that he had taken many color slides in Italy and in Switzerland. He published the second edition of his book, I Fiori delle Alpi, in 1997. A classic in its field, the book covers nearly all the flowers that grow above the tree line. Now 100, Rasetti is the sole surviving member of Corbino's Boys – the illustrious ''ragazzi di Via Panisperna.''

[Franco Rasetti died in Waremme, Belgium, on December 5, 2001.]