1 Rasetti (a sinistra) e Enrico Persico (a destra)

Interviewed by:
Thomas S. Kuhn
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Kuhn:

I will ask some questions, but I will also ask that you don't restrict yourself to my questions; if they suggest something, please go on from there. In many cases I don't know the questions that I want to ask. I will start out really as far back as we can go which means that I will start out by asking a question which perhaps is of interest particularly to Professor Persico. You and Fermi were together a great deal as boys before the university and I take it you'd studied a great deal of physics together. At that time, were there already between you and Fermi any discussions of relativity, of quantum mechanics, of the new physics of the period, or was it mostly or entirely classical physics.

Persico:

Oh, no. Fermi was already much acquainted with modern physics, especially relativity. I learned of the existence of relativity through Fermi. I remember once I asked him what he would have done if he had some big money to invest in scientific research and he told me, "Relativity." Relativity experiments. Yes, I cannot tell at what time that happened, but I remember quite well that we were just students at the Liceo. I did not know of the existence of relativity before that.

Kuhn:

In any case that remark was made before the university?

Persico:

Oh, yes, certainly, yes. As for quantum theory, I don't remember that we discussed about that at that time.

Kuhn:

Was relativity at that point very exciting, intellectually exciting?

Persico:

Yes, yes. It was a time where a lot of people were —.

Rasetti:

In society people were discussing relativity without knowing anything about it or understanding it. It was a popular subject. I remember in Pisa, it may have been around 1921 perhaps, there came a German lecturer to give a lecture on relativity and show a film. To give an idea of the amount of interest excited by this talk, it was even in the opera theatre and probably there were over a thousand people in the audience.

Kuhn:

When you then talked relativity in this period, was still mostly special theory discussed or was general theory also discussed. The general theory existed.

Persico:

I cannot answer. I think it was the special theory as far as I know.

Kuhn:

But so far as you remember the problem of the quantum may not have been discussed, at least you have no recollection?

Persico:

No, I have no recollection.

Kuhn:

To what extent did these subjects come into the university curriculum? There was a popular lecture on relativity.

Rasetti:

But there was no lecture, no course on relativity unless it may have been mentioned incidentally by mathematicians. But I don't think it even was. Certainly in the physics department, in the physics courses, there was not a word about relativity at that time. Not in Pisa, and probably not in any other university. Not in Rome —.

Persico:

Except in the third year of physics, Levi-Civita had a course on absolute differential calculus and I followed it. But that was in 1921 perhaps.

Rasetti:

But it was incidental in a course in mathematics.

Kuhn:

You would not find it at the end of an electrodynamics course, for example?

Persico:

No.

Rasetti:

No. First, I don't think there was an electrodynamics course, at least one that you would call that. There was a course on electricity and magnetism but stopping perhaps at Maxwell's equations.

Kuhn:

It would help me a great deal if you would tell me about the sort of normal physics curriculum in Italy at this period in Rome or Pisa. Fermi went to Pisa; you went to Rome. What determined the decision, in your case and his case?

Persico:

Well, you see the decision of Fermi was determined by the advice of his friend Ingegnere A. Amidei. He wanted to go to the Scuola Normale. I stayed in Rome because it was the most natural thing to do. I had my family in Rome, my mother in Rome.

Kuhn:

What motivated Amidei to give this advice? I don't know anything about the Scuola Normale. Was it a very special institution in Italy, was it unique?

Rasetti:

Weil, in several ways. First, it offered the economical advantage in paying a scholarship and it included not only tuition but living expenses there. Also the students had more contact with the professors than they usually have, especially in the first year, say, in just being registered at the university. Especially, let us say, in the first two years, where the physics and mathematics courses are common to hundreds of students mostly from the engineering school, contact between the professors and the students was almost non-existent. The students attended lectures — those who wished, many did not — and met the professor only in the final examinations. Whereas at this Scuola Normale Superiore, they were supervised. They had some extra courses, some extra work to do under the direct supervision of these professors and a closer contact was established.

Kuhn:

Was the curriculum very different at Pisa?

Rasetti:

No, the curriculum in Italian universities is essentially established by the ministry of education and it is more or less standardized. There may be slight differences. Some advanced courses may be offered in one university and not in another but all basic courses are essentially the same. I had the same courses everywhere. In physics there was an extremely reduced curriculum. All the physics that was taught to the students in four years was the following. There was a two-year elementary physics course on a very elementary level because it was common to all the engineering and chemistry students. It was general physics, college physics, that is taught in the United States. I had a course in general physics, using calculus. (Like the pre-med.) And then, after that there was a one-year course, and there still is, called "fisica superiore", that is, advanced physics.

Persico:

At that time it was called "fisica complementare."

Rasetti:

"Fisica complementare" and then later "fisica superiore." Well, in that the teacher could teach almost any branch of physics. He could teach optics or electricity and magnetism or thermodynamics. It was more or less left to the choice of the teacher what complementary or advanced physics would be taught. And that was about all. Then, of course, we had quite a number of courses in mathematics.

Persico:

Geodesy. It was compulsory.

Rasetti:

And all the shape of the standard ellipsoid, of the geoid, and all the differential geometry about the geodesic curves on the geoid, and all that sort of thing. We had a course in theoretical mechanics in ail the universities but this was a course that was more mathematical than physical. It was a theoretical mechanics taught and conceived by mathematicians, with not very much emphasis on physical applications. The way it was taught in Pisa very seldom was any reference made to any practical experiment. Very, very theoretical. But I mean it was taught here by a professor who is housed in the mathematics department in general, who has contacts with mathematicians more than with physicists. Actually, there were two courses, there was a course on theoretical mechanics and a course called mathematical physics. Theoretical mechanics was self-explanatory. 1vthematical physics may have been comprised of, for instance, theory of elasticity or electricity and magnetism, any branch of classical physics requiring a lot of differential equations. Essentially that may have been the definition of what was meant by mathematical physics. …

Persico:

But when I was a student I attended the lectures on rational mechanics which, of course, is what Franco said now. Then in the third university year I was on advanced mechanics, “meccanica superiore.” This course was not compulsory — it was by choice and it was headed by Volterra. So I attended it and I learned a lot of things about hydrodynamics and integral equations.

Kuhn:

Would any of these mechanics courses have included Hamilton-Jacobi theory, for example?

Persico:

I don’t think so.

Rasetti:

It was not fashionable then. (He’s classical.) People did not seem to have much use for it- in classical physics.

Persico:

Volterra may have taught it.

Rasetti:

Volterra, yes. You would expect Volterra to do that. But Volterra was the only one who did that. I remember in Pisa it was not done. How much mathematics did one take along with this? Well, in the first two years there was a course on analysis, algebra, differential —.

Rasetti:

Algebra was especially advanced because the first course in Italy is certainly much more advanced than the first approach to mathematics in the American universities. It was already what would be called an intermediate course in the States.

Persico:

And then in the third and fourth year there was a course on “analisi superiore”, advanced analysis, and that was the course headed by Levi-Civita on absolute differential calculus and general relativity.

Rasetti:

But somebody else in another university may have done that.

Kuhn:

But one could very well go through physics and never have a course on complex variables?

Rasetti:

Absolutely. I never had a course in complex variables.

Kuhn:

Could you assume a firm foundation in differential equations in that period?

Rasetti & Persico:

Yes, yes.

Kuhn:

Did one also tend to take related sciences, chemistry, astronomy, and so on?

Rasetti:

Yes, they were compulsory. We had them compulsory at least when I was in Pisa and we had physical chemistry and inorganic chemistry; organic chemistry, I think, was elective. It could be switched for something else. I think we had the choice between organic chemistry and mineralogy or something like that. General chemistry was requested.

Persico:

Here are my notes on mathematical physics, 1920-21, Volterra. This is advanced mechanics by Volterra, ‘21-‘22.

Rasetti:

You kept your notes.

Persico:

Mathematical physics, Volterra, ‘21-‘22. This is the year before, 1920. I attended it two years. This is advanced analysis by Levi-Civita. The same. And this is meteorology. [Laughter] That was by Eredia.

Kuhn:

To what extent would Volterra’s lectures have been typical of the content of these courses elsewhere or to what extent would much of the same material have been in other courses at other places?

Persico:

I can’t say it, maybe you can.

Rasetti:

I don’t know very well either. I don’t think I ever took a course in “meccanica superiore.” Of course, these requirements changed somewhat from year to year and also some of these courses that

were elective were a bit more advanced courses, so I don't know. I remember geodesy very well, but I don't remember that "meccanica superiore." It might not have existed, because I don't remember anything of meccanica superiore."

Kuhn:

Was there any astronomy, astro-physics going into the curriculum at all?

Rasetti:

If one wanted, yes.

Persico:

I attended a course on astronomy by Professor Eredia and he gave me a position as his assistant. I was for a year assistant in astronomy.

Rasetti:

In Pisa it was Pizzetti who was well known as a theoretical astronomer.

Persico:

But that was essentially only applied to observations.

Rasetti:

Yes, positional astronomy exclusively.

Kuhn:

Well, I gather that in this period in Italy there were very few students of physics.

Rasetti:

Very few.

Persico:

Oh, yes.

Kuhn:

Why were there so few and how did you two happen to go into it?

Persico:

Well, the reason there were so few, I think, was because it was not very well known at this time that one could take physics as a career.

Rasetti:

Well, first, it led to teaching and nothing else.

Kuhn:

But that was true also of mathematics, wasn't it?

Rasetti:

Yes.

Kuhn:

But there was a great mathematical tradition.

Rasetti:

Because the mathematicians in Italy had had a school of good mathematicians and this in turn had made them powerful in the ministry of education, in the general university life. Mathematicians were influential people and formed a strong, compact group. That is shown by the completely disproportionate number of chairs that mathematicians used to have and partly still have in Italian universities compared to physics. I would say that in any other country you would have in the university about as many professors in physics as in the mathematics department. In Italy, when I was, for instance, in Pisa, there was one professor of physics and. there was only one chair and that meant that you could appoint only one professor "with tenure" in physics. If you count the theoretical astronomers and the theoretical mechanics, men who were naturally trained in mathematics, they consulted with the other mathematicians. As far as I know they never set foot in the physics part of the university during the four years I was there. There were five pure mathematicians plus these two whom you might count as mathematicians. That makes a margin of seven to one physicist.

Kuhn:

I take it that what this also means is that the concession to physics was purely experimental.

Rasetti:

Experimental. Theoretical physics — this combination, this binomial expression "theoretical physics," was not known in Italy. Even the word did not exist.

Persico:

It was sort of mathematical physics. It was more mathematics than physics.

Rasetti:

That was essentially an excuse for studying some differential equations. It was the study of mathematical problems that had originated from physical problems. That is the way one should put

it. So these people who were teaching these courses were not interested in physics itself. They were only interested in the mathematics, and how it originated from these physical problems.

Kuhn:

What books were used with these courses?

Rasetti:

Mostly books or notes written by the professors who taught them. In Pisa, for instance, Bianchi, who taught differential geometry, had his own book. When they didn't have books there was — I don't know if they still have it here — the "dispensa." That is, either the professor himself dictates the evangelic corpus, these lecture notes, or some student takes notes during the lectures, and the professor sort of puts it in better shape. Then it's mimeographed and distributed.

Persico:

But that was only in the first two years. For advanced courses there was no such thing.

Kuhn:

So you had to take notes. Were you encouraged, urged to read books, articles?

Rasetti:

Not very much.

Kuhn:

Were you expected to be able to read books or think in foreign languages?

Persico:

No, nobody talked to us about that.

Rasetti:

No, when I was in Pisa nobody was supposed to read in a foreign language.

Kuhn:

So you couldn't take for granted a knowledge of French or, particularly, German?

Persico:

French was known, but it was not of much use in physics. German was riot taught at all. Well, I had some advice which I remember still from Professor Eredia, the meteorology professor, to study German as soon as I could. Eredia was my professor in high school, the Liceo, and he told me that if I wanted to study physics, I had to learn German. And I followed the advice, but never learned it very well. [Laughter]

Kuhn:

But this would not be a matter of course in some places.

Rasetti:

No, no, no. Certainly, nobody expected us to learn German or English.

Persico:

The first book on physics that Fermi and I studied on a high level was Richardson's Theory of Electrons.

Rasetti:

I studied it too, upon Fermi's advice.

Kuhn:

So you had English already.

Persico:

Well, I studied English in the first year at the university by myself.

Rasetti:

I first studied English when I was about eight or ten years old. I am ashamed of how I talk in it because I have been reading English books all my life.

Persico:

At the Liceo I had only French, and at the Ginnasio. But Fermi knew German before the university.

Rasetti:

Fermi must have known —. Well, when I first met him he knew German perfectly. He read German like Italian.

Kuhn:

Well, apparently this learning of German was prompted by Ingegnere A. Amidei, who said he should learn German. That question comes out again in the letter that Segre quotes in the biographical introduction and I'm still not clear about this question of the Scuola Normale, because I take it in Fermi's case, his family would have preferred to have him in Rome.

Rasetti:

If it's said, I don't know of it.

Kuhn:

It wasn't dictated principally by financial considerations.

Rasetti:

Of all this, I have no idea. I didn't know Fermi until he was in Pisa.

Kuhn:

You went there —.

Rasetti:

Because of my family being there. I went all through high school in Pisa.

Kuhn:

But this was not the place to do science in the sense that the Ecole Polytechnique has been the place to go.

Rasetti:

No, no, no. Actually, well, I would say the dominant field for this Scuola Normale Superiore, at least in those times in Pisa, were the humanities and mathematics. I would say the students were divided into these two groups.

Kuhn:

It would not have been particularly a science-physics school.

Rasetti:

Not at all. I would say, in fact, that there were more in the humanities than in anything else. Second to that was the group of mathematics students and then a sprinkling of other fields. In fact, in the case of the director and assistant director of the Scuola Normale Superior, one was a Latin professor and one was: a mathematician. Rosadi was the professor of Latin at the university and Bianchi was from the mathematics department. So that was already indicative of the main tendencies that are presented there.

Kuhn:

Now let me ask again the question I asked with respect to the pre-university period. You had heard about relativity, you say, there but there was none of it in the course.

Rasetti & Persico:

No, no.

Rasetti:

No, I don't think I even heard before I went to the University. Relativity happened during the war. Before the first world war, at least in Italy, nobody knew about the existence of relativity. It had not been glamorized, so to speak. Then came the first world war, relativity was developed essentially in Germany, and Italy was on the other side and there was no communication. So until 1919, so to speak, there was no possibility. So it must have been around 1921, in-between '20 and '21, I guess, that relativity all of a sudden became a popular subject. So I first heard of it when I was already in the university. I never heard of it before.

Kuhn:

Now, what about the quantum?

Rasetti:

Oh, even less. Even less. By 1920 or even '22, quantum theory in Italy was essentially confined in Fermi's mind and there was very little outside. For instance, I first heard of the quantum theory and Planck's constant from Fermi. My teachers, I suppose, knew about it, but they had never mentioned it to me. Fermi was the first person ever to mention to me Planck's constant and the whole theory. I don't remember exactly what year. My guess is in the beginning of the third year at the university. That was when he first mentioned the quantum theory to me. I remember Puccianti was our venerated maestro. [Laughter]. Puccianti knew of the existence of the Bohr atom and Polvani knew about it. I remember that when Fermi discussed these things with them they had heard about it. I don't know how much they knew about it, but at least they knew of the existence of Bohr's atom. From Fermi also I heard for the first time about the nucleus. That is very significant. I don't remember in which year, but that can be reconstructed from a popular article that he wrote. You remember that be wrote a review article on the nucleus that is published.

Persico:

On the atom?

Rasetti:

Only on the atom? No, but I remember then that there came one day an issue of Nature with a review article by Rutherford on the nucleus. I knew nothing about it, next to nothing about it. Perhaps I had just heard of the existence of the nucleus but I knew nothing then of the structure of the nucleus. And there was a new article — that may have been in 1920 or something like that — by Rutherford about the experiments on the first artificial disintegrations. And Fermi was extremely excited. Nobody in the physics department would have noticed this article in Nature. Fermi immediately noticed it and I remember be gave a talk, sort of an abstract of these articles, to Carrara, who was the other physics student, and myself. Carrara is a microwave man now. He told us what he had learned by reading Rutherford's articles. So it's, interesting that the nucleus was around very early.

Kuhn:

But none of this had any effect on the curriculum?

Rasetti:

None whatsoever. I would say that Fermi's knowing relativity or quantum theory or anything like that had no influence. It had no effect whatsoever for these courses. As far as the university's curriculum was concerned, that was nonexistent. You had only to pass the exam in elementary physics, as physics vent, and in the "fisica superiore" I don't remember what was taught. Maybe optics. It was the tradition more or less to teach in these courses the field of personal interest for the teacher. Isn't it strange that I cannot recall in the least what was taught in "fisica superiore", and, who was teaching?

Persico:

Maybe Polvani. No?

Rasetti:

Polvani should have been. It's funny that I cannot recall anything about this. I have a complete vacuum in my mind about this physics course in Pisa — what was taught and —.

Persico:

There was no other physics professor besides Puccianti.

Rasetti:

Polvani was the second man. He was Puccianti's right-hand-man, man Friday. You know, he picked up Puccianti's cigars where he left them. He would pick them up and went after him. He did everything from picking up his cigar butts to handing him a light to light his pipe, to preparing the experiment for demonstration lectures. In Italy, especially in those times, great importance was attributed to preparing lectures, demonstration lectures. I would say for many physics professors it was perhaps the activity in which they invested most of their working time. For instance, Quirino Majorana in Bologna, used to make quite a show and prided himself in producing extremely difficult experiments before an audience. Let's say, perhaps, he performed the Cavendish experiment on gravitation, or things like that, in front of an audience. Puccianti did not go that far, but I remember, for instance, in optics he performed quite elaborate experiments. He showed, for instance, … the anomalous dispersion in sodium vapor, and he showed it to a large audience. And Polvani, very patiently, was his victim; he had prepared all these. So Polvani was his right arm. He did everything for him. And next to that there was Pierucci who was a very inconspicuous man. And then there was Chella. I don't know if he achieved any significance in any way. He must have died young or maybe he left the field of physics because one never heard of him after I left Pisa. And that, I think, was about all.

Persico:

I think Rome was the only university perhaps that had two full professors of physics. It was considered normal that physics was taught by only one. In Rome there were two, Corbino and Lo Surdo.

Rasetti:

You see, usually the "fisica superiore" or "fisica complementare" was taught in all or most universities. But it was not taught by a full professor. Courses can be taught by a "professore

incaricato," that means one who has no tenure, no official position, who teaches that particular course by one year appointment by the university without any commitment of extension to further years. He can be an outsider. Very often he's employed by some other agency and just as a sideline he just teaches one course by appointment. So the tradition was that there was just one professor of physics against six or seven of mathematics. And that the "fisica superiore." The second physics course would be taught by these year-by-year appointments. So that shows how unimportant physics was considered compared to other branches of knowledge.

Persico:

There was also a laboratory course. In Rome there was an elementary laboratory course, and it was pretty modern. There were experiments already prepared. The students had just to make the measurements and to write a record.

Rasetti:

So it was in Florence. I know it very wail because when I got my first appointment, after graduation, in the University of Florence, that was the job I had for the first two years. I had to prepare the experiments and conduct this lab course and it was quite thoroughly done.

Persico:

There was a theoretical, an older laboratory course in the third and fourth year. That did not work because there was no professor who would want to spend his time with experiments. He just gave his students a problem and no apparatus. They just had to look for scraps. [Laughter] There was no organization at all.

Rasetti:

Then there was usually a machine shop but only professional machinists had the right to use the machines. So the student did not have any tools at his disposal, no finished instruments. He had some, as we said, scraps, and no tools to put anything together with.

Kuhn:

You answered as far as one can perhaps my question as to why so few people went into physics. You two were both exceptions. How did each of you happen to go into it? What were the ways to be attracted to physics? How did you come to go into this very unpopular field?

Persico:

Well, my first discovery of interest in physics was before my acquaintance with Fermi. I had discovered an old book, which belonged to my father, on elementary experimental physics (Gernot), Traite de Physique. I studied it and I found it very amusing, very interesting. But I never thought that could be my chief interest. I was supposed to become an engineer and it was probably conversations with Fermi, who presented to me the possibility of becoming a professor of physics; that changed my direction. The thing was not decided even when I entered the university. I was not yet decided and I started at the university as an engineer.

Rasetti:

Oh, you did too?

Persico:

I studied (???) also, and after two years at the university I made the decision. The first two years were almost common with engineering students.

Rasetti:

Well, my case is in some respects similar, in some respects different. In high school we had a terrible course in physics, perhaps the worst course taught. So I didn't like the subject, not because of physics, but because of the way it was taught. I was very poor in it, and in fact, I had the lowest mark in it of all my courses upon graduation from high school. I was strong in Greek, Latin and philosophy. But my interest had always been in the natural sciences, in zoology, botany, mineralogy, paleontology and so on. Now when I came to register at the university, I was fully aware that my chief interests were in the natural sciences, but I thought that those would not lead to any other profession than high school teaching or at most perhaps after a very, very, many long years a university appointment. So for practical purposes, I registered in engineering. I was also aware that for the first two years the curriculum is common to physics, chemistry; it can be used for many purposes. Then for two years I was an engineering student, but toward the middle of the first year I became closely associated with Fermi and from Fermi I learned a little of the beauty and interest of physics. I must say that even though I had a horrible experience in high school because of the poor way physics was taught, I was always aware that behind all that dreary elementary physics there was a field of great beauty. For instance, strangely enough, I knew nothing about physics except thermodynamics. When I graduated from high school, one of the few things I had clear ideas about was the concept of entropy. [Laughter] Very unusual thing. I remember that I could not understand, probably because nobody had explained it to me in a reasonable way, Gauss' theorem. But I knew thermodynamics quite well.

Kuhn:

This is when you graduated from high school?

Rasetti:

From high school, yes. This happened through an uncle of mine, who was a professor of medicine. He had wide interests also in mathematics, physics, chemistry — a highly cultivated person. He spent some summers in a research institute in the Alps where they conducted research on physiological effects of high altitudes and cold on people. So it was populated with medical people, physiologists, pathologists and so on. I was a high school student and since one of these was my uncle, I went there and I helped them. I would carry the instruments and also I was being used as a guinea pig, for example, staying half naked under the snow-storms to see if we could catch pneumonia. We called the experiment "to catch pneumonia," but, of course, we never did. They measured the temperature of the air we breathed, and things like that. With all this, he taught me real thermodynamics and told me of some books I might read. So I happened to know about thermodynamics and practically nothing else of physics.

Kuhn:

I take it from what you said before that this was a subject that except by some accident really was not in your Italian curriculum.

Rasetti:

No, no. In fact, this thermodynamics remained the only thermodynamics that I had studied until the end of the university. So I realized that physics was interesting but I knew next to nothing about it and I knew very little mathematics. So when Fermi tried to explain to me how beautiful physics was, I felt that it was interesting, but I felt that it was difficult and I didn1t understand it. That attracted me to it. It sort of annoyed me that there was something that I understood nothing about.

Kuhn:

Did you get from Fermi at this time already a sense that this subject was in a state of terribly rapid change?

Rasetti:

Yes, yes. Absolutely. Fermi conveyed this sense by telling me precisely about quantum theory, about relativity. He had, the Zs. f. Phys. in his hands practically all day, and he told me, “See, they have done this, see, they have understood the orbits of the electrons in the atom.” He gave me the impression of the tremendous development of the field that had occurred in Germany just before and during the first world war. And he told me that in, Italy, nobody knew anything about it, but in the rest of the world was this extraordinary field. So he gave me an extremely vivid impression of the situation. And. I was amazed, of course, seeing that such a young man had absolutely by himself understood the situation and had read by himself all these foreign papers in journals and had understood the extremely backward situation with respect to physics in the Italian universities. I must say that I discovered Fermi very early. I don’t remember how many weeks after, perhaps a month or two after we had registered. You see, we registered in October, 1918. Certainly, less than two months after I had been in the university I had seen this student who was sitting next to me. We got started to talking accidentally. It took me just a few days to realize the absolutely exceptional nature of Fermi. I remember that one day I came home in the afternoon and I told my mother, “I have met another student who is a genius, a man like I have never seen before. He must be a sort of prodigy. He knows more than all the professors put together in physics and he understands everything. He is in years above his age.” I was, so to speak, tremendously impressed just a few days after I had met him.

Kuhn:

Shortly after the university, Fermi had this fellowship which took him first to G6ttingcn and then to Ehrenfest at Leiden. That’s described, I think, by Segre in the biography as an Italian government fellowship.

Rasetti:

[Gives name of fellowship in Italian.]

Kuhn:

Was that a regular fellowship, regularly available? For study abroad?

Rasetti:

Yes, yes. Probably there were just a few for all the sciences put together, and they have to apply. Fermi already had publications. He would have very good qualifications because he had already published several papers.

Kuhn:

Did he ever talk much with either of you about what was the matter at Gottingen? I mean everybody knows that somehow or other he did not have a good time. People say he was shy, but then he does not seem to have been so shy. What was the matter?

Rasetti:

With me, he never told me. He never told me. But on the other hand, from all he wrote to me from Germany and from what he told me when he returned or when he came back for Christmas his impression of Germany seemed enthusiastic.

Kuhn:

You don't still have those letters, do you?

Rasetti:

About Germany? No, I never kept anything, unfortunately. I don't know about the physics, but about Germany in general I remember that he was very much enthusiastic about it.

Persico:

Well, I cannot remember. I remember that he did not make comments on —.

Rasetti:

No, I definitely remember at least that he said that he found everything so superior to what he was used to in Italy in every respect. The country is so well-organized. They like good functioning in everything. In Italy, especially in those times, practically nothing functioned properly. That was not long after the end of the war and there was a tremendous disorder in everything, transportation, housing, government administration. So he was extremely impressed even though Germany had been defeated, still that machinery worked very well. So this I remember. But about physics, I cannot remember any comments that he made.

Kuhn:

You then next saw him when you were both at Florence together.

Rasetti:

Yes. I went to Florence in November, '22 and he came exactly two years later in October, '24. Fermi had first been abroad, then back in Rome for a while. He got the appointment, precisely one of these temporary appointments, "professore incaricato" of theoretical physics and rational

mechanics in Florence in October, So we were there two-years because then in ‘26 Fermi got the chair in Rome and a few months later I also came here. So we were very close. We practically lived together. I have lived for two years in the [physics] I building in an unheated room where the average temperature throughout the winter was 4 centigrades so as to have water at the maximum density. So I have lived two years in these unheated rooms. Then, however, after two years my father died. Then my mother came to live in Florence and so we bought an apartment in town. Then I didn’t live in the physics building which, incidentally, is on a hill several miles out of town. That’s why it was convenient to live out there. But then I lived in town and went back and forth. Then Fermi went to live in the room I had vacated in a nearby unfinished building that now houses the optics department and at that time was called the “vagoncino”, the little wagon. So Fermi went to live there and I spent all day there. We were practically together from morning till evening, from discussing physics to hunting for certain geckos, a sort of lizard that we collected in order to scare the wife of the janitor who cooked our meals. We captured about twenty of these lizards and one day we released them all, about twenty or thirty in the dining room. So they crawled all over because they have a sort of sucker and they can walk on smooth surfaces and upside down on ceilings. They spread all over. It would be interesting if one could have a photograph of Fermi lying on the ground for an hour or two with a rod about six feet long in his hand of Pyrex glass. Not that it had to be Pyrex glass, but in the physics department if you want a stick or something, you picked up a piece of Pyrex glass. At the end of this rod we have made a loop with silk thread and tied the pieces to the end of the glass rod. The loop then would close around the throat of the lizard. So we saw these as we went by the wall along the road that connects the physics building with the astro-physical observatory. In the crack of this wall used to live these lizards, these geckos, more precisely. And we would see one disappear into a hole, into a crack in the wail, then we would lie on the ground, about six feet away, holding this rod. And sometimes we would spend an hour like that waiting for the lizard to stick his neck out. Then we would pass the silk loop around the head. You know we saw that they were sort of tickled, like when you tickle a cat. They didn’t mind at all. So they let themselves get this noose around their neck, then we pulled back suddenly and there were dangling the geckos. So we would capture them and put them in a glass vase and we captured about twenty or fifty of them. And that was precisely when he was making the gas statistics. He was probably thinking while lying down and catching the geckos with the noose.

Kuhn:

Is Laura Fermi right in suggesting that most of these ideas were yours?

Rasetti:

Oh, the idea of the geckos was mines Fermi would not have noticed their existence to begin with. I have always been a zoologist and botanist and mineralogist and paleontologist and, so forth.

Kuhn:

By this time, ‘24, in Germany, in Copenhagen, it is very clear to a number of people that this whole line of development of post-war quantum mechanics has something badly wrong with it, and really fundamental things have got to come about. There’s a sense in which everybody always knew this because of the conflict with classical mechanics. But there’s something new, a new feeling by ‘25, by ‘24, that the problems that ought to be solved by the techniques that were solving problems before aren’t working anymore. Did you and Fermi have that feeling also?

Rasetti:

Yes, I remember that Fermi got very much excited at first when the article of Bohr-Kramers-Slater came out — you know, non-conservation and so on. That, of course, was contradicted by the experiments of Bothe-Geiger and Compton and Simon, and so on. That shows that, Fermi was extremely aware of the Well, we were all aware of the unsatisfactory nature of these semi-classical theories. We were fully aware of that.

Kuhn:

He liked that — I mean, he was initially at least very much impressed and excited about that article.

Rasetti:

Yes.

Kuhn:

It didn't seem just silly —.

Rasetti:

No, no, not at all. In fact, I remember that we plunged into that article and discussed it immediately as it came out in Nature.

Kuhn:

Did you worry also about the photon problem, the Compton effect, the dual nature of light?

Rasetti:

Yes. Very much, but I think that was nothing special with us. I mean, everybody was.

Kuhn:

That problem, after all, had existed for a long time ever since the photo effect and it's amazing for how long people managed to push it aside.

Rasetti:

Yes. There has always been a tendency in writing books, articles, to just push away unpleasant problems that one doesn't know what to do with and just ignore them in a way. Fermi was very much aware of this fact. He always said that it was not a nice thing just to ignore these problems because we don't know how to solve them or even really how to formulate these problems in the proper way. That is no reason for putting them aside.

Kuhn:

What about the Stern-Gerlach experiment? This was for many people again also terribly puzzling because the atoms should have precessed where they were instead of flipping into particular directions. Was that a worry?

Rasetti:

I don't remember. Probably it was, but I don't remember.

Kuhn:

Again, another problem that was very bothersome to many people was the fact that by '2 it was pretty clear that you could do the one electron case, but —.

Rasetti:

Yes, yes. Exactly. Nothing worked for the helium atom, yes, yes. That was very disturbing. That worried everybody very much. I remember that.

Kuhn:

Professor Persico, was there any of this same sense at Rome?

Persico:

What is that?

Kuhn:

By '24, '25, in Rome, did people know more about what was going on?

Persico:

No, no.

Rasetti:

No, because essentially Fermi was the only one who knew these things in Italy so if Fermi wasn't worrying about them, nobody else was.

Kuhn:

So physics in Rome must have been still pretty much as it was ten years before.

Rasetti:

Classical, it was still classical. Fermi was still the singularity — all the modern physics —.

Persico:

At that time there was much interest in relativity.

Rasetti:

In relativity, but not in the atom. Nobody knew about the atom, or that it existed practically.

Kuhn:

Was there anybody who didn't believe in the atom?

Persico:

… The general feeling was that the atom was still the (???) atom and that there was not much —.

Rasetti:

… Exactly. No serious problem for physicists to worry about the atom.

Kuhn:

Do you remember stages in the development of the Fermi statistics?

Persico:

Vaguely.

Rasetti:

I only remember what is written in the introductions to Fermi's collected papers. Fermi had been concerned with the statistics because he had published those papers on statistics previously. … So, that he had been worried about statistics is obvious. One day he came and showed me the paper or at least a summary in almost finished form of his statistics.

Kuhn:

Do you know or did you discuss whether he was much interested in the Einstein papers of '24, '25. These are the ones that give the Bose-Einstein statistics.

Rasetti:

Yes, he was. Yes, he was aware of them, but I would say that when he wrote his paper, he wrote it —. No, not independently. I think he even opened his paper by mentioning Bose-Einstein. No, it was certainly influenced by that.

Kuhn:

How did he get interested in the fluorescence radiation problem? Was that through your experimental work?

Rasetti:

Yes, yes, yes. All his interest in fluorescence and Raman effect was because he had watched me do these experiments and he thought about them.

Kuhn:

It's interesting how much that particular interest pops up in connection with the quantum theory in just this period. Heisenberg was very interested. Well, Bohr has a little piece in Naturwissenschaften about degeneracy and incomplete polarization. Heisenberg does an article in which he succeeds in predicting for some cases, by a very funny argument, what the partial polarization will be and what the percentage of polarization will be in certain cases. Did Fermi know that particular paper, do you know?

Rasetti:

No, I don't think so. I don't think so. No, I'm sure that when that experiment was done, Fermi knew nothing of any quantum theoretical interpretation of those phenomena.

Kuhn:

Well, he knows the g factor.

Rasetti:

Yes, but just the classical theory and then change the g to 3/2 instead of one, but that is all. No, because you see Fermi at that time did not have any understanding of the Heisenberg-Born-Jordan-Dirac formulation of matrix mechanics.

Kuhn:

Well, no, now this is before that though. I'm thinking of the old quantum theory.

Rasetti:

Oh, I didn't know that there was a sort of —. I was not aware of this. How was it possible to have any concept of superposition of states in the older quantum theory? At least the word superposition of states could not have been employed in those times. But how even anything resembling it could be done with the Bohr-Sommerfeld theory, I don't know, because, after all, it's a theory of stationary states, essentially, and nothing else. How can anyone do anything non-stationary with Bohr-Sommerfeld theory? Transitions, nonstationary states were all —. … I'm sure that Fermi was not aware of it.

Kuhn:

Fermi was still at Florence, and you were at Rome when first matrix mechanics and then the Schrodinger papers begin to come out.

Persico:

Yes.

Kuhn:

How were those received? You were interested, at least, in Rome in the Schrodinger papers.

Persico:

Yes.

Rasetti:

No, the matrix mechanics had no reception in Italy, neither one way or the other. Nobody in Italy even tried to understand it. Fermi was the only one who tried and he did not understand that. I should say that I don't know about others, but as far as I know, nobody else even tried. Who would try besides those here present? Nobody knew even that quantum theory existed almost.

Kuhn:

Did Fermi say anything that you remember about the matrix mechanics papers?

Rasetti:

Oh, Fermi tried to read them, but he could not understand them. He said, "I cannot do any. I don't see how I can use it, how I can do any calculation with these. I don't understand what's behind it." Oh, he read them and he was very much puzzled by them.

Persico:

I don't remember that I ever discussed them with Fermi.

Rasetti:

I know that he showed me these papers and said, "Now I'm trying to read them and see what Heisenberg is trying to say, but so far I don't understand it."

Kuhn:

And you think in Rome it probably did not even go that far.

Persico:

No. I don't remember if I tried to read the papers, but I certainly did not succeed. I don't remember if I even tried. Probably I did not know the existence of these papers."

Kuhn:

Now Schrodinger was different from the beginning.

Rasetti:

From the beginning. I remember very veil when Fermi tried to understand Kramers' and Heisenberg's paper. They gave already the formulas of the dispersion theory without understanding really how this came about. Somehow they got the Kramers-Heisenberg formula. And Fermi just couldn't understand that.

Kuhn:

That's even before the matrix mechanics.

Rasetti:

Yes, it was a sort of prelude to matrix mechanics.

Kuhn:

You say that you could not really understand how they got that?

Rasetti:

I should say that at that time nobody in Italy could understand that paper. It became understandable only after subsequent developments.

Kuhn:

Now we are alright again. We have just said that it is really with the Schrodinger equation that things really begin to happen with quantum mechanics in Italy.

Rasetti:

Yes, at least with Fermi, because Fermi first understood it and then he poured it into the brains of a few people around him.

Kuhn:

With Schrodinger, people like Fermi who had not understood matrix mechanics, who hadn't really understood those funny late developments of the Correspondence Principle, must have felt more at home. Particularly also because of its interpretation.

Rasetti:

Yes, on the idea of the charge density varying with the combination frequency, and all that.

Kuhn:

And the idea that radiation was an interference effect between two wave functions.

Persico:

Yes, I think that was rather congenial with Fermi's mind.

Kuhn:

What happened to that then? Because clearly that was not an interpretation that lasted very long in most circles.

Rasetti:

I don't remember the transition in Fermi's mind. When Fermi was in Rome in '27 he already knew the Heisenberg Uncertainty Principle, the matrices and the statistical interpretation of quantum mechanics and he had fully absorbed all that. But I don't remember when there occurred in his mind the transition from the Schrodinger interpretation to the Born statistical interpretation.

Persico:

… What I remember about the uncertainty, principle is that I was much impressed by Heisenberg's paper and when I spoke to Fermi about it, I was surprised to find that he was not so enthusiastic about it. I had the impression that he did not think it was so important as I believed. Probably Fermi was not much interested in philosophical aspects of physics and that was too philosophical.

Rasetti:

Yes, that was very characteristic of Fermi. He was not interested in anything that he could, not use to perform some calculation and. can predict some phenomenon when he finds it. Something that could not be used, that remained in the stage of a general idea but that couldn't be —.

Persico:

Did you have any occasion to talk about this indeterminateness with Fermi?

Rasetti:

No, before coming to Rome, I have no recollection of it.

Kuhn:

But that would not have been before coming to Rome, because that paper came out in '27.

Rasetti:

That's right, that's right. So I came to Rome in February, '27, so that's right. I could not have a recollection of that before. But even in Rome I don't remember how and when it was discussed and what were Fermi's reactions. I have no recollection of it.

Kuhn:

You people say that he was not interested in philosophical aspects but there is that one paper on (causality).

Persico:

Oh, yes. Very typical.

Kuhn:

But that theory is exactly on this branch of problems.

Persico:

Yes.

Rasetti:

Yes, but still it is a problem for which you can give definite numerical answers and it is not one of those problems that remain in a vague form. It is formulated in a way that it makes a perfectly definite answer.

Kuhn:

There's a remark in the biography by Segre that at least later in life Fermi sometime indicated that he was not sure that this statistical Copenhagen interpretation was the right one. Was there any dissatisfaction of this that you know of earlier?

Rasetti:

Not that I know of. Certainly he never even remotely in his late years approached the degree of dissatisfaction of de Broglie or Schrodinger with the standard statistical interpretation. In fact, I would say that the only recollection that I have of Fermi at various periods expressing an opinion on quantum mechanics were quite definitely in favor of the standard interpretation.

Kuhn:

Were you people at the Como Conference?

Rasetti & Persico:

Yes, yes.

Kuhn:

That was, of course, one of the places where the interpretation question was much discussed. Bohr gave a paper.

Persico:

Yes, I did not understand a word of what Bohr said [Laughter] Probably we were not sure in which languages he was talking. [Laughter] First of all, I did not understand the English language.

Rasetti:

You know, sometimes people have been in doubt after hearing a lecture by Bohr as to what languages he had, spoken in.

Kuhn:

How would you feel about that paper?

Rasetti:

I don't think I had any reaction about it.

Kuhn:

Did that conference itself have any direct effect on the development of Italian science?

Rasetti:

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. Fermi, at the time of the Como Congress in September, '27, was famous in Germany, in Copenhagen or somewhere else, but in Italy, his greatness was only appreciated by a handful of people. And especially he was not greatly appreciated by —. Well, the university yes, because after all, he had a professorship of theoretical physics in Rome. But still, it was a relatively small group. The people who realized who Fermi was didn't even include all the physics professors in Italy. And included a group of mathematicians or the University of Rome because they were 1riends like Levi-Civita and Castelnuovo and Enriques. And I would say that apart from these, nobody else in Italy knew who Fermi was.

Persico:

He was perhaps considered more by mathematicians than by physicists.

Rasetti:

Yes. This after all, is not surprising. These people would have no first-hand appreciation of Fermi. People who were not theoretical physicists — how could they know that Fermi's statistics was an important paper? They have no way of knowing it. Well, in Como when (Franck) and hundreds of people; Sommerfeld, who was known as sort of a high priest of the German science, —.

Persico:

Respected by both mathematicians and physicists.

Rasetti:

And so, when Sommerfeld told everybody how important Fermi's statistics was for the theory of electrons in metals, then everybody began to realize that Fermi had achieved something very important. So that was really the revelation of Fermi in Italy. The reputation in Italy came back through Germany.

Kuhn:

Did it also show, at least the physicists in Italy, how much they had missed?

Rasetti:

Yes, yes. Although those physicists who had missed modern physics were practically beyond the possibility of conversion, so to speak. People like Pochettino for instance, or La Rosa or Lo Surdo.

There was a group of old profess3rs among whom these three still did not admit quantum theory, relativity, all these new-fangled things. Mostly, I think it was because they were too lazy to learn them and so, to justify their laziness, their inability to study and understand them, they depreciated them and said, “That is not serious physics.” Quirino Majorana was like that. You know, the typical complex of people who don’t believe in relativity and try to disprove relativity. It’s strange. It’s one of the phenomena that has amazed me most. For instance, nobody tries to disprove the Uncertainty Principle, but the number of people who tried to disprove relativity had hoped with an almost religious faith that relativity would fail are legion.

Kuhn:

I realize I skipped over one thing in which you were involved somewhat and that’s electron spin, Professor Rasetti.

Rasetti:

Also I can say again that Fermi read about the spin and explained it to me and we, so to speak, wrote a paper together which consisted essentially of Fermi putting the ideas and me writing them down.

Kuhn:

How did you and he feel about the idea of spin? I mean you come out with one argument against it that can be very serious or not, depending upon how seriously you choose to take it.

Rasetti:

I confess even now I don’t know how this business of the magnetic structure is. Does it mean anything today? I don’t know. …

Kuhn:

But how did you foci about it then? Was it a serious argument?

Rasetti:

I wouldn’t say so because after all, we knew that this sort of argument —. One didn’t know anything of the inner structure of the electron. After all, taking the spin for granted explained so many things that —. We didn’t mean that paper very seriously. Fermi, I don’t think, did. He said, “Well, it’s sort of a cute point to make,” but certainly he did not consider it as a serious argument against the spin.

Kuhn:

He was reasonably convinced —.

Rasetti:

Yes, yes. He was very enthusiastic about the theory of the spin.

Kuhn:

Because for many people that was very difficult. Pauli didn't like this. The whole notion of the electron as a point particle spinning was repulsive to Pauli.

Rasetti:

I didn't know that Pauli felt that way.

Kuhn:

Oh, Pauli was, among the major figures, the last to be convinced. Bohr held out for a while, Heisenberg held out for a while, but Pauli held out for quite a long time.

Rasetti:

Like Bohr was about the last to be convinced about the neutrino. Bohr was the last to give up the energy non-conservation in beta decay. He didn't like the idea of the neutrino.

Kuhn:

How long did he go on holding out against the neutrino?

Rasetti:

I don't know. I didn't send you photostats of my two letters from Pauli, did I? I'll send them to you. He wrote two very interesting letters to me when I was writing those introductions to Fermi's papers. I'll send you photostats as soon as I get to Baltimore. I will run them off on the Xerox. And I think I should send the original to Mrs. Pauli. Perhaps I should send the original to her and make a certain number of copies to keep because that's the best thing extant on the history of the neutrino. …

Kuhn:

When I talked with Professor Amaldi this morning, he told me something I had not known. When you did your [work on the Raman effect of nitrogen 14] Pauli again said that it must be wrong. … Tell me about the reception of that paper.

Rasetti:

Well, I just described the experimental results. I was in Pasadena at that time. All I know is that that immediately produced a theoretical paper of Wigner and Witmer in which they showed that really on good arguments that —. Because you see I had doubts. I didn't know enough about the electronic wave functions of the molecules. So I said, "Well, if the electronic wave function is even then it is plausible to assume that it's a very strange result!" Of course, I was fully aware that if the electronic function had been odd then it would have implied another kind of statistics. So I didn't know enough about the electronic wave function in molecules to be sure of that. But then immediately appeared that note of Wigner and Witmer where they showed on good arguments that the ground state would have an even electronic function. So that's all I know about reactions.

Kuhn:

Do you know about this Pauli reaction?

Rasetti:

Oh, no. About Pauli I know this. I had not attributed such great importance to this nitrogen spectrum. But, when I came back, in Rome they had raised so much fuss about the statistics of the N 14 nucleus that I said, "Well, it's worthwhile to do it with high dispersion because I just got one picture in Pasadena. So I should do it with high dispersion to be absolutely sure of this result." The new spectrum confirmed the old result. Then in Zurich there was Meyer, who was professor of physics at the university, not at the technical high school, who said, "Obviously these experiments are all nonsense. That's a diffraction of the light. It's not a molecular rotation Raman effect." Now Pauli said, "No, I believe in Rasetti's result, and I believe it's the Raman effect, and they bet On it. I don't remember how much but they actually made a bet on this and got so excited about it. Pauli bet in favor of the Raman effect and Meyer in favor of the diffraction. Then there appeared my work that I had done in Rome. I had thought of the technique of filling the spectrograph with mercury vapor and thus reabsorbing all the exciting 2557 line. I did that because the over-exposed image of the exciting line obscured the nearby rotational lines. But, of course, incidentally it proved that it was not diffraction. You know, at that time I didn't know of Pauli's bet against Meyer. So I explained that in Zurich and in Leipzig. I gave in each place a lecture on the Raman effect done in Pasadena and Rome. So, when I went to give these lectures in Zurich, I produced these results with the primary light reabsorbed in mercury vapor, and so on. Then Meyer said to Pauli, "I concede. You win." And he paid his bet.

Kuhn:

Pauli seems to have been involved in a lot of bets of this sort. How much were you involved either in conversation or actual work with the earlier quantum electrodynamics work of Fermi?

Rasetti:

Nothing at all. I have never known enough theory to be concerned with quantum electrodynamics.

Kuhn:

I would be particularly interested in knowing at what point you decided the divergences were a basic problem as against not having found the right mathematical —.

Rasetti:

I don't know. I don't think anybody now knows. Perhaps the only one who may have known was Ettore Majorana, because he was the only man he had around who could understand. Except Racah was around in those times. Racah and Wick. For theoretical questions of the early thirties I think perhaps they would be the best sources because they were the theoreticians who were closest to Fermi. They were both in Rome for extended periods and they could talk with Fermi of these subjects. I couldn't, I'm not a theoretician and I don't understand these things. And perhaps Majorana, assuming that Majorana talked with anybody much, but he's dead anyway, so —.

Kuhn:

At some point, late twenties or early thirties, there was this deliberate decision to switch the effort of the whole group to nuclear physics.

Rasetti:

Yes, that we know very veil. Yes, that's more recent history and also being an experimental physicist I know more about that. You see, theoretical physics, Fermi did in his own brain, at five in the morning, at home. Nobody knows what went on in his mind, what theories he tried. Whereas in experimental work, he had usually to associate with somebody else, or at least work in the presence of everybody else. So in that we knew what was going on. If he was building a counter, then we all knew it. But if he was trying a new approach to quantum electrodynamics, we didn't know anything about it.

Kuhn:

flow did that decision to turn to nuclear physics come about?

Rasetti:

Well, I think we all collaborated into it. Next to Fermi, I and Amaldi were most in favor of the change. Yes, Segre was the only one who was not convinced. He said, "Look, we have been doing very veil in spectroscopy. Spectroscopy is not exhausted yet. It is a field in which we are still sure we can reap some harvest. In nuclear physics we don't know anything. We can't tell an alpha particle from a gamma ray in this laboratory. We've never seen any experiments. We don't know a thing. It takes years before we start mastering the technique. Whereas in those years we have already the spectroscopic technique well in hand and we can produce some more work." Well, the conclusion, of course, was that spectroscopic work was not interrupted. Still papers were done after that time. It was, well, the hyperfine structures and the atoms with very high principal quantum numbers.

Kuhn:

What did you and Fermi say to Segre? What was wrong with Segre's idea?

Rasetti:

Well, we said that we thought that in spectroscopy there were no new basic phenomenon to be discovered and there were facts interesting in the details but no basic general idea. In fact, more or less, that's the way it was. Nuclear physics was more exciting. But Segre was definitely against it. Segre first became convinced of the interest of nuclear physics when he saw artificial radioactivity produced by neutrons. He had nothing to do with the nuclear group, so to speak, until then anyway. He was a latecomer in the nuclear group. Whereas Amaldi and I worked with Fermi from the beginning. Well, I had been for one year in Dahlem but even before that we had made counters, poor counters, poor cloud chambers, but we had started very early doing this work.

Kuhn:

You went to Dahlem deliberately to learn the techniques.

Rasetti:

Yes, because we realized that our techniques were very primitive. We had to learn to do things a little better. We had built counters already before I went to Dahlem. Fermi and I started glass-blowing, because in those times it was a do-it-yourself system. We were doing the glass-blowing ourselves. Fermi and I were blowing Geiger counters. Actually they were probably the first glass enclosed Geiger counters that were ever built. AU our knowledge of Geiger counters came from Rossi, from occasional contacts with Rossi. Rossi was making cosmic ray counters. In those times it was the tradition that counters were nude in a brass bite, with 2 Bakelite stoppers at the ends. You would stretch the wire through holes in the stoppers and then cover all the outside with picein. Then Fermi and I said, "Well, this is the dirty way to make counters. If a spectroscopist had to invent a Geiger counter, he would not do it that way. He would seal it in glass like any respectable discharge tube." And so we made counters sealed in glass. They worked poorly because that time the quenching mixtures had not been invented and so these counters were very critical. To some extent they counted. Probably they were the first such although extremely poor. At least I didn't know of anybody who built them that way. Everybody built them with picein and Bakelite or hard rubber. So we were aware of the interest in nuclear physics very early. These were things of about 1930. Then, of course, things got serious after I got back from Dahlem at the end of '32 because there I had learned how to build good cloud chambers and good counters.

Kuhn:

You were in Dahlem at the beginning of '32.

Rasetti:

I was there from October ['31] to July ['32]. I was there at the discovery of the neutron, at the time of Joliot and Chadwick. The discovery was in January or February, ['32]. … I was there and that is a sure fact.

Kuhn:

Tell me how that was received there.

Rasetti:

They were very much amazed. Everybody thought of the gamma rays, of Bothe and. Becker and didn't think there was anything else. Everybody was very much surprised. But we saw immediately, I think two or three days after we read the paper of Chadwick, the protons from the neutrons. From paraffin. We saw a photograph of the protons in a cloud chamber. So we accepted it immediately and we were very much surprised. Undoubtedly nobody had believed in the neutron.

Kuhn:

What about the positron, that, I gather, gave many people a lot more trouble.

Rasetti:

The positron I don't know because I had no direct contact with the experiments. I don't know in what season the paper of Anderson came out. Yes, we noticed it but we had no personal contact. Whereas in Berlin-Dahlem, Lise Meitner was very much interested. Then Bothe came there. I talked to Bothe about this. At that time radiation from polonium plus beryllium was thought to

consist only of gamma rays, which, of course, existed. So we were very much in the midst of it. I was closely associated with Meitner and I talked with Bothe so when the neutron came out it was very exciting. Weil, when the positron came out, I don't know. I was doing something else, so it was not so exciting.

Kuhn:

How did this movement, which got its real start in Rome with Fermi — first in Florence and then in Rome — spread out into other parts of Italy? Could you feel the effect where you were?

Persico:

I went to Turin in 1931 and I must say that in Turin it was not appreciated at all. I tried to make some seminars on Fermi's work, but they were not accepted. They were not appreciated.

Rasetti:

Neither beta decay theory nor the experiments on neutrons?

Persico:

No, but even the neutron work, the experimental neutron work, I tried to give expositions of it and. make some [lectures]. I also repeated experiments on radioactivity produced by neutrons in lectures, but I had the impression that people did not think it was important.

Kuhn:

Was it at least by now possible to put wave mechanics in the curriculum?

Rasetti:

We taught wave mechanics, yes.

Persico:

I suppose that theoretical modern physics was tolerated but experimental modern physics was not.

Kuhn:

That's very interesting and rather strange.

Rasetti:

[Persico] had a group of theoretical students.

Kuhn:

You said before that there would be one physicist and six mathematicians — now they're more nearly equal?

Rasetti:

Well, the six mathematicians are still there, of course. They haven't lost any jobs, any chairs, but the physicists have captured some. I don't think we've quite reached that quantity yet.

Persico:

Well, in Rome there are —.

Rasetti:

Six or seven, but that is exceptional. Most universities still have two or three. So most universities still have six or seven mathematicians and two or three physicists. Oh, the mathematicians have been established through a century of power. I don't mean that the mathematicians have too many positions, I mean that the physicists have too few.

Kuhn:

One last question about Fermi. Who did he think of as the leading figures or what did he think of the various people that other people thought of as the leading figures? I gather Born, for example, he found too much a formalist.

Rasetti:

Yes, he did not like Dirac's style. He acknowledged the greatness of Dirac's achievements, but he didn't like his style. He was not a physicist in his style — too abstract for him. He liked Pauli very much.

Kuhn:

How about Heisenberg?

Rasetti:

He liked Heisenberg, I would say, less than Pauli. Pauli was his style of theoretical physicist. He grudgingly appreciated Bohr. He always held a slight grudge against Bohr and the Copenhagen school because of the bad reception of his paper on the energy loss of fast particles in gases. He thought they had dealt unfairly with this paper. In fact, I think they had. He always bore a slight —. Oh, he was friendly with Bohr and the other people there. Moller, for instance, came here to Rome and they were very good friends. But he had quite a slight grudge remaining about that. But Pauli, I would say, was one of the theoreticians with whom he felt more akin.

Kuhn:

Well, in the case of Bohr, was his attitude also in part his own resistance to the philosophical approach?

Rasetti:

In part, he felt Bohr was too philosophical for his taste.

Persico:

Not about Einstein.

Rasetti:

I don't think I ever heard him discuss Einstein. Well, after all, special relativity, by the time Fermi was in a position to discuss these various physicists, was accepted am was not a field of work anymore. It was a sort of closed field. Fermi never had much interest in general relativity. He knew it; he knew all the tensors. In fact, he knew it so much that his first paper was on general relativity. It was his first field of work, he knew it when he was nineteen or twenty but afterwards he was absolutely uninterested. So I never heard him express any —. Of course, he recognized Einstein's greatness. I'm pretty sure that I hear that he expressed extreme admiration for the achievement of creating general relativity. After all, special relativity was in the air. If it hadn't been Einstein, after all, Poincare had the idea and Lorentz had the formulas in a way. It was a question of putting: the two together. Whereas general relativity really is a one-man creation. It is an amazing achievement. Fermi was fully aware of that.

Kuhn:

When you spoke of Fermi doing theoretical work in his head, which he obviously did, you also said, at five & clock in the morning. Did he work at strange hours?

Rasetti:

No, Fermi never worked late at night as far as I know. Never in his life. He would get up at six and work at home before going to the university. He was a man certainly the absolute opposite of the bohemian who keeps irregular hours. He was methodical, always came at the same hour, left at the same hour, had his meals at regular hours, and as far as I know, he never worked at night. It's against the tradition of most theoretical physicists. For instance, Placzek was the man who could only work from 10 o'clock 'til 3 in the morning or 1 in the morning. Then he went to bed and slept all day.

Kuhn:

Julian Schwinger also.

Rasetti:

I'm not surprised, yes. Mathematicians are like that, very often. At least if the Hopkins mathematicians are a fair sample, most of them work all night and sleep most of the day. But Fermi was extremely methodical, the opposite of the bohemian, I would say.