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J. Robert Oppenheimer

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Interviewed by: Charles Weiner

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Abstract:

Interview with J. Robert Oppenheimer, American theoretical physicist. In this interview, Oppenheimer talks extensively about Enrico Fermi, discussing Fermi's impact on physics, his research style, and his do-it-yourself attitude. Oppenheimer describes the importance of Fermi's later contributions in areas such as meson physics, as well as the development of Fermi's theory of beta decay. Oppenheimer also reflects on how the migration of European scientists, such as Fermi, to the U.S. helped close the gap between European and American physics. At the end of the interview, Oppenheimer shares his thoughts on the differences between the physics community of the early 20th century and that of the 1960s.

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Weiner:
Dr. Oppenheimer, a physicist can have an immediate impact on his own times, perhaps through a particular piece of significant work or through his influence on his colleagues. He also can have a long-range effect on physics, by probing new questions, developing new approaches, and perhaps by laying foundations for new areas of inquiry. In which of these senses did Fermi make the greater contribution? And how would you characterize both his short range and long-term influence?

Oppenheimer:
Well, you could ask the same question about more familiar and obviously somewhat greater men, like Newton. If you will answer that, I will answer it for Fermi.

Weiner:
Well, we—

Oppenheimer:
The answer is, both. And the same would be true of Fermi. His impact during his lifetime was very great; in the professional community, among physicists, he was deeply loved, imitated, as far as it lay in people's power to do so, by very many. He changed the style in which a whole set of problems were thought about and the language that was used. He had an experimental style as well as a theoretical one. Among physicists, this was something that was felt very soon. The world is full of physicists, unfortunately not full enough, who owe a great deal to the fact that Fermi was their senior or their contemporary.

For the world outside of physics, he was part of a chapter or chapters on atomic energy which are a part of history. A very central and very engaging part of this chapter. So that mark will certainly endure as long as we care about the things we care about today.

I can't answer, because I would have to compare it with someone else. I know of very few people who have had impact at the time which has not left some lasting mark. The opposite is of course not true.

Weiner:
Was he particularly effective in the small group approach to problems, working with a small group?

Oppenheimer:
Well, Fermi's best group was Fermi alone. That's what President Kennedy said about Jefferson. He worked well with small groups. He was very careful to seek people who were congenial, or to make sure that they became congenial. That's a style that was very common for him. But much of his work was done alone.

Weiner:

Was this part of the general do-it-yourself attitude he had, that carried over even into his experimental techniques and calculations?

Oppenheimer:

Yes. I think you saw that most strongly when Fermi lectured about or took an interest in a subject in which he'd never worked. He didn't do what most mortals do, which is read the literature and say, yes, he understood it, controlling a calculation here or making a connection there. He would essentially tear up the literature and do it over again, often in a way that wasn't quite as good as what he tore up, but surprisingly often, in a better way than it had ever been done before. And we owed the accessibility of quantum electrodynamics and a great deal that flowed from that to Fermi. He didn't make it. It was made by Dirac, by some other people, Heisenberg and Pauli and others, but it was made in a very awkward way, and put off all students, and Fermi made it so that ordinary mortals could understand and deal with it. That was because he wanted to understand and deal with it. His do-it-yourself quality applied not so much to the things he did himself, that's true with any good scientist, but to the things which he learned from others. There was very little he learned by the book.

Weiner:

Did he have a special domain of physics, or did he feel that all was fair game?

Oppenheimer:

Well, physics is an enormous word. We only learn from day to day how big. And probably nobody loves it all equally. But he had very wide potential interests and rather wide actual interests. In fact, anything that sounded as though it were an answerable question interested Fermi. And that included some questions which were not so answerable.

Weiner:

I'd like to skip to another area, and that is, on Fermi's feelings of his own competence, on the basis of his work in physics, to make judgments on political matters arising from this work or related to this work. Did he feel competent?

Oppenheimer:

This would require a symposium of people who knew him in Rome, and people who knew him well in the late thirties and early forties. I met him in the late thirties, I think, when he came and stayed with me, when he was giving the Hitchcock Lectures in Berkeley. But it was the first time we'd met, and although we then got to know one another, I wasn't an intimate of his.

My impression of Fermi at that time was that he had a rather robust confidence in the constituted authorities, and that he didn't put as answerable questions the very large matters of how should the world arrange itself to live with great technical change. He was also conservative and tended not to believe that ambitious plans for recasting patterns of human behavior would ever really amount to very much in practice.
But I knew him increasingly after he came to Los Alamos to stay, and in the postwar years. I would think that his initial approach to questions was a surprisingly no-nonsense, matter of fact, commonsense view, in which the fact that he was a physicist deepened his insight into all technical questions, but did not very much affect his views on human and political questions.

He had a good deal of trust that when the military said they had a requirement, they must know what they were talking about. And he had a tendency to suppose that those who in some vague sense were the government must be quite expert at that, as he was expert at physics.

I think he lost this confidence, not in himself, but in the fact that other people were just like him in different areas of human activity. And I think also that in some measure, I don't want to overstate this, he realized that some of the things that had come to him, many of them things that had come to several of us together, were pretty hard, and that he no longer felt entirely content with the way in which he had dealt with them. I know that he was content, discontent, and spoke to me later, of the casualness with which we had dealt with the so-called hydrogen bomb question. I know that some time later, perhaps in '51 or '52, I had been asked to persuade him to work on—be I guess the chairmanship—anyway, to work on the Atomic Energy Committee of the Research and Development Beard in the Pentagon. Others had approached him and he had said no. I talked to him about it, and he said, it was not only that he didn't want to be bothered and he'd done enough committee work, it was primarily…

Weiner:

Dr. Oppenheimer, what is the importance of Fermi's contributions from the late 1940's until his death in such fields as meson physics and other subjects in which he branched out in that period?

Oppenheimer:

Well, I have to start by saying, that the chain reaction was in the 1940's. And Fermi sometimes said, and I think deeply meant, that that was the thing that he was proudest of. This strikes people who have contempt for practice as a rather odd thing for a physicist to say. But it was very much in character.

I would say that the next thing which made a very big difference to the profession of physics was that he was the first to use adequate analytic quantitative techniques of measurement in the high energy physics domain. And this—when he first had an operating accelerator, he didn't begin, as some colleagues have been known to do, by letting the beam hit various things and reporting, would wonder what all happened, but he began by studying the nature of the various beams he could produce, and really got a controllable source of mesons, positive and negative. Then he didn't scatter the mesons on brass and sealing wax but on hydrogen, which was a great help from the point of view of theoretical understanding. In this way, he began to sort out different behavior of the collisions which were the same for proton-proton and proton-neutron, and those which only could occur from proton-neutron because of the exclusion principle, say, the isotopic spin, one and zero, components of the scattering waves. And discovered the—in this case, one half and three halves—and he discovered a good deal of structure, in the dependence of the scattering, these mesons, on the energy of the collision.

The first thing he discovered was that large scattering, in the state of larger isotopic spin common to like and unlike nucleons, and quite marked, which was very soon recognized by theorists as related to one means of approximately describing meson-nucleon interactions—the so-called strong coupling theory, which was started before the war, at least before this country was in the war. And this really gave a new point, a new lead and a new vigor to the theoretical description of strong interactions, and also set an example, a paradigm of what to do with accelerators, since no one could build an accelerator without thinking about what to measure. And the emphasis has not been on the technological problem of making a beam, but on the twin technological problems of making beams, understanding them, and exploring the consequences of their interaction.

So, although it could have happened in other ways, in fact Fermi was the first person to found modern high energy physical techniques in the laboratory. This gave rise to the great theoretical sequence of developments which enormously transcended in appropriateness the ridiculously inadequate methods that had been used, from Ukawa's time in '35 on to 1950 or so.

Weiner:

This work was at Chicago in the early fifties?

Oppenheimer:

Late forties and early fifties. It was—he very soon realized that he would like to get into the high energy physics field, and this was his first job on it, and he worked closely with associates, Herbert Anderson, Leona Marshall, many others.

He also did some theoretical papers. Almost everything he did has a continuing interest. But I do not think in the so far visible effect on physics, anything can compare with his pioneering the field of high energy strong interactions experimentally, and from a practical point of view, there are not very many things that compare with the first chain reaction.

Weiner:

This is all in the postwar period?

Oppenheimer:

Well, no, the chain reaction was in '42.

Weiner:

But this work you describe—

Oppenheimer:

Yes, after he returned to Chicago and had time to think a bit and to apply for the necessary funds and to build an accelerator, design it and to think what to do with it and to do it.

Weiner:

Fermi came to this country, backtracking a bit, on visits in the thirties, and then settled later in that decade. He was one of a large group of European physicists who came to this country in the late thirties and throughout the thirties. The state of physics in the 1920's and 1930's in this country has something to do, I think, with the reason—was one of the factors attracting people to this country. And it would be interesting to know what the effect of the arrival of the new group of physicists from Europe was on the American scene.

Oppenheimer:

Well, it was certainly one of two ways in which the isolation and the gap between European and American physics was essentially eliminated. One was through Americans who had studied in Europe and came back and decided that there was no reason that things like that shouldn't be going in this country, and went to work on it. And the other was through Europeans who came at the height of their powers to this country, and in order to have companionship, simply had to improve the state of discourse around them. I think Fermi was not one of the first. It may very well be true that he came several times before he moved here. But my recollection is that his coming permanently was for him the only decisive thing, and for us also more or less the only decisive thing.

I wouldn't want to try to sort out the effects of Weisskopf and Bethe and Fermi, and say, this one had so-and-so many points. But they all did an enormous amount, and the fact that they were all very good, and yet wonderfully adaptable, in the sense that they genuinely came to this country not as a temporary and resented alternative, but as something that could be lived with and enjoyed, made a great difference to American physics. I don't think it would have been so successful had there not been, by that time, a rather sturdy indigenous effort in physics. After all, a good deal of the invention in high energy physics, and in microwaves, and solid state, had got pretty well started in

America before anybody came.

Weiner:
This group of Europeans arriving really represented various styles and personalities.

Oppenheimer:
Oh, yes.

Weiner:
Fermi certainly represented one of a group coming from Europe, but he had his own talents, his own personality. Can you characterize his differences from others arriving, not in physics but in his personal—?

Oppenheimer:
Well, this is an enormous subject, if only because the others arriving, no two alike either, or at least I don't know…

Weiner:
How were the European physicists who arrived in this country in the thirties received by their American colleagues? I know that the Europeans probably had expectations of what the American scene was like, and the Americans probably had expectations of what the Europeans were like too.

Oppenheimer:
Well, I think there's no general answer. I was of course peculiarly Californian in those days. I saw a good many people, but I waited for them to cross the Sierra. And I very seldom came East. So I met Bethe on Mt. Rainier or something like that. And Weisskopf came to our ranch in New Mexico. And so there was a good deal that went on, shabby treatment, low salaries, indifference, which they may remember for the most part with good humor, but which does not do authorities in this country too much credit. We never properly, experts will tell you, had recovered from the Depression, and people still worried about dollars.

Locally, around San Francisco, two very eminent physicists arrived, both of whom have had rich and full careers in this country. Both of whom were for a time at Los Alamos during the war. Felix Bloch at Stanford was made very welcome, I believe had a good appointment. We went out of our way to extend our seminars to include Stanford, and there was the closest collaboration between the two groups—for instance, Nordsig who did a famous paper with Bloch was a student of mine. He was a student at Berkeley.

A few miles away, Segre came to Berkeley, who was more or less kept dangling for a few years, but Lawrence was persuaded that he was really pretty good after all. So it wasn't completely uniform. I have no doubt that similar tales describe Weisskopf's and

Plotchek's and Nordheim's experiences. I don't really know.

I think all of them were very glad to be here, and in the end got to be very fond of this country. Even Pauli, I think, left with something of a heavy heart to go back to Switzerland after the war. They all, I think, were right to appreciate what was good in this country, coming from Europe in the thirties. I think this is for me the dominant sense, thinking with a good heart and very eager to do the physics they knew how to do, and very anxious to have companionship in that.

Weiner:

This expectation on their part was perhaps different than the previous decade of the twenties, when Europeans had a different view of Americans.

Oppenheimer:

Well, it may be that they didn't pay much attention to it. I think the *Physical Review* was not a priority item. It came late and nobody rushed to open it. Europeans were aware of the fact that not inconsiderable characters occasionally appeared, like Willard Gibbs. But they discovered that very late, of course. And in fact, one of the important parts of quantum mechanics was done by Carl Eckhart when he was a young man, and done in a very beautiful way. It was not very widely noticed, and Schroedinger did the same thing, perhaps not quite as well. This was to show that the matrix mechanics and the wave mechanics were the same theory. I was in Europe at the time, and Eckhart's paper was noticed with amazement. I heard a good many disparaging remarks about Millikan and the cosmic rays. They were not necessarily limited to Europe. The main thing was not very active interest.

But you remember, Wigner was here quite early, and the work of Breit on proton-proton scattering might not inspire high blood pressure, but it just couldn't be ignored, and Lawrence's cyclotron was something that was very promptly recognized and imitated in Europe.

Weiner:

That was already the beginning of the new period and perhaps of the new feeling about it.

Oppenheimer:

It was one of the things. On the one hand, Rutherford had a contempt for the physics they did in Berkeley, which was warranted but extreme. But on the other hand, everybody felt this was a damn fine machine, and people were very glad to have the effort and help that Lawrence very openly, generously and warmly offered.

Weiner:

I'd like to jump back to a remark you made earlier about Fermi's robust conservatism, when you referred—in connection with some of his judgments on matters arising from the social application of physics.

Oppenheimer:

The anti-social applications.

Weiner:

A better way of putting it. But what about in his own work? For example, Fermi's work on beta decay—what was the meaning of this work and how did it reflect his personal style?

Oppenheimer:

Well, it was very good physics. It was a young man's physics, and if the word conservative is used for it, and it can be, it needs to be rather purely defined.

This was an offshoot of what I spoke of, Fermi's rewriting of electrodynamics. He rewrote it not only so that other people could use it, but so that he rather liked the subject. And you know that the fundamental process which electrodynamics describes is the emission or the absorption of light, a succession of such things, as in scattering. Nobody thinks of an excited atom as containing a light quantum, but it loses its excitation often, in general, by sending out a light quantum. And Fermi thought that this sounded very much like what a nucleus does, when a beta particle comes out of it. So he made his theory of beta decay to look like the theory of light emission, and in order for that to work, to satisfy the conservation laws of spin, angular momentum, he had to have something besides the electron, so the pair of the electron and the other thing, which he called neutrino, had more or less the same properties as light, as far as spin is concerned. And so he put that together in what was a very minimal alteration of the theory of quantum electrodynamics, and for a long time people said it was too simple, and they said it's not that way, and they fixed up other theories. All the changes were wrong, but it took approximately 20 years to find out that Fermi's primitive version, though not quite right, came closer to being right than all the improvements.

So it was fairly conservative in its execution, although the fresh idea, that you should not talk about these radiations as having been in the nucleus but as having been created by the nucleus—a very important change. It was also made timely by positron theory, where particles kept appearing and disappearing, in pairs.

Weiner:

The idea of a particle being created in the nucleus seems very daring, very bold.

Oppenheimer:

No more than a light quantum being created in an atom. You get used to it. Also, no more than a pair of particles being created near a nucleus by a gamma ray, which by then was a fairly well established process.

Weiner:
This is a field in which you were actively involved, along with a large group of physicists, at that time in the early thirties. In a sense it was a frontier field of physics, one that was exciting and challenging. How have these frontier fields changed? For example, today, what would you say the frontier of physics is?

Oppenheimer:
Well, the quality of the work is in general much higher today, and progress much less rapid, because the problems seem to be a bit harder. But when I think of the bright people that are working on the hard questions today, some few were Fermi's students, many have been through here for a year or two or longer—there was no such thing then.

Weiner:
You were speaking of differences in the contemporary scene in physics, the large number of people involved, and this is in response to my question about things that excite the physicist and challenge him. I know physics is always passing through a state of change, but do you think that this period is a special transition period?

Oppenheimer:
Of course, physics is an enormous subject, and I inevitably think about the part of it which of all has the greatest interest for me, and that is the laws of matter. We are obviously in motion, and there is an upward component to the motion. It spirals. It's confused. But we know more each year, and the things we know are not things that we supposed we would know. They are really rather new and fresh, and they correspond with increased ability to describe and within limits to predict. So I think it's a wonderful period. But most people half my age say they're too old to enjoy it.

Weiner:
In this period, the physicist occupied a different place than he has had in the past, and that is, from a relatively small private community things have changed, he has a different environment now. How would you characterize this difference?

Oppenheimer:
Well, I think we'd have to say, in what context we were talking. There's a good many things to say about it, most of them rather dull. Perhaps it would be fair to say that the community is very much larger. The number of physicists is very much larger. The ways in which they can occupy themselves, earn a living, play a useful part, are very much more varied than 50 years ago. The number of truly gifted men, who by their gifts and by character come somewhere near the phrase "great physicist, great scientists, great men" is obviously not going to be very much bigger today than it was in 1910. And it's

pretty hard to beat Einstein, Rutherford and Bohr, who were all on the scene at that time.

And so if you look with a dull eye, you think it's a much less distinguished company, because it's much more diluted. But if you look with some attention to what people are doing, I think you see the opposite, namely that gifted and imaginative and technically enormously resourceful work is very much more widespread than it was 50 years ago.

I believe that it is just for a man to decide whether to rejoice or to weep about this. But he musn't get mixed up about it. It's not the same world.

Weiner:

Thank you very much.

[End]