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J. Robert Oppenheimer

November 18 and 20, 1963

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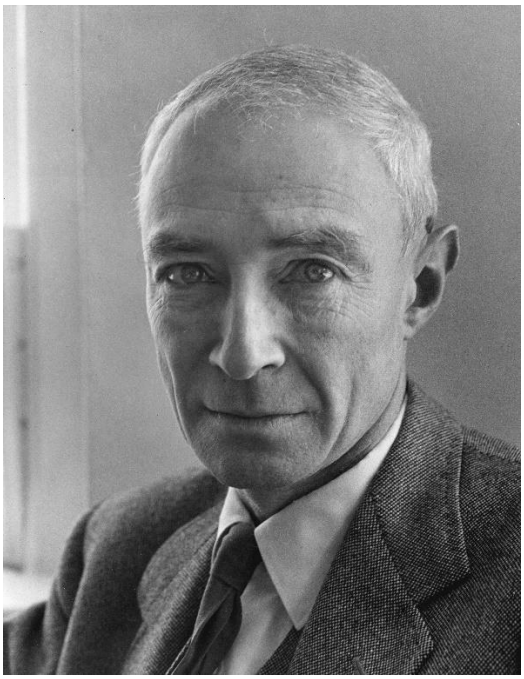

Abstract:

This interview was conducted as part of the Archives for the History of Quantum Physics project, which includes tapes and transcripts of oral history interviews conducted with circa 100 atomic and quantum physicists. Subjects discuss their family backgrounds, how they became interested in physics, their educations, people who influenced them, their careers including social influences on the conditions of research, and the state of atomic, nuclear, and quantum physics during the period in which they worked. Discussions of scientific matters relate to work that was done between approximately 1900 and 1930, with an emphasis on the discovery and interpretations of quantum mechanics in the 1920s. Also prominently mentioned are: George D. Birkhoff, Niels Henrik David Bohr, Max Born, Percy Williams Bridgman, James Chadwick, Edward Condon, Paul Adrien Maurice Dirac, Paul Ehrenfest, Ralph Fowler, James Franck, Werner Heisenberg, Ernst Pascual Jordan, Edwin Crawford Kemble, Arthur Klock, Hendrik Anthony Kramers, Robert Andrews Millikan, John Von Neumann, Lothar Nordheim, Wolfgang Pauli, T.W. Richards, Ernest Rutherford, Richard Chance Tolman, George Eugène Uhlenbeck, Whitehead, Eugene Paul Wigner; California Institute of Technology, Harvard University, Institute for Advanced Study (Princeton), Rijksuniversiteit te Leiden, Universität Göttingen, Universität Zurich, and University of California, Berkeley.

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[Begin Session 1, November 18, 1963]

Kuhn:

I'm looking for the stuff on molecular structure.

Oppenheimer:

That paper is Born and Oppenheimer, *Annalen der Physik*, and it's probably in '27. ["Zur Quantentheorie der Molekeln" *Ann. D. Phys.* 84 (1927) pp. 457-84.] We have it. We have a complete bibliography and we will give it to you. One of the few things about which I have an amusing story to tell.

Kuhn:

Good. You said that you found, in thinking about these things, that your memory on detail was not what it might have been and so forth. I'd just like to say, let's get as much of that as we can, but I'd like you to think of one of the topics that interests us, no necessarily the primary one but one which is pretty obviously of a good deal of importance; I think you can help with a lot and it's not so dependent upon details of memory about technical papers. It is the whole subject of the development of physics in this country and the turning point with respect to theoretical physics that you come in so much with yourself.

Oppenheimer:

I will do my best.

Kuhn:

All right. I'd just as soon go back as close to the beginning as you will go.

Oppenheimer:

Let's do that briefly so we have it, and if what I say is inadequate, I'll see if I can do more. Actually, perhaps by talking about it, I have a fairly vivid recollection of how this set of interests started. I was given a perfectly conventional tiny collection of minerals by my grandfather who was living in Germany; I was there at the age of five and the age of seven, but I do not know which of these two times it was. From then on I became, in a completely childish way, an ardent mineral collector and I had, by the time I was through, quite a fine collection which I gave to Pauling the first time that I spent some time with him when I was National Research Fellow at Cal Tech. He was then still stuck with crystals—inorganic crystals—so that he not only used them but he was very pleased at these enormous calcites and God knows what all. This was certainly at first a collector's interest, but it began to be also a bit of a scientist's interest, not in historical problems of how rocks and minerals came to be, but really a fascination with crystals, their structure, bi-refringence, what you say in polarized light, and all the canonical business. I would say that, if you look at a period when I was 10 or 12 years old, minerals, writing poems and reading, and building with blocks still—architecture—were

the three themes that I did, not because they were something I had companionship in or because they had any relation to school but just for the hell of it. I gave up the blocks probably at the age of 10, and the minerals became a charming preoccupation. I started trying to understand them. I had very great trouble because I didn't always know the vocabulary; I think it was a month before I realized that "intercept" could be used as a noun as well as a verb and this was the bane of me.

Kuhn:

You were reading then; your tool for getting at the next stages was reading?

Oppenheimer:

It was entirely a book. I was a member of the Mineralogical Club very early; I was made an honorary member a couple of years back in what turns out to be a hell of a select society. But I didn't learn anything from the Mineralogical Club; their interests were in showing new finds. The president was Kunz after whom the Kunzite is named. This caused me then and later to seek opportunities to go to diggings and finds and travel.
I think the most important change came in my junior year in high school. I went to what was then called the Ethical Culture School where the teacher of physics and chemistry was Augustus Klock; he died a month ago and I think the services for him were held last week. He was marvelous; I got so excited that after the first year, which was physics, I arranged to spend the summer working with him setting up equipment for the following year when I would then take chemistry [noise] both. We must have spent five days a week together; once in awhile we would even go off on a mineral hunting junket as a reward for this. I got interested then in electrolytes and conduction; I didn't know anything about it but I did fiddle with a few experiments although I don't remember what they were. I loved chemistry so deeply that I automatically now respond when people what to know how to interest people in science by saying, "Teach them elementary chemistry." Compared to physics, it starts right in the heart of things and very soon you have that connection between what you see and a really very sweeping set of ideas which could exist in physics but is very much less likely to be accessible.
I don't know what would have happened if Augustus Klock hadn't been the teacher in this school, but I know that I had a great sense of indebtedness to him. He loved it, and he loved it in three ways: he loved the subject, he loved the bumpy contingent nature of the way in which you actually find out about something, and he loved the excitement that he could stir in young people. In all three ways he was a remarkably good teacher. I think this is a generally held view.

Kuhn:

Were there a number of other people in this school who later showed up as professionals whom he produced?

Oppenheimer:

There were some. Two names come to mind and later there would be others. Well,

three names come to mind: Ted Sterne, an astrophysicist, Lewi Tonks, who was with General Electric for many years and got into the nuclear power business recently, and, in a different key, my brother went to the same school and did eventually turn into physics by a perhaps somewhat more circuitous route than mine. I don't know. I would mention one other episode which I remember at high school. I should admit that in all those days I was always formally a very good student which I guess I enjoyed but which fogs everything because I don't have any recollection of real difficulty. I wrote an essay for an English class and I wrote it about oxygen. My teacher who was also a remarkable man and later became a very prominent educator, said, "I think your vocation is to be a science writer." Well, I didn't think so, but I found it interesting in retrospect.

I didn't get to go to Harvard immediately after graduation; I was in Europe and I went off on a long prospecting trip into Bohemia into the old mines, Joachimsthal, and I came down with a heavy, almost fatal case of the trench dysentery. That and its aftermath kept me out of school for a year; it would have had to keep me out for a half a year and I guess it seemed more practical.

Kuhn:

You had planned to go to Harvard?

Oppenheimer:

I had planned to go immediately but it was called off because I was sick abed—in Europe, actually, at the time. In the spring and summer it seemed like a good idea and I travelled first in the south and then I the summer out west and for the first time got to know New Mexico, Colorado, California, and (crossed into) Mexico. Then I did go to Harvard; I remember talking to some older colleagues as to whether I should study chemistry or should be more addressed to the problems of mineralogy with the idea of becoming a mining engineer because I loved that kind of life. I don't remember who it was, but one of my friends said, "Study chemistry; there are always summer vacations."

Kuhn:

Before you talk much about Harvard let me ask you just a bit more about the earlier period. How did your family in general feel about the extent of your interest in science? Was it not clear fairly early that this was the greatest of the interests?

Oppenheimer:

I think that my father was one of the most tolerant and human of men. His idea of what to do for people was to let them find out what they wanted. I think that both he and my mother were pleased that I was a good student, were pleased that I was highbrow, were perhaps somewhat mockingly proud of my vigor in collecting and learning about minerals, and were quite content with that aspect of my life. I think my other especially was dissatisfied with the limited interest I had in play and in people of my own age, and I don't remember exactly what years but I know she kept trying to get me to be more

like other boys, but with indifferent success. My mother had a reasonable education but she was a painter and her real interests were not scientific; we never, as far as I know, talked about this. I may sometimes have tried to explain something miraculous, but I have no concrete recollection.

Kuhn:

Would your grandfather have been interested in science? I mean, is there anything particular about this gift of a mineralogical collection?

Oppenheimer:

I wouldn't know. I saw him for the last time when I was seven and he was then a very old man. He was an unsuccessful business man, born himself in a hovel, really, in an almost medieval German village, with a taste for scholarship. He gave me also, in addition to the mineral collection, an encyclopedia of architecture which I still have. It was clear that one of the great joys in life for him was reading, but he had probably hardly been to school. He knew that I was interested in architecture and so he gave me these books; what caused him to give me the mineral collection I don't know—perhaps just because it was the sort of thing one gave to a very young child. And it was nothing; it was a box with maybe two dozen samples labelled in German.

Kuhn:

Was there anybody else among your schoolmates, or anybody else, that you did this sort of thing with?

Oppenheimer:

No, I shared an interest in poetry and literature with several people. I had a very good friend who came to this school from New Mexico perhaps in his sophomore or junior year and who at that time had some interest in biology. But his main interests were really a young man's philosophic interests; he was preoccupied with the old difficulty that if everything is natural how can something be good, in the form which the 19th century writers had sharpened this. He is to this day one of my closest friends and our paths have crossed often. He did study biology at Harvard, but then he went to Europe with a Rhodes Scholarship where he discovered that it was respectable to study the arts and became a producer, a writer of plays, a critic, and a historian of the arts—the theater primarily. His name is Francis Ferguson. He would normally be here; he's now travelling around the world, but you'll meet him next year. Is it time to go on to Harvard now?

Kuhn:

There are just a couple of other things in this same area. As you went from the collection of minerals as such to both crystallography and properties like bi-refringence, did you begin to do laboratory work? And if so, at home or at school or…?

Oppenheimer:

I had a microscope and I had a polarizer; that limited what I could do at home except for the most primitive sort of field tests of minerals. I think I didn't complicate physics and chemistry at school very much with mineralogy.

Kuhn:

How did you feel about the laboratory aspect?

Oppenheimer:

I loved it. This whole bifurcation belongs to a later period. I think if you had asked me then, I would have been somewhat doubtful whether there was such a thing as a vocation of science apart from mining engineering, chemical engineering or teaching, but if I had though about it, I'm sure I would have thought that it was inseparable from doing experiments because I still thought that three or four years later.

Kuhn:

When you did elect in advance presumably, or slightly in advance, to do chemistry at Harvard, was it then on the whole the presumption that you would be a teacher?

Oppenheimer:

I expect that that wasn't a question that crossed my mind. It was a desire to learn which probably was much, much too uncalculated, but what I really wanted was to study physical chemistry because this was the thing whose glimmering I liked in elementary chemistry. I should add—I think it's only honest to add—that at school and at Harvard I learned a lot of things that had no immediate connection with chemistry or with physics. I learned Greek at school which was even then somewhat exotic and perhaps, if I had been asked, I would have said that that and chemistry were the two high points; and I continued doing things like that at Harvard on a quite massive scale so that the notion that I was travelling down a clear track would be wrong. I determined to get a mastery of French and its literature which I knew very poorly; I had a very exciting time reading *The Principia* with Whitehead, who had forgotten it, so that he was both teacher and student.

Kuhn: He didn't ordinarily teach it at Harvard, I take it?

Oppenheimer:

When people came along and asked him. I must have been the first—or *we* must have been the first—I was not alone but there were just two of us. I had the very good luck not to take but to audit [G. D.] Birkhoff on Sturm-Liouville equations, because he'd been working on it and wanted to talk about it.

Kuhn:

Did he talk well about it?

Oppenheimer:

He was a remarkable fellow. He would begin: "Well, you know, walking across the yard this morning it occurred to me—" And that would be the beginning. But we had this admirable book of (Bocher), and in a curriculum in which mathematics was mostly very poorly taught; this is the only class that I remember with happiness.

Kuhn:

I will press you on this a little bit, lightly, because I had him for third year calculus in sophomore year, I guess…

Oppenheimer:

This was a graduate seminar.

Kuhn:

This was the first time that anyone had ever managed to make mathematics difficult for me, and it wasn't difficult mathematics; it was partly that the book was bad… On the other hand, Birkhoff was generally regarded as a terribly bad teacher in my day and I wonder how much of this you were getting from him—I don't mean to be—

Oppenheimer:

I would say generally that at Harvard I did a great deal; the maximum number of courses you could take was six but I audited two or three more and this third year, my last, I was also spending a great deal of time in Bridgman's Laboratory, so that it's obvious I was getting a smattering and very superficial introduction, and that will become even more clear when I tell you what went on in physics. But I would say that it was quite helpful; I knew very little mathematics—I still do—and it was quite helpful that I had had this exposure in what must have been the spring of '25 or maybe the fall of '24 in view of what was to happen in physics and it wasn't that little.

Kuhn:

I'd like to be clearer if we can, particularly because I had wanted to ask you about math, but also just in general: what level had you reached by the time you got to Harvard, either in course or by yourself in various sciences?

Oppenheimer:

I will try to remember. I would say that apart from this quirk about mineralogy and the abnormality of Greek, I probably had what was then a good high school grounding and damn little more as far as knowing anything. I knew what the calculus was, I knew what analytic geometry was, but I had had no formal training in it. I had probably looked at some texts. About physical chemistry I had read, but in a completely unenlightened way,

a great deal more than one normally would but again with no discipline and no coherence, so I think you could say that when I went to Harvard I was as well formed as the high schools of the time formed people, but not any better.

Kuhn:

Do you remember any books in particular that would have carried you into advanced subject matters?

Oppenheimer:

I do, but I don't always know the time. I have a book—I saw it yesterday—which is salt-encrusted; it's Jeans' *Electricity and Magnetism* and it's clear that I studied that when I went sailing in the summer. That could have been the summer before I went to Harvard. No, it couldn't have been that summer because I was West, and the summer before I was in Europe—I imagine it was between my freshman and sophomore years. My guess is that it was the arrival at Harvard, the free availability of the whole library—which happened very soon—that suddenly got me reading very widely; I would think so. I don't remember too well. I'm pretty sure that the summer in Europe I didn't read a great deal and the summer out West I didn't read a great deal, and I don't think I could have done this before, so it must have been after. Funny books, you know! I mean, Jeans isn't funny, but *The Dynamical Theory of Gases* was the first of his I read, I would guess, again, my late freshman year or between then and my sophomore year.

Kuhn:

Those are both highly mathematical books and you would have had to have read a reasonable amount of calculus to be able to do much with them and to—

Oppenheimer:

I would think that I knew enough then; there may have been dark places—no, I would think that I knew enough. I certainly could deal with the calculus before I went to Harvard but I had no official awareness of it.

Kuhn:

What did you actually start out by taking at Harvard, do you remember? You were, at this stage of the game, going to be a chemist?

Oppenheimer:

Yes, I was going to be a chemist and I graduated in chemistry. I thought that, as far as that part of life was concerned, I would go as fast toward physical chemistry as I could, and I must have taken qualitative analysis, quantitative analysis, gas analysis—these things in which Harvard had the great specialists: [T. W.] Richards, [G. P.] Baxter, and so on—organic chemistry, but I— [interruption]

It wasn't until my third year at Harvard that I did get to take physical chemistry and by

then it was a great disappointment. I remember Richards saying, "Nature loves a logarithm." We could look this up but it's probably not important. I don't remember what I did in my freshman year; I took this double course in French which was the last I was to take. I must have had to take a standard English course; I must have taken—probably not with Osgood thought once I did—the introductory course on calculus. I would have taken qualitative analysis which, I guess, is the first thing on the chemistry; I took no physics; I took philosophy A which was then a really very good course and had a nice time with it. I had as my instructor Ralph (Eaton), a wonderful man. I was part of a little science club which was partly faculty but mostly graduate students and which talked about scientific questions and philosophical questions related to science.

Kuhn:

Who was in that?

Oppenheimer:

Well [W. J.] Osterhout I remember very well; and the taxonomic botanist who is very well known, although I've forgotten his name; Jeff Wyman was a member; Francis Ferguson was a member.

Kuhn:

Was this a continuing group or was it one which had organized itself while you were there?

Oppenheimer:

I think it may have existed a year earlier or it may have come into existence that year; it existed two years of the three I was at Harvard. I think it didn't continue beyond that. John Edsall was probably a member. (Weyerhauser) was a member—I don't know what became of him. It's not good for me to try to scoop this barrel out because I won't get it right. I can't recall how it came over me that what I liked in chemistry was very close to physics; it's obvious that if you were reading physical chemistry and you began to run into thermodynamical and statistical mechanical ideas you'd want to find out about them.

Kuhn:

How much of that would you in any case have gotten from Richards?

Oppenheimer:

Well, I didn't run into Richards until the third year there and then very little; it was a very meager hick course in physical chemistry which would not get by today anywhere. But I didn't know that until I got there.

Kuhn:

I take it, but I may be quite wrong, that it was not a very physical physical chemistry course.

Oppenheimer:

It was formal, and tentative and timid; Richards was afraid of even rudimentary mathematics and very timid about non-phenomenological descriptions of things. I remember it but that was not what was really going on by then. Now how this happened I don't know, but I must have started reading physics in a major way; I don't believe that I audited a course, I don't believe that I went to any seminars in physics that first, freshman year, but I petitioned the physics department for graduate standing.

Kuhn:

How early?

Oppenheimer:

That must have been in the spring of my freshman year. I was not as young as I would have been if I had gone straight to college, and this was granted.

Kuhn:

This meant what?

Oppenheimer:

That I could take graduate courses, and also, as it turned out, that I could work in Bridgman's Lab although that wasn't to happen until the year after. It wasn't that I was a candidate for an advanced degree, but in essence, they stipulated that I knew something about physics. This was—

Kuhn:

Were you asked to demonstrate this?

Oppenheimer:

I believe that what I did was to write a list of books that I had read and to indicate the nature of my interest and curiosity. And I can only quote a story—it's maybe apocryphal—but years later I was told that when the faculty met to consider this request, George Washington Pierce met with them and said, "Obviously if he says he's read these books he's a liar, but he should get a PhD for knowing their titles." I don't know—

Kuhn:

That's a nice story. Had you already, do you suppose, on that book (list) begun to read things that would get into the area of quantum mechanics, or would this be classical physics?

Oppenheimer:

I think that I came into this through electron theory which I know was a very high preoccupation; I read Richardson's book and now it's going to be difficult—I think not strictly possible—to sort out the temporal sequence of things. I did take graduate courses in physics, of which the two that meant most to me were Bridgman's thermodynamics and statistical mechanics and Bridgman's electromagnetic theory.

Kuhn:

These would be in the third year?

Oppenheimer:

I took them the second year. No, obviously not—he would have given them in alternate years. Anyway, the two years, and I think that I started the thermodynamics with Kemble, and I took a course on wave theory with Slater, which is why I also knew about the Bohr-Kramers-Slater theory. Slater came back to Harvard before I left in '25. He gave a seminar.

Kuhn:

But he wouldn't have been back until the late summer of '25, would he?... I think his year away was definitely '24-'25 rather than '23-'24… The fall of '25 would have been the first point at which he could have been teaching at Harvard.

Oppenheimer:

I think he may have come back for a visit because I know he gave a seminar. Or well, that may not be so. I did take a course in wave theory from him. He must have been there. But he knows that. I don't know when he returned; he was not there, he was away, when I first began circulating in the physics department. Well, perhaps I met him later and someone else—Van Vleck, maybe—gave a paper on his stuff, but I have this double recollection of the ghost fields and of the course on wave fields which he gave—a half course. I find it hard to think that I would have taken that as a post-doctoral student.

What else did I study? I took a rather poor course in colloid chemistry from one Coolidge and a terrible course in probability from another Coolidge, [J. L.]. I had an advanced calculus from [W. F.] Osgood. I don't think I did anything with geometry. I continued this chemical line, and I won't try to remember all the things I audited—

Kuhn:

I'm sorry, I'm wrong. I slipped a year; Slater would have been back in the fall of '24.

Oppenheimer:

I think he wasn't there the first year I was around and was there the second, but I'm not sure… I found Bridgman a wonderful teacher because he never really was quite reconciled to things being the way they were and he always thought them out; his exercises were a very good way to learn where the bones were in these two beautiful parts of physics. I think as far as science goes, they were the great point of my time at Harvard; but I can't emphasize strongly enough how *much* I read and more really just in exploration. It's a very off picture, you see; I've never had an elementary course in physics except for a very elementary school course and to this day I get panicky when I think about a smoke ring or elastic vibrations. There's nothing there—just a little skin over a hole. In the same way my mathematical formation was, even for those days, very primitive, and this was more than evident in the way I went about some of the things I did later. And very eclectic. I took a course from [J. E.] Littlewood on number theory—well, that was nice, but that wasn't really how to go about learning mathematics for the professional pursuit of physics.

Kuhn:

This was when you were in Cambridge?

Oppenheimer:

That's right.

Kuhn:

With Bridgman, or more generally with this science club, what role was played by philosophical discussion? How much were people at this point influenced? How developed were Bridgman's own points of view? How did one feel then about operational aspects?

Oppenheimer:

Well, he didn't articulate a philosophic point of view, but he lived it, both in the way he worked in the laboratory, which, as you know, was very special, and in the way he taught. He was a man to whom one wanted to be an apprentice. There was, of course, wide-ranging talk of all kinds of philosophical problems on the Harvard campus. I would say that I probably participated in that rather less than most undergraduates. I had a few very close friends, but we really all of us were working almost to the limit of our capacity and, I would have thought, not talking too much. There was a thing called the liberal club which I guess was founded when I was there; I think Henry Cabot Lodge was a member and I was a member. I don't know what that was all about; I felt very much a fish out of water, but I wrote something for them.

Kuhn:

What sort of thing did you write for them, do you remember?

Oppenheimer:

No, I don't. I know that I gave a name to their magazine which was called *The Gadfly*, but I don't remember what I wrote.

Kuhn:

Did you do any quantum mechanics with Kemble? He, I think, was giving *the* course in the subject at that point.

Oppenheimer:

Well, I must have, because I knew about it. I don't remember it as vividly as the other courses. I know that the first course I ever took at Jefferson was thermodynamics which Kemble was giving and I suppose the next year I took quantum mechanics. I knew about it and I think more than I would have and much more securely than I would have if I had just read about it. I know about action and angle variables adequately and I knew about the rules of quantization, something about the problems of their collapse. It's going to be hard for me to be sure where I learned things, from what book, but I'd like to give a clear impression of a very quick, superficial, eager familiarization with some parts of physics with tremendous lacunae and often with a tremendous lack of practice and discipline, just because the time was limited. And although I liked to work, I spread myself very thin and got by with murder; I got A's in all these courses which I don't think I should have.

Kuhn:

You speak of having taken the double course in French and having ended up your French at that point. Had you had German all along?

Oppenheimer:

Yes, and I had had some French, but I didn't know the literature well and I couldn't write it fluently.

Kuhn:

Did German come from the home, or had you studied this also?

Oppenheimer:

I had studied it. We didn't talk it at home; my mother didn't talk it well, my father didn't believe in talking it. But I had been to Europe a couple of times. I learned a little French and something of how it sounded from a governess at one point. I never went back to studying Greek but I continued to read it. Latin I only got to read again much later; it's an old man's language.

Kuhn:

When you speak now of reading very widely, would you have been reading stuff in French and, more particularly, in German?

Oppenheimer:

Oh, yes. Scientific things I would read in whatever they came in, but literary things I would be reading in French and a little in Italian. One of the courses I audited was on Vita Nuova with a very grand old man called [C. H.] Grandgent; he was old even then.

Kuhn:

Would you suppose at this point you were reading things of Sommerfeld?

Oppenheimer:

I had it, I'm sure, although I had a tropism for the slightly old-fashioned English texts; I think Richardson and Jeans I mentioned not by accident. They were not the only ones but they were typical.

Kuhn:

Still it would have been hard to get much quantum mechanics from old-fashioned or even new-fashioned English texts.

Oppenheimer:

It would indeed. I think it true that in French and German the languages never got in the way at all; I cannot say that of Russian (or of many other languages).

Kuhn:

Do you suppose, for example, you read Bohr's "Quantum Theory of Line Spectra" and the Kramers follow-up on it—

Oppenheimer:

Not Kramers at that point, but Bohr, yes. When did Bohr visit the United States? Didn't he visit in '23 or something like that?

Kuhn:

I'm not sure when his first visit was. It may well have been then.

Oppenheimer:

I think that I heard him speak, but I, of course, wasn't ripe to appreciate it and I didn't meet him.

Kuhn:

There are, of course, a lot of things happening in the whole area at the time you were in college reading and learning about it.

Oppenheimer:

I didn't learn about quantum mechanics until I got to Europe. I didn't learn about electron spin until I got to Europe. I don't believe that they were actually known in '25 in the spring in America; anyway, I didn't know them. I wouldn't have said that I had a clearly defined interest in quantum theory. I was interested in it but it wasn't separated off. Insofar as I had an interest it was still very much on electron theory and it was because of its possible connection with that and my enormous joy and admiration in Bridgman that I worked in his lab as an assistant really, but very badly, but I think perhaps during the whole—at any way, during the second half—of my third year there. I'm not quite clear whether it was a whole year.

Kuhn:

We don't have to go as far as electron spin; you speak, for example, of having heard from Slater about the Bohr-Kramers-Slater theory.

Oppenheimer:

That was in the seminar and I would, I think, have felt that this was pretty much over my head.

Kuhn:

Do you remember how people around reacted to that whole way of looking at things?

Oppenheimer:

I remember not liking it. This could have been because somebody else said that, but I don't think so. I think I didn't like it. I didn't like the looseness of the relation between the waves and events.

Kuhn:

Electron theory could at least have gotten you much interested in the photo effect, in the whole subject of the photon?

Oppenheimer:

Yes—I don't want to make it up—but I think that I was interested in what the hell the electrons were doing and above all in the phenomenally great conductivity of metals when you tried to think of it in terms of kinetic theory; this seemed to me so puzzling that I thought it was the thing I would like to look at, but I didn't know how.

Kuhn:

Well, you surely were there at Harvard at the time of the Compton effect. This was the time presumably that Duane was vigorously denying that the effect existed and saying that he couldn't get it in his experiments.

Oppenheimer:

I wasn't involved in the controversy. I knew about the Compton effect and I know about light quanta and I knew about the duality, but I wasn't putting much together and I was not at all involved in this controversy. In fact I don't think I heard about the recoil experiments until I was in Cambridge. I don't know when they were done.

Kuhn:

Certain of them were immediate follow-ups of the Bohr-Kramers-Slater paper, so that the Geiger part were done late in '24 I think, the Compton-Simon experiments in early '25, at the time when you still would have been there.

Oppenheimer:

Yes, but I'm not sure. I probably heard or overheard a lot of things that I only partially understood and they didn't enter into me.

Kuhn:

So at least the sense of excitement, crisis, imminent change that existed I think not in many places in America but that surely existed in a number of places in Europe was not something you were particularly aware of?

Oppenheimer:

No, if I'd been asked if this was a "hot time" I'd probably have said no more than what was normal. I had—

Kuhn:

What did you feel just about that sort of basic issue of the conflict between quantization and classical theory, just the level at which the Bohr atom is itself a paradox?

Oppenheimer:

I think that I was still in the position of someone who had a lot to learn from others, and it wasn't yet my job. I wanted to understand what other people thought and knew; I was still, in the bad sense of the word, a student, I think.

Kuhn:

Still, I'm curious about—You clearly did know these things, and this was clearly an unusual sort of contradiction to exist within physics when contradictions of this sort were not generally thought to be a standard part of physics and this one was one, in a sense, at a very elementary level.

Oppenheimer:

Right, and I was surely aware of it, but I don't think that I felt responsible for it.

Kuhn:

More than that, your awareness of it was compatible with your saying 'physics is not in any hotter state than usual.'

Oppenheimer:

Well, this already implies what for Harvard in '24 and '25 was not true; namely, an awareness of the theoretical picture on a grand scale. I don't believe that existed. Maybe Slater had it, but my relations with him were those of an auditor and student. I knew he had been involved in something that might be important; I knew that I didn't like the smell of it; but I'm not sure John Slater had that feeling either, and if he did, I'm not sure that he communicated it very widely. I don't even remember who were graduate students at that time and with whom else I might have been talking. I think [M. E.] Hufford probably was there about that time, although that may have been later. He also worked with Bridgman, and a man called [F.] Crawford stayed for quite awhile. But if there was an atmosphere of critical judgment and of general discussion, it was not one to which I penetrated; and my guess is that Bridgman wouldn't have been thinking in those terms. Kemble was aware that this was very strange—I would have learned from him about that—but I'm sure Kemble never thought that it was going to be fixed quickly because I remember a year or two later when he really saw that it had been fixed, he said very sadly, "It's all over."

Kuhn:

Where was he then?

Oppenheimer:

That could have been in Europe that I met him, but it could also have been on my return to Harvard, although I think not.

Kuhn:

I have the feeling that this is something he realized in Europe. I have talked a bit with him, and he was trying still at that time to handle a problem that had interested him for a long time by the old methods, not really aware that the time had been reached when you had to use the new methods or nothing.

Oppenheimer:

In any case I think it's fair to say that he expected to live a very substantial part of his life fighting in those trenches and no doubt I had no reason to question that. And remember at this time two things: one, I was a student and there was an awful lot that people had found out in physics and in many other parts of the human scene, and I was impatient and hungry to find out what that was, which is the opposite of what it takes to study nature. I wasn't ready to and this because acute and critical the following year. But—

Kuhn:
Did you think of that as a next step, as one you were eager to get to, or were you in the state of mind that would have gone on learning?

Oppenheimer:
Well, I think that this wasn't clear to me. It became acutely and painfully clear the next year, but I think that the fact that I went to squeeze metals and to measure their conductivity and continued to study and to read was an expression of what I thought; namely, you find out what's known, what has been found out, and you get to work trying to do some experiments you find to be interesting, without any kind of firm notion of the connection between one and the other. I don't know, but I don't think I could have behaved that way otherwise.

Kuhn:
You've referred already to the year at Cambridge and we might get to that. I'd love to know how you picked Cambridge and also whether you would straighten out that life for us.

Oppenheimer:
I don't know why I picked Cambridge, but I wanted to go to Rutherford's laboratory. I know I talked to Bridgman about it.

Kuhn:
You had no real sense yet of a growing separation between theoretical and experimental—? That there was theoretical—?

Oppenheimer:
No, none whatever. I didn't know you could earn your living that way, and I don't know if I had known it whether it would have seemed right to me.

Kuhn:
I take it you mean more than the practical sense of whether you could earn your living, but—

Oppenheimer:
Whether it was a way of life. Why did I want to go to Cambridge? I suppose that some of those English texts may have had a relation to it. I know that Rutherford did. When did Bridgman go to the Solvay Congress? Was that '24? He talked about that.

Kuhn:
Yes, I'm quite certain he was there in '24 but again I would have to look it up.

Oppenheimer:

That's all right, but he talked about it and he was very much impressed with being presented to the King, all right, but he also was very much impressed by what he learned of English physics at this meeting. It was a congress on electrons and metals. I wanted to go to Cambridge to work in Rutherford's laboratory, but Rutherford wouldn't have me. He didn't think much of Bridgman and my credentials were peculiar and not impressive, and certainly not impressive to a man with Rutherford's common sense. But then it was arranged, I think by Rutherford, that I could work in Thomson's lab.

Kuhn:

Why, with your interest in electron theory as such, did you want to go to Rutherford rather than Thomson in the first place?

Oppenheimer:

I don't know. Perhaps because I thought that he was in charge of a great laboratory.

Kuhn:

For electron theory, Thomson would presumably have been the man to go to, unless one thought that he was already out of date.

Oppenheimer:

Let me tell you what I vaguely had in mind. It was not a good idea, but I was puzzled by the conductivity business and I thought I would like to study what happened with beams of electrons and thin films of metal. This was not electron diffraction; it would have, presumably, if done properly, had led to it. But I wouldn't have been stubborn if someone had said, "Why don't you do this? It's much more interesting." I'm quite sure I wouldn't have been pig-headed about it, but this is sort of what I had in mind. And this is not something which Rutherford would have frowned on, I think; at least I can't reconstruct it more clearly than that. It didn't occur to me to go to Germany. It didn't occur to me to go to Copenhagen. I don't even know why I left Harvard, but I somehow felt that that was more near the center.

Kuhn:

You did go to Thomson's lab then?

Oppenheimer:

I did.

Kuhn:

Did you have a fellowship or did you do this on your own?

Oppenheimer:

I didn't have a fellowship till I took my degree. I had a fellowship at Harvard every year, but because I could get along well without the money they didn't pay me. And I may have had a fellowship of the same kind, because I didn't take my degree in '25; I stayed with my class, but I simply completed the requirements for it.

All right, can I do anything useful about Harvard? I'm going to tell you one episode which may be a measure of how this two years of scrambling had put me in physics. I really read a lot and the day I was to leave—maybe waiting for an exam, maybe after they were over—I went back to Widener [Library] again and I took down a book that I had seen and passed over many times; that was a book by T. J. J. See, *From (Starlight en route)* which was his relativistic electron theory. I looked it over quickly and burst into a sweat because it was clear that it was crazy but it was so similar in the way it talked and all the externalia to everything I had been studying that I just got scared. This I think indicates that I did know quite a bit of physics, but it's something I remember and most of the rest I don't.

Kuhn:

That's very interesting. Was See an issue? I mean, was that book taken seriously at the time at all?

Oppenheimer:

I'd never heard of it, but I had passed it and decided I had other things to do.

Kuhn:

He occupies a considerable section in the Widener card catalogue.

Oppenheimer:

And then this hot June day when I was probably finished with exams I thought I'd look at something that was different; I may have heard his name but I have no recollection of it, no recollection of ever discussing it until some years later. I just regarded it as a personal aberration.

Kuhn:

I take it from the way you talk about this that in going through this at Harvard you were doing this pretty much still by yourself; there was nobody else except to the extent that in your last year you were working closely with Bridgman.

Oppenheimer:

And I regarded the courses in physics and some others, but very much the courses in physics, as, of course, a master-pupil relation, but as a very vivid relation. I enjoyed them; I probably participated, since they were small classes. They were not the

comradeship of people the same age with the same interests, but there was a lot of comradeship involved. My friends were the same age and they were people who were mostly going into biology, but they were just a few. I lived with a biologist—a biochemist—and we had a very close friend who was also a biochemist; we tended to eat dinner together and so on.

Kuhn:

There was really no one you remember particularly who was in physics in this group?

Oppenheimer:

I'm sure there were students whom I knew, but in some funny way, by this crazily abnormal curriculum, I was doing gas analysis in the chemistry department all this time. By never having the same underpinning in physics or in collateral preparatory courses I had no "co-moving coordinates," there wasn't anyone else who was having the same lack of preoccupations and the same preoccupations, and I couldn't name with certainly anyone who was in Bridgman's class, for instance. I think Crawford was, but—There must have been fifteen people or ten people—I don't remember.

[Break]

Oppenheimer:

I said this in order to explain the quality of what I could say rather than because I want to withhold or elaborate.

As far as I remember I was introduced to Thomson, and he and Everett [J. J. Thomson's mechanic and lab assistant], worked in one corner of this large basement room and I worked in the diagonally opposite corner. I think my intention was to get the art of making thin films, perhaps to postpone the more difficult question of what I would do with them when I got them. I did make some beryllium films which, I think, Chadwick used for other purposes but I will not try to tell you the miseries of evaporating beryllium onto collodion and then getting rid of the collodion, and so on. I did some studying; I remember the course with Littlewood. I don't remember any formal courses in physics. I went to the seminars and then I really began to read the contemporary journals.

Kuhn:

When you say "seminars," would this be the Cavendish colloquium?

Oppenheimer:

Right, and before long the Kapitza Club which was a wonderful thing. That may not have happened until spring. I don't know when it was, not the first month or two but perhaps just before or just after Christmas, I found out about the quantum mechanics and shortly thereafter, or almost simultaneously, about Schrodinger's first paper.

Kuhn:

Do you remember when you first found out about the quantum mechanics? Did you find out about it in Dirac's form, or did you find—

Oppenheimer:

No, from the Heisenberg paper. I found out about it probably by hearing it talked about and then by reading it, because I did then turn to the contemporary journals and read them, probably selectively, very hard.

Kuhn:

You had not really been reading periodical literature before this—it had been books?

Oppenheimer:

That's right. The business in the laboratory was really quite a sham but it got me into the laboratory where I heard talk and found out a good deal of what people were interested in. I still had the feeling that I should be able to understand what was going on in metals, but of course, I didn't, and even Schrodinger's paper, which might have given one ideas, didn't connect with this problem for me. I met Ellis; I met Rutherford, of course; I met Chadwick. Chadwick used to come around: I think he had a sense of duty. I remember his opening a *Physical Review* to a paper of Millikan's on cosmic rays with the remark, "Another cackle. Will there ever be an egg?" I met Blackett whom I liked very much and I met [C. F.] Powell. We used to lunch together at each other's diggings.

Kuhn:

You were living in college?

Oppenheimer:

No, I was living in a miserable hole. [Salomon] Bochner, who is a mathematician around the corner, moved in there at one point and then in the spring I moved out to a place along the river halfway to (Grandchester) which was less miserable. I dined in college. I think we were supposed to; I think there may have been a rule you were supposed to dine in college. I forget when I met Dirac, but I think it was when he gave a paper on the elimination of the nodes, which would date it rather. I may have met him before that.

Kuhn:

Where do you think he presented this?

Oppenheimer:

That would have been the Cavendish colloquium. Well, in a rudimentary way, I began to

get pretty interested. Someone who was extraordinarily helpful, kind and helpful, was Ray Fowler, who had that gift of helping young people and who was, I think, also Dirac's patron in a very different way. It was he who reminded me of the trouble with band spectra which wouldn't come out on the old quantum theory and said why didn't I learn Dirac's methods and why didn't I have a look at it. I guess I naturally had Dirac's paper at hand and it wasn't a very hard problem. I think it was right, even, which is indeed rare in my papers. [Paper no. 1]

Kuhn:

You'd had no awareness of this problem at Harvard? I mean, Kemble had been working on it, of course, in the old quantum theory.

Oppenheimer:

I could very well have had. In fact, it could have been that I brought the problem, but I think the notion of bringing it together with quantum mechanics was something that Fowler would have encouraged. I believe that I didn't discuss the work with him until it was done but that perhaps christianized the paper. I don't remember that, but it's perfectly possible. The other problem, the collision problem, is something that I thought about on my own, no doubt. It was a natural question to think of in Rutherford's laboratory which was the great collision factory of all time. That was a mess, that first paper. It was through my interest in it though—

Kuhn:

Did you do the collision problem paper at Cambridge? It's again missing from our records?

Oppenheimer:

It's the continuous spectrum problem, which I understood to be a collision problem.

Kuhn:

O.K. That makes better sense of it now.

Oppenheimer:

It was my interest in that that made Born ask me to come to Göttingen. That would have been in the spring, but I don't know just when. Through Blackett I met Ehrenfest; through Rutherford I met Bohr; and at that point I forgot about beryllium and films and decided to try to learn the trade of being a theoretical physicist. By that time I was fully aware that it was an unusual time, that great things were afoot. It was in the spring of that year, I would guess early spring, that Dirac said he didn't see the connection between Schrodinger's theory and Heisenberg's but he was sure it existed, although he hadn't been able to work it out. And I think I saw Eckart's paper on it because I saw Schrodinger's, but I'm not sure of that. I think, in any case, it is something Eckart can be very proud of. I was away fairly long over Christmas and in Corsica in the spring; but

after my return from spring vacation, my health was better and I got to work on these two things. I don't fully know what I was doing that kept me in Cambridge into June and July, perhaps working on these things, perhaps studying, learning.

Kuhn:

The second of them was read in July '26.

Oppenheimer:

It must have been in before then.

Kuhn:

No, I don't think it was published then. It was read in July at the Cambridge Philosophical Society. You would likely have stayed for the reading.

Oppenheimer:

It's conceivable. In any case I must have left about this time.

Kuhn:

When did you meet Born?

Oppenheimer:

Born came to Cambridge while I was thinking about this; perhaps I talked to him about it. In any case, he expressed a desire to have me come to Göttingen. Ehrenfest had been extraordinarily warn and friendly. At some point in the spring Leiden gave a week for the American students of physics in Europe—maybe it was broader than physics. Carl Anderson was there, [R. J.] Van de Graaff was there, and I met Uhlenbeck, again Ehrenfest, and Lorentz, who was the grand old man of it. It was wonderful and I realized then that some of the troubles of the winter had been exacerbated by the English customs.

But Ehrenfest came to Cambridge and we went out on the river and talked about collision problems, Coulomb's law and how that could be, and so on. I know that, when I was going to go back to Göttingen, I wanted to see if I couldn't do better with continuous spectra and related collision questions. I did come back to this country briefly.

That is very thin on Cambridge, but it was my coming into physics: And when I got to Cambridge, I was faced with the problem of looking at a question to which no one knew the answer but I wasn't willing to face it. When I left Cambridge I didn't know how to face it very well but I understood that this was my job; this was the change that occurred that year. I owe a great deal just to the existence of the place and the people who were there; specifically I owe a great deal to Fowler's sense and kindness.

Kuhn:

Do you remember conversations there about the things that were going on in quantum mechanics in that year? Were people much aware of taking Heisenberg at all seriously before Dirac? I mean, he had been there that summer; he is always supposed to have delivered a lecture on the new theory before it was published.

Oppenheimer:

Fowler knew it was serious. Dirac knew it was serious. I don't believe that in the laboratory itself it was too serious. Blackett was interested in de Broglie and Elsasser; he was interested in waves. He published something about it; he showed that the classical limit for coulomb collisions was the low energy limit, just on dimensional grounds, on primitive model grounds which could have been dimensional. So he was interested and I think that went on about that time. [P. M. S. Blackett, *Proc. Camb. Phil. Soc. 23* (Apr. 1927) pp. 698-702].

Kuhn:

When you say 'de Broglie and Elsasser', were people actually aware of Elsasser's paper at that point? Or was it just de Broglie?

Oppenheimer:

I think Blackett was aware of it; yes, I think definitely. Maybe that's something Born talked about; maybe it's something Ehrenfest was aware of; but it's certainly pre-Göttingen for me. No, I don't believe that the laboratory—and I think of Ellis, Chadwick, (???)—that they realized that something very big had happened. Dirac was not easily understood, not concerned with being understood. I thought he was absolutely grand.

Kuhn:

Did you (???)

Oppenheimer:

Not as much as in Göttingen, but enough for him to regard me as a friend and for me to regard him in a complementary way. That probably didn't start until April sometime.

Kuhn:

What about spin in this period?

Oppenheimer:

That everybody did know about and, because it was pictorial and nonformal, it got digested. I think the question of going <u>behind it</u> must have been early in other people's minds but it was not in anybody's talk.

Kuhn:
They not only knew about it but took it quite seriously from the start?

Oppenheimer:
I would say that once the factor two got straightened out, it was something that was regarded as true. I don't know what you mean by 'seriously'; seriously as a problem not yet, seriously as a description, yes.

Kuhn:
Because on the whole the sort of spectroscopic problems on which it hinged qualitatively had not been Cambridge problems to any great extent. Dirac surely did know how to handle them and was handling them with q numbers, but he was perhaps the only one.

Oppenheimer:
Well, the spectroscopic problems were known rather well to Fowler anyway, and that was after the Bowen-Millikan things. This had stuck very much because it had to do with x-rays; it did seem part of Cambridge.

Kuhn:
That's quite interesting because the reasons that the people who did take it seriously took it seriously vary a good deal from one place to another. I hadn't really had anybody point to that particular reason for doing it before.

Oppenheimer:
Well, this may be subjective. It was very important to me—this redundant explanation of the doublets—that had to get straightened.

Kuhn:
You had been pretty thoroughly aware of that problem when? When you arrived there—?

Oppenheimer:
No, I learned about it reading that winter. Perhaps somebody said something, but more probably, I read it, because I think it true at Harvard, that if I read a current periodical, it would have been waiting for something, but in Cambridge I formed the avid habit of reading everything as fast as I could get my hands on it.

Kuhn:
Did you read back in the literature at that point also?

Oppenheimer:

When I needed to, but never systematically; that is, if there would be a reference or an argument that was supposed to be clear in another paper I would try to find the paper. I was vaguely aware that the Schrodinger equation and the Sturm-Liouville equations had something to do with each other but I guess, preoccupied with Dirac's algebraic approach… I knew I couldn't handle the things I was most interested in that way, so this underlay the changed approach of the little works I did in Göttingen.

Kuhn:

How did you feel about the contrast, conflict, comparison between the two or, if you like, three approaches? Q-numbers look closer and are closer, I suppose, in some ways to matrices, but the whole spirit of all three of these as to the way you handle them is quite different.

Oppenheimer:

Well, I would say that because of its proximity and its beauty I liked what Dirac did, but I thought it was hopeless—anyway for me—to do very much by these methods on collision problems and changes with time and so on.

Kuhn:

You pulled some remarkable canonical transformations out of the hat in that first paper.

Oppenheimer:

I don't remember. And I think that I thought that the wave methods would be more helpful and more natural; but I felt fine about it after the publication of the transformation theory. Both Jordan and Dirac—I think Jordan's paper is also very good, because then it became clear that one could and should adapt the language to the problems and it was stupid to argue about which was finer, but it happened that I started with the algebraic approach.

Kuhn:

Were there quarrels on this subject in Cambridge as there were in Göttingen?

Oppenheimer:

There were in Göttingen; there were very bad quarrels in Göttingen. I don't think there was anybody to quarrel in Cambridge. Well, Hartree was there and probably played more part and probably said more to me than I am recollecting. I can't remember.

Kuhn:

Did you know Thomas there?

Oppenheimer:

I didn't know him. I don't know whether his visit to Copenhagen was just for a day or

whether he was just there that spring.

Kuhn:

I think he was there for a whole year.

Oppenheimer:

Well, that was the year. Anyway, I didn't know him. I shouldn't say I didn't know him since even that is not sure, but I don't believe so. But I did know Hartree; I liked him very much. It's even possible that he had some kind of responsibility for me as an advisor or something like that, I'm not sure.

Kuhn:

You, I think, translated that year a version of Born's collision paper to the British Association or something of the sort. In any case, you must have been involved there with this first statistical interpretation. I wondered how you had felt, how you thought others had felt, about that at the beginning?

Oppenheimer:

I think this may be what Born came to England for and I find it hard to believe that even then he would have needed a translator, but it may be that I had some part. It took me years, in fact, even now I have to pinch myself, to realize that the statistical interpretation wasn't completely obvious. So that I was, wrongly, more impressed with this mathematical scheme, the Born approximation, as a way of really getting insight into problems; the other part I thought was not arguable. Born has had to tell me, "You don't realize what a great thing I did for physics," very often simply because I happened to be at a time with such short roots that I didn't see what else you could do. Perhaps prepared by the Slater business of the year before. I would like to quote one brief episode: when Rutherford introduced me to Bohr he asked me what I was working on. I told him and he said, "How is it going?" I said, "I'm in difficulties." He said, "Are the difficulties mathematical or physical?" I said, "I don't know." He said, "That's bad."

Kuhn:

This was at what point?

Oppenheimer:

This was while I was working on the two-body problem. The other—he couldn't have said that.

Kuhn:

By the time you had already decided to go to Göttingen, did you then pretty fully see theoretical physics as a career and see yourself developing in the directions in which you very rapidly did develop?

Oppenheimer:

I had very great misgivings about myself on all fronts, but I clearly was going to do theoretical physics if I could. I did. It didn't seem to me like foreclosing anything; it just seemed to me like the next order of business. I felt completely relieved of the responsibility to go back into a laboratory. I hadn't been good, I hadn't done anybody any good, and I hadn't had any fun whatever; and here was something I felt just driven to try. In that sense, right, but in the sense of thinking about the future, no.

Kuhn:

Somehow or other it's not an altogether natural thing to have happen to you at Cambridge in the sense that you were not an awful lot more likely there than at Harvard, I should think, to have discovered that this existed, except to the extent that you discovered it by reading.

Oppenheimer:

Well, there were many more visitors. The fact, I would say, that Thomas was in Copenhagen, and Hartree, Dirac, and Fowler were more interested and it was closer, and also the time. I don't know what would have happened that year at Harvard had I stayed at Harvard. It might have gone better, but 1924-25 at Harvard was very different from 1925-26 in Cambridge. And Rutherford, you know, for all his simplicity, was very much interested in these new things; he didn't want to be bothered with them but he understood that it was important. He talked about it.

Kuhn:

Why don't we hold it here and then simply plan to pick up with the transition to Göttingen on Wednesday.

[End of Session 1]
[Begin Session 2, November 20, 1963]

Kuhn:

I think we have agreed to go more or less straight to Göttingen today. I would like to get you to expand on one thing which you said last time if you will.

Oppenheimer:

If I can.

Kuhn:

Well, I'm quite certain this is not so much a matter of detail of memory but rather of sense of the situation. Remember you told me the story of Bohr's asking you "are the problems mathematical or physical?" I wondered how this as a question had struck you.

You said something about your own training in mathematics. I wondered whether this was the sort of question which at that stage of the game was a new idea as to how one might divide up the sorts of problems one ran into in research.

Oppenheimer:

I thought it was a very good question and I thought it put a rather useful glare on the extent to which I became embroiled in formal questions without stepping back to see what they really had to do with the physics of the problem. I think that it's a reproach which would not have characterized my work at other times but at that time it was a very useful question to me. It's of course very different with different physicists. Some use the mathematical language as the direct immediate description of nature and any thought of stepping outside that is only a concession to intelligibility; it's only pedagogical. I think this is largely true of Dirac; I think that his invention is never initially verbal but initially algebraic. Others, and I would say Heisenberg is a very good example, walk on two feet, and Heisenberg is capable of getting terribly impatient. The whole Heisenberg-Pauli approach to electrodynamics is a monstrous 'boo-boo', but that's because they were interested in the physics and this 'gauge-invariance' and 'auxiliary conditions' was just something that was in the way; but still, the price they paid for it is unbelievable. Bohr, I would say, regarded mathematics as Dirac regards words; namely, as a way to make himself intelligible to other people, which he hardly needs. So there's a very wide spectrum. I was simply learning and hadn't learned very much.

Kuhn:

I'm interested that you describe Dirac's approach as being principally algebraic. He insisted to me—and it surprised me and I raised the question—that it was principally geometric. I said I'd always thought of it as much more algebraic than geometric. Do you react to this?

Oppenheimer:

Well, yes. I think that this is a long dispute between mathematicians as to how the main, pat traditions are to be characterized. Certainly compared to [C. N.] Yang, who is also highly algebraic, Dirac is very little geometric because Yang is always having to draw pictures. It seems never to have bothered Dirac and I do not believe there is a drawing in his entire published literature.

Kuhn:

Except for the article in the *Scientific American*.

Oppenheimer:

Right, and I'm sure that he's not responsible for that. Of course to do the algebra of groups, to do any abstract algebra, is to deal with concepts which have a very rich geometric content and in that sense he's right; on the other hand, his methods are algebraic.

Kuhn:

That was certainly my own impression. I gathered from him that part of what he had in mind was that he had gotten deeply interested and had found considerable revelation mathematically in projective geometry, a subject I know almost nothing about. But a lot of the things that looked algebraic to me had for him appreciable projective geometry background.

Oppenheimer:

I see. Of course a projective geometry is also a lovely analytic structure; it isn't any longer only algebraic. He did publish a paper which I think was wrong on the Dirac electron in projective space, and that was set straight by (Gorsai) and (Lee) very recently.

Kuhn:

That's part of the story I didn't know.

Oppenheimer:

Well, it's last year and nobody can think what it could possibly be about.

Kuhn:

Coming at this, clearly you need a lot of mathematics from the time you're beginning at Cambridge, and from then on, that you haven't really had before. How did you go about getting it? Did you go out and get books to learn a new branch of mathematics? Did you work it for the problems?

Oppenheimer:

In the first place, I never did learn very much. I probably learned a good deal by a method that is never given enough credit, that is, by being with people. I learned some by having to do things. You could say that the paper on Richtungsentartung was my own painful analytic discovery of the rotation group and it is a ridiculous way to do it, especially after dealing wit the molecules by much more powerful methods; but when I was through with it I knew something about the rotation group. I learned some by lectures, more I would say by reading, but I doubt whether I made a systematic attempt. I knew Whittaker and Watson from cover to cover and I read Riemann's papers, but you could almost say that these were undertaken in the same spirit as Flaubert—something that was nice to know rather than for utilitarian purposes. I had to learn a little more about Laplace transforms and so on, for instance, when we came to the cascade showers; but that was pretty much in me and it was just some slight (wrinkles) that we had to invent. I still don't know much. I think if you asked me not 'what did I do' but 'what, in view of the times I was going to live through, I should have done' I should have learned more mathematics. I think I would have enjoyed it, but it was a part of my impatience that I was coreless about it.

Kuhn:

Was it always a tool for you; I mean, was abstract structure of mathematics as such something you didn't emphasize much? I ask this against the background of remembering that you told me that you'd read through the *Principia* with Whitehead.

Oppenheimer:

Well, I think that I have always had very great interest in questions of principle and most of the *Principia* is not about principle but as a whole it is. I think that indeed it was a very exciting experience to read Riemann's *Gesammelte Werke*; it's just a little book, you know, but what a book! And I think that mathematics in and for itself has always appealed to me, but it has never been my vocation. I would never be somebody who would come upon something in the course of a physical problem and then say, "But there is a mathematical structure here which I would like to describe in the abstract." Many people are so; Wigner, for instance, very strongly. But he was, whether self-taught or not, mathematically literate in a very different sense than I.

Kuhn:

I'm terribly curious about the answer to that question myself and I hoped by now to know more about it. I trust we will begin on it—on where his mathematical education for a chemical engineer came from—tomorrow.

Oppenheimer:

I think von Neumann also started as a chemist.

Kuhn:

He started as a chemist, I think, not as an engineer; in fact it seems to me he did mathematics in Budapest and chemistry in Zürich simultaneously, taking two degrees almost at the same time, commuting. There is a remarkable story whose detail I've lost at the moment.

If you were answering Bohr's question now about the difficulties you had been having at that point—

Oppenheimer:

I would now say that there were, in dealing with continuous spectra, questions of principle which had not been articulated, which gradually got straight, some of which Born cleared up, although not all, and some of which are very pestilential today, because long range forces do not fit in any of the existence proofs or any of the axiomatic dealings with field theory and that is after all the main curse with which I was bothered. But I would say that, for instance, the question of normalization should be got out of the physical stage into a formal one very much more cleanly and clearly than I did until later, and then the formal side is not at all unmanageable. After all, one of my troubles is

that there never isa free particle in a Coulomb field, and the attempt to describe this, which is just what people have trouble with today in field theory, was bound to get into trouble. Well, I should have given that up.

Kuhn:

I think probably the thing to do is to start on Göttingen. You told me that Born invited you there after discovering your interest in the continuous spectra—this, I take it, and the collision problems.

Oppenheimer:

Yes, they're very closely related.

Kuhn:

Did you go there as an assistant?

Oppenheimer:

No, I went there as a graduate student and he asked me whether I wouldn't like to take my doctorate there. Heisenberg was not there, but it was a very lively center. I had a great deal of contact with Jordan, from whom I learned a lot. He was a very odd man even then, perhaps more so then, but very clear; he was very abstract, very impatient and very clear. I got to be quite a friend of von Neumann's.

I lived in the Cario house—have you heard tell of this? [Gunther] Cario, C-a-r-i-o, was an experimental physicist who worked I guess in Franck's institute, but maybe in Pohl's; his father was a doctor who had been disqualified for malpractice. They had been very well off and the inflation had ruined them and they had the typical bitterness on which the Nazi movement rested. This is not for a moment to say that they were Nazi; I have no knowledge of that. But they had a very large house not far from the center of Göttingen, an enormous house and a garden of several acres surrounded by a wall, and no money; so they took in typically American visitors. The Hognesses were there, the Condons were there until he got sick of Göttingen and went to Munchen; Dirac stayed there, the Carl Comptons stayed there, and many many others.

This was only a few minutes' walk from the institute and it was very near the wall around which Dirac and I used to walk.

I got to know Houtermans quite well although we had not so close professional interests then; Elsasser was there, and Houterman's wife was there—the girl that was to become his wife. I knew certainly all the Americans. I didn't get a chance to be at all close to Hilbert but I at least had the chance to see what a man he was; he was at that time under treatment with an American liver extract for pernicious anemia. I got to know Franck as one gets to know a very dear man and a very great professor. I was not infrequently a guest at the Born's and we used very often to make these famous walks to

(Nickelausbad). There were a lot of other Americans I can no longer identify.

In the sense that had not been true in Cambridge and certainly not at Harvard, I was part of a little community of people who had some common interests and tastes and many common interests in physics. I remember this more than I do lectures or seminars. I think it quite probable that I attended some of Born's lectures, but I don't remember. I'm sure I gave a seminar or two, but I don't remember. I met Courant, which is obvious. I met Heisenberg who came there and I had not met him before; Wentzel, and Pauli in Hamburg or in Göttingen so that something which for me more than most people is important began to take place; namely, I began to have some conversations. Gradually, I guess, they gave me some sense and, perhaps more gradually, some taste in physics, something that I probably would not have ever gotten to if I'd been locked up in a room.

Kuhn:

Wigner was there the same year. Did you get to know him at that point? I ask this to some extent particularly because of your reference to group theory and the Richtungsentartung paper.

Oppenheimer:

Was Wigner a student of Born's?

Kuhn:

No. He was a student of Polanyi's [in Berlin], but he'd taken his degree, he'd been an assistant in Berlin for one year, and then he came to Göttingen for one year. In fact, I think he was Hilbert's assistant, and I am curious because Nordheim, who was also an assistant of Hilbert's—

Oppenheimer:

Nordheim I knew.

Kuhn:

Nordheim told us that as an assistant of Hilbert's he was really isolated, rather more than I would have supposed, from the physics group. I'm sure that Wigner was there in '26-'27 also, and it's from there that those group theory papers begin to come out.

Oppenheimer:

Right. Well, I would say that I was unaware of all this. It's almost certain that we would have had occasion to meet, but it left no mark compared with what became a rather warm friendship with von Neumann. There's no recollection of Wigner and that may have been because he was then genuinely shy as compared to his now famous mock-shyness. I did know Maria Goeppert then—well, this is enough of society then.

I should add that, although this society was extremely rich and warm and helpful to me, it was parked there in a very, very miserable German mood which probably in Thuringia was not as horrible as a little bit further east; Göttingen is not in Thuringia—not as bad as in Thuringia—but it was close enough to Thuringia to be bitter, sullen, and, I would say, discontent and angry and loaded with all those ingredients which were later to produce a major disaster. And this I felt very much. I had one friend who is not a physicist. He was the son of the publishing house (Ulstein); they had money and he had a car, which was rare. We used to drive over to Kassel sometimes and think that the French had been there. They never quite made it to Göttingen. He parked this car in a barn outside of Göttingen because he thought it was dangerous to be seen driving it. This is a small example.

Kuhn:

Among the physicists this resentment must have been a good deal more marked with some than with others.

Oppenheimer:

I would say that I didn't think the physicists showed this; I thought that the older Mrs. Cario showed it very strongly. I think possibly Pohl, who was in charge of the third institute, resented Franck and Born, both of whom were so much brighter. This was the year Franck got his Nobel Prize and there was a great torchlight parade, and there was no doubt some jealousy of that. But Franck was not a man to have any resentment and at that time I think he had no foreknowledge of what was to come. I think he took a very sunny view. I could be wrong because I was a youngster and I could have missed the whole point. Born of course was never a sunny man, but I think he was driven by an ambition to do good physics and was very self-confident in this year when I was there; I think he put off some of the Americans by how sure he was that his way was the right way. I have a little story about that which didn't put me off but which produced a very strange result. Well, so much for the general background.

Kuhn:

You don't want to tell us that story?

Oppenheimer:

I'm coming to it. This has to do with one of the papers. I still was not entirely well and I had several attacks during the year but they became much more isolated and interfered less and less with work. I did work at night most of the time and I think probably rather hard.

Kuhn:

Just the papers themselves?

Oppenheimer:

Well, the learning I had to do. I'll say straight out that the most exciting time I had in Göttingen and perhaps the most exciting time in my life was when Dirac arrived and gave me the proofs of his paper on the quantum theory of radiation, but I had some exciting times on more modest things. I really don't know what's in the paper on continuous spectra; I don't want to know what's in it. It's a subject to which I returned and never really got it completely straight; I guess I must have hated it.

Kuhn:

Well, it runs through recurrently in one way or another all of the years almost up to the point that you get interested in relativity, relativity of electrons. I hadn't really realized it until you pointed out to me the relation of this whole thing to the collision problem and to your thin film work—how much that was a consistent theme throughout.

Oppenheimer:

The methods of course were right, but the analytical machinery was criminally cumbersome and I don't know on what ground it was accepted as a doctoral thesis except that anyone who was willing to look at that paper could then have done it better.

Kuhn:

Was it Born who put you on to the Weyl-Hellinger normalization approach?

Oppenheimer:

No, I think not. It may have been in a published paper by Fues; it may have been through Jordan, but I doubt it. I don't think that Born participated much in this Arbeit. I think if he had, It couldn't have looked the way it did. The little paper on screened Rutherford scattering was almost to make up for the fact that I hadn't done the Rutherford scattering right myself, but it also did have a point which was to show in what measure—and this became important in the theory of stopping—the high energy validity of Rutherford's formula was an accident; because although the angular distribution of the scattered particles is given right, the connection between angle of deflection and impact parameter is entirely changed by the quantum theory. This is not at low energies but at high.

I mentioned the paper on degeneracy [i.e., the one on Richtungsentartung]; this had always puzzled me a great deal and had I gone about it using compact algebraic methods it would have been all right. I just strung it out with spherical harmonics and about one of the results I remember Pauli saying, "If that weren't true, nothing would be." But there were others of which this was non-trivial, which really did express the group properties of the representations of the rotation group. The application of this to impacts was, I think, correct but not very inspiring.

Kuhn:

Did the whole concern with space quantization grow out of the earlier attempts at

collisions and the polarization effects in collisions and that sort of thing or was that a separate puzzle?

Oppenheimer:

No, I think that it was because the Stern-Gerlach experiment is something that remained for me a miracle and I wanted to see how it worked out.

Kuhn:

When did it become a miracle? This was a bad puzzle for a lot of people; it wasn't for you at Harvard in any case, I think.

Oppenheimer:

I think it was one of the things that I would have learned from Kemble and which would have made a very great impression anyway.

Kuhn:

Well, I don't think it was a bad puzzle for him. There were plenty of people—in fact, there were two sorts of reactions to that as far as I can make out: the reaction that said, "Things are so strange in quantum mechanics anyway, what difference does it make"; and the other one, which was particularly exemplified by Einstein and Ehrenfest, that the absence of any correspondence principle way of handling this made it deeply disturbing.

Oppenheimer:

Well, I think that I would have been more inclined to think of it as Ehrenfest did than Bohr, because for Bohr it was simply a vivid example of something he knew ahead of time. I think anyway that it was this preoccupation to show that the new theory dealt with all this in a harmonious, consistent and intelligible way that got me into that.

Kuhn:

Do you have any notion of structure for the development of that problem in your own mind? I'm particularly curious about this one because this puzzle and the way it dawned on people, the way it affected different people, I think is a very interesting question. Partly it is because, although I'd be glad if you had counter evidence, I'm not at all sure that this is something of which it is right to say that Bohr knew it all along. There were important debates between Bohr and Sommerfeld over this.

Oppenheimer:

Well, I had met Bohr but I had never talked with him about it. I thought of it as one of the great pedagogical paradoxes of quantum phenomena and I guess my feeling then was that he had had put into our hands a wonderful instrument which ought to be tried out. It would on the one hand, clear up the paradoxes of quantum phenomena and, on

the other hand, illuminate the richness of the theory we had, but this is to put a very rosy glow on what it was all about.

Kuhn:

Let me ask you just one other question about this space quantization paper. I haven't studied the thing with case and in fact I know it largely through somebody else's notes on it, so I may very well be missing the point. You do one thing in here which surprises me a great deal; when in connection with the question of phase relations you take a whole bunch of wave functions for the atoms and put a gamma phase function, a different one, and then add them all together, do they add rather than multiply?

Oppenheimer:

I think this must be a primitive density matrix; that is I tried to describe, for instance, an unpolarized atom. What you would normally do is take the wave functions for the different stationary states, calculate expectation values for them, and then add with appropriate weights. But you get of course the same result if you add the wave functions but with random phase, and I think that this was what that was. I would have to look but this I think would today be done in terms of a density matrix.

Kuhn:

Tell me how the molecule paper came about, because that's suddenly quite off the track of everything else.

Oppenheimer:

I think that this may very well have been something that I picked up; it may have been from Born, but it could have been really from any one of a number of people. Born and Heisenberg I think had written about it. Maybe Heisenberg said—I don't know, but somebody said—that one ought to look at that and see how it is. I did and it seemed to me that there was only one important point and that was that something new came in. The expansion parameter was the fourth root of the mass ratio and with that then everything tucked into place. The question of corrections of accuracy of other level splittings of the order of the vibrational or rotational ones were complications, but in principle why molecules were molecules was clear. I told Born about it.

Kuhn:

Up to that point he had not participated?

Oppenheimer:

No, no. This was my doing. I think I probably showed him how it went. He was very pleased and I said I was going away to Paris for the Easter holiday or someplace and I would send him the paper from there. I did; it was perhaps four or five pages and it made this one point, saying that in principle this was the point.

Kuhn:

This would have gotten to the point of showing that there were no first and third order terms in the expansion.

Oppenheimer:

Right, and showing the order of magnitude of the vibrational energies, of rotational energies, and indicating what complications might arise if there were electronic fine structure. And that's probably about all.

Kuhn:

Do you suppose you still have that draft?

Oppenheimer:

I'm certain I don't. I kept no paper of any kind until I had a secretary in '42 or '43. I thought that this was about right; it was very light of touch and it seemed to me all that was necessary. Born wrote back completely horrified and said that he would write it up properly. He produced this thing which must be thirty pages long but which has nothing else in it except a few theorems which are fairly obvious, the mean of the average or the average of the mean are sometimes equal, and so on. I didn't like it, but it was obviously not possible for me to protest to a senior author. He, I think, was very disturbed that I had dealt with it all so casually, but that's the origin of the dull opus. I think I probably had nothing to do with the writing of the long paper; I may have changed a few things, put in a sentence or taken out a sentence, but certainly very minor, and it was certainly not something that was comfortable between us. It was obviously a rather deep difference of taste which was not something you could argue about.

What happened in Göttingen which excited me and which I didn't publish until later but did talk about was this Stark effect thing. I was by then quite a bird on worrying about continuous spectra and I saw the asymptotic form of the wave functions. I think I used parabolic coordinates, which work; they may have been used in calculating intensities in the Stark effect. Who would have done that? Pauli didn't do that, did he?

Kuhn:

I don't know who did it with the new quantum theory.

Oppenheimer:

Well, I think parabolic coordinates, but anyway then it was very clear and I was quite excited by this because it seemed to me to open up an answer to the question of how electrons got through metals so fine.

Kuhn:

You saw that—I mean, those two things went together for you quickly?

Oppenheimer:

Not in detail because I knew there were insulators and this was too general. Everything would have conducted. And I knew the difference between insulators and conductors was not that much a matter of degree.

Kuhn:

You speak of it as the Stark effect and almost immediately it becomes the problem of conductivity, and I'm curious as to which of these you were looking at when you started.

Oppenheimer:

When I started I wanted to deal with the hydrogen atom in an electric field more or less the way one would deal with it in a magnetic field.

Kuhn:

With an eye to that problem rather than with an eye to—

Oppenheimer:

Then I made, as you may say, just from the formalism the discovery that the wave functions were not normalizable. I tried to think what that could mean. Then I developed this method of the two Hilbert spaces, the almost orthogonal wave functions, which never in my hands or in Kramers' was made into a systematic approach but which could with contemporary methods be made into a systematic approach. Then I had not known—I don't know why, although I kept reading the *Physical Review*—this cold emission work that Millikan started. This is to go rather a year later, six months later. When I got to Pasadena I had the general dependence of the current on field strength long before that.

Kuhn:

That also originates then in Göttingen, this application of it?

Oppenheimer:

Yes. It is the hyper-Stark effect problem, you see. And I found that [C. C.] Lauritsen had fitted his empirical curves with he same function. But this was more like physics because although anyone could have picked it up and although it couldn't be more obvious, it came rather as an enlightenment and it did have a useful effect on the nuclear things because it was after having that reported that Gamow, and Condon and Gurney, both saw that there was something useful to do with it.

Kuhn:

Am I right in thinking that your first actual publication of this is simply as the third of the three things in that note on aperiodic processes? [No. 8]

Oppenheimer:

I think that's right, yes. This is deplorable, but it was like me not to make any distinction between some mud in which I'd gotten stuck and something which I was really very pleased with.

Kuhn:

It's a striking curiosity to me as I hit that to find that very neat and novel method and way of conceptualizing that problem simply as the third of three notes.

Oppenheimer:

Of which the first is formal and the second rather empty.

Kuhn:

Well they help, but they help with minor perplexities.

Oppenheimer:

I think I did that paper probably during the summer. I think I probably started on it after leaving Göttingen and submitted it for publication which I was back in this country, I don't know just when it was.

Kuhn:

You submitted it in August [1927] and over the signature, Jefferson Physical Labs, so that it must have been just after you got back from passing through Cambridge [Mass.].

Oppenheimer:

No, I went to Cambridge [Harvard].

Kuhn:

I'm forgetting then. About how long were you there when you came back?

Oppenheimer:

I was there till Christmas, using the first part of my National Research Fellowship. (That anyway) was fun and much greater fun was Dirac's radiation theory which I got very well into me and is the reason that I could invent Fock space for the Heisenberg-Pauli monster and could take some interest in quantum electrodynamics and relativistic theory. I thought it was extraordinarily beautiful—the connection of the Einstein coefficients with the 'one' in the transition amplitudes; it couldn't have been nicer. I also remember that we did talk about the fact that there was sure to be trouble with this business once you put a charge in there. Dirac hoped it wouldn't happen and, because

the Coulomb field was not formally in the picture, thought there might be some miracle.

Kuhn:

When you say "we"—?

Oppenheimer:

This is talk with Dirac. You see, he then went on to very good, but to me very dull, dispersion papers whereas I would have thought that he would go on to the question, "Well, how about the charges?" I was terribly impressed by the Jordan-Wigner quantized fields, but I am not sure whether that happened while I was in Göttingen. I think perhaps Jordan told me about it.

Kuhn:

I think it must have happened, but in any case you know Jordan had done that piece of work with just a sign wrong earlier and brought Wigner into it.

Oppenheimer:

Oh, yes. He forgot the minus ones. No, that's what I knew; I knew the wrong one and then later it got straightened out.

Kuhn:

I'd like you to tell me anything you can remember about Jordan's interest and involvement with all this. He says, and I suspect looking at the publications, that there's probably a good deal in what he says, that this whole question of field quantization had been on his mind really before it was on anybody else's. When Born looked at the Dirac paper and was perplexed by it and said, "Gee, it's neat, but can it be about anything?" or something of this sort, Jordan said, "I reminded him that after all this was what I had been talking to him about and nobody had been paying attention to."

Oppenheimer:

I know that his feeling that continuous waves and fields—their quantization was the most unitary description of nature and one should look at it. This is something that I heard from him; I tended to agree with him.

Kuhn:

Do you think you may have heard it from him before the Dirac paper actually came out? I gather you saw a good deal of him in the fall.

Oppenheimer:

Yes, although I got to know him better as the year went on. My recollection on that wouldn't be well resolved. I remember Ehrenfest's fury at Jordan's triumphant, "*Wir haben die Wellen gequantelt*," and this is from that period. I think I must have been to

Leiden in the spring and so it is clear that Jordan thought this was not so much the way, but the only way. It really wasn't at that time more than an act of faith that this had anything in it that configuration space didn't. When Pauli wrote his review article he refers to this in a footnote and says it's all very nice but there isn't anything there that you can't do just as well in configuration space. Well, there is something there, but that of course doesn't come until you get to the place where the particle localization is no longer meaningful, no longer a physically meaningful nor a formally helpful idea; then this takes over and it's infinitely better than Fock space.

Kuhn:

I have the feeling myself, and I'm not sure whether it's right, that people looking back can take the Dirac paper to be a second quantization paper in effect, so that the effect of the Jordan-Klein paper was to simply show how to handle certain sorts of interactions on Dirac's theory. I have myself much more the feeling that Dirac was not thinking of this at all as a field theory, that it is only a transformation to new coordinates, that there's nothing in his mind like quantizing waves, and that it's really Jordan who brings in this way of looking at the Dirac paper.

Oppenheimer:

Well, certainly Dirac left space-time as fast as he could. On the other hand, I think that Dirac would say, and he would be right, that this is the only way to get the quantum into radiation theory, into light. This is the only way to get at light quanta. It's not the second quantization, it's the first. And, to use somewhat later terminology, that Maxwell waves are not quantum waves at all and have no more than a very loose formal analogy to Schrodinger waves. I think Dirac was quite clear about that. But it is true that for him any 'p' and 'q' was all right and he immediately went to action and angle variables. I don't believe he wrote down the relativistic 'D' function; that was Jordan and Klein, I think, who did that, and yet it's the kind of function which should have pleased him as an invariant generalization of the delta function. So in this sense he was not very comfortable in space and time. I think Jordan was the one who pushed for this. He is the common element between Jordan and Klein, Jordan, and Jordan and Wigner.

Kuhn:

And Jordan Pauli also.

Oppenheimer:

And Jordan-Pauli. It would seem very artificial to suppose that his presence in all of these was pure geographical accident.

Kuhn:

No, I think he was the leading partner in all of those and he points back to that very last section of the Born-Heisenberg-Jordan paper, which is apparently purely his own and which neither of the others liked very much, as being the one that really starts this

whole thing for him out of a long-standing interest in the Einstein fluctuation problem which very few other people were either very much satisfied with or very much interested in.

Oppenheimer:

I have to remind you that I was quite inexperienced, quite young and quite uneducated, but my impression is that Jordan was a "very lot of a physicist" and that if this didn't keep up, if what he did afterwards was less interesting, if in general he was not as well recognized and regarded as this line of work would seem to suggest, it was in part because he was really an unbelievably queer duck with tics and mannerisms and harshnesses and I won't say brutalities, but apparent brutalities, which put people off very much.

Kuhn:

Well, I was curious because I must say you're the one person who has said to me that he knew Jordan fairly well and got quite a lot out of him; a lot of other people said they knew him but they found the stutter so hard to get through.

Oppenheimer:

Yes, but you see I was just badly enough off myself perhaps to make it easier for him. The stutter was not good.

Kuhn:

Before we get away from Göttingen, which I really don't want to do too quickly, there clearly must have been a lot of things in this year which were subjects for a good deal of discussion. You said for example the other day that the whole question of the matrix approach versus the Schrodinger approach was alive there in this period.

Oppenheimer:

It was of course before the uncertainty principle and complementarity, but Göttingen was a Bohr town. It had been conquered I guess when he went there in 1913 or whatever, the first time he went.

Kuhn:

There were still problems then.

Oppenheimer:

And Born felt very embattled not to let the wave lovers get away with it. I think it was perhaps around that time that Schrodinger did his harmonic oscillator whose packets never come apart, and that caused a good deal of irritation—that he should have known better than to regard this as a typical example. You see after all, Born got into the matrix thing very early and very heavily, liked it, but then he didn't hesitate to use the wave

equations to discuss collisions and I do not think that he wanted to use matrix algebra. Dirac did a little along those lines but really it was not that; he used the Schrodinger equation and different representations.

Kuhn:

There's a rumor around that I have not gotten either Born or Heisenberg to talk about which is that Heisenberg was pretty upset at Born for using the Schrodinger equation, and I notice again that he uses it from the start in the Born-Oppenheimer paper.

Oppenheimer:

Well, you see, that would have been something that could have been done but it would have been very clumsy and you see how clumsy because the fourth root is needed in the wave function and you would never have seen that. The matrix elements have it but the characteristic values don't and I think it would have been very hard to get the hint which is absolutely obvious once you write it out in wave form. Collision problems in matrix form are not mathematically well defined; even when you know the answer it's not nice to put it in that form.

Kuhn:

What sort of issues did this present in Göttingen in this year?

Oppenheimer:

The great issue I think was whether a probabilistic and indeterminate and dualistic view of the wave-particle problem, which you couldn't get away from it you followed the history, was right or whether the waves really were a substratum and particles an illusion. Against this view Born was clearly embattled, and I must say that it never seduced me nor tended to seduce me.

Kuhn:

Were there people at Göttingen who were seduced by it?

Oppenheimer:

This is perhaps taking it a little seriously. I would say that Condon had so marked a preference for waves and analysis and such a distemper over matrices and Göttingen and Born, that he would certainly not have made a philosophical statement about it but he had a very strong taste in favor of wave mechanics. He obviously got over it because the methods of Condon and Shortley are quite catholic, but just at that time he was dyspeptic about it. Whether there were any Göttingen physicists who took this view I really don't know. I have the feeling that there were graduate students or visitors, but I do not think it true of Born, obviously not true of Jordan, not true of von Neumann, not true of Dirac. The devil was foreign.

Kuhn:
It's terribly hard now to find anybody who will admit to or will remember the name of anybody else reputable whom he will associate with having supported or sympathized with Schrodinger's own approach.

Oppenheimer:
I think that that's wrong but I do not think that they congregated in Göttingen.

Kuhn:
That's right. What about spin at this stage of the game?

Oppenheimer:
I don't know when Pauli's paper came out.

Kuhn:
While you were there I think.

Oppenheimer:
I think so. I think I had a proof or something of this paper. I think the honest thing is that I was not engaged in it but I knew what was going on.

Kuhn:
Well, even before the Pauli paper, which fits these things together, what had the feeling in general been about spin?

Oppenheimer:
It was a problem.

Kuhn:
It was a problem to fit it to wave mechanics but it was apparent that something of this sort had to be fitted in.

Oppenheimer:
That's right. At least this was the universal impression, both in Cambridge I the summer of 1926 and—Anyway it was in the air. I don't know whether Pauli talked about it, but anyway I had knowledge of it before it was published and I didn't feel responsible. I thought it was fine I didn't feel responsible.

Kuhn:
Had anybody taken the Darwin approach, which comes out about the same time or a little bit earlier than Pauli's, at all seriously?

Oppenheimer:

I knew of it all right. I think because it wasn't something I was working on or felt responsible for—The conversations must have covered this, we must have had seminars on it, but they left no unequivocal trace. I know I felt the paper on directional degeneracy, which should at least have been able to describe the silver atom, had a hole in it because it didn't even consider this, but it didn't occur to me to try to fill that hole.

Kuhn:

What about this Heisenberg exchange phenomenon, non-interacting terms, which breaks in this same period?

Oppenheimer:

That was very exciting. That there was a lot of talk about and I regarded it as a kind of discovery of the meaning of quantum theory. I think that the radiation theory and this were probably the events in physics which were most interesting during the period I was there.

Kuhn:

Tell me more about what you mean when you say 'the discovery of the meaning of quantum mechanics'?

Oppenheimer:

Well, here first in very primitive systems one had the new formalisms. In the case of matrix mechanics a rather clear idea of the connection between the formalism and the observation, but also a very limited idea because what one was talking about were these dipole amplitudes in optical phenomena. Well, that's a very small part of what you can find out about an atom. Then with the wave mechanics one had a more supple formalism which had this connection to experiment but which was already very thin and, through the probabilistic interpretation and Born himself, some greater physical content. But let us say the two equations, $pq–qp$ and the Hamiltonian equations on the one hand and Schrodinger's equation on the other, had a lot to say about the world of nature and it had to be explored. People who had lived with atomic problems and paradoxes of course had special worries that they tried to clear up and see if it would work; one of them was Heisenberg's, one of them was Dirac's.

But generally I thought that it was a period in which, using the formalism or its more or less natural extensions, a great deal could be discovered about the content of quantum mechanics which couldn't have been known to its inventors. This is a very rare thing; I mean, one is still in that state in general relativity. It's a formalism rather poorly warranted by any experiment or observation whose consequences are far richer than anyone knows. Well, that's for many reasons, but quantum mechanics was also a formalism which had a hell of a lot to say if you knew how to ask it some questions and

I think the two things that happened in the '26-'27 winter—but the two things that revealed most about phenomena which had not been adequately described before were the radiation theory and the exchange terms.

Kuhn:

I now know exactly what you mean, but I'm wondering how typical this attitude was. How many people would have thought of themselves as exploring nature by trying to see what you could do unexpected with the formalism as against applying the formalism to straight problem solving?

Oppenheimer:

I think when Heisenberg looked at the helium atom he was trying to solve a problem, but I think that Jordan was certainly seeing how far he could push the formalism. After all, in a small way the Stark effect was of the former kind, of seeing what was in the theory; and molecules were problem solving because one knew that there was something to get straight. So, the distinction isn't all that sharp. But I think that if Heisenberg had found that there wasn't anything new but just that the integrals of wave functions happened to give the helium spectrum right, it would have been problem solving. It was the fact that there was an element of novelty and something which had never been described before which turned it from solving a problem into exploring the content and meaning.

Kuhn:

What about the transformation theory? To me looking back, that's perhaps the most exciting of all the things in this year.

Oppenheimer:

Yes, that was done by the summer of '26. I knew about it anyway and it was very exciting to me when it happened because it was the logical completion of the proof of the equivalence of Schrodinger and Heisenberg. I would say that I think that I understood that quite thoroughly, thought in those terms, and could use it when necessary.

Kuhn:

Do you have any impression either for yourself or for others to what extent people really leaned on Dirac's as against Jordan's or Jordan's as against Dirac's formulation?

Oppenheimer:

I think that it was Dirac's that made the great impression in Copenhagen and on Bohr. I imagine that in Göttingen Jordan's was more familiar and more accessible. I think Dirac's was prettier, so his was the one I knew. I knew that Jordan had done an equivalent thing.

Kuhn:
I actually have some impression that in Copenhagen, at least in the discussions that lead from transformation theory into the uncertainly principle, there may have been more leaning on Jordan's.

Oppenheimer:
That's possible. Bohr's later references are to Dirac.

Kuhn:
It's a beautiful thing, you're quite right. That was one of the papers I had read shortly before you arrived and it was one of the very exciting moments in there.

Oppenheimer:
It's a lovely paper.

Kuhn:
Just in a general fishing expedition let me ask you more generally whether you remember any sort of specific episodes of meeting of minds or failure of meeting of minds in colloquia or elsewhere, particular sorts of concerns over problems on Dirac's part, Jordan's, Born's or anyone else's.

Oppenheimer:
I think if we keep within the rather narrow group of Jordan, von Neumann, Born, Dirac, and, as far as I can remember, visits from Pauli, Wentzel and so on, the mood was 'let's get going boys'. If was a very optimistic mood; in the background, a feeling of superiority over the wave lovers, a feeling of confidence, not even necessarily over-confidence. I think Ehrenfest quite rightly felt that this mood was too cocky and above all that formalism was tending to outrace the understanding. I don't know when he wrote his "Erkundigungsfragen," but it's later; but certainly he started to get ready for it then.

Kuhn:
I asked you when the recorder was off, and you said we'd come back to it at the right chronological point, about the extent of Dirac's concern to explain spin, how much that may have been in the background of the paper which does explain it but looks as though it explains it rather as an accidental by-product.

Oppenheimer:
Well, I wasn't actually with him when he did the spin paper.

Kuhn:
But he had already been in Copenhagen and it's from Copenhagen that that story of the

Dirac-Heisenberg bet, which Dirac finds so implausible, comes from.

Oppenheimer:

He had it on his mind, there's no doubt of it. He talked of it.

Kuhn:

Do you have any reason to think that he was working on it?

Oppenheimer:

He may have been late in the spring. He certainly wasn't most of the spring. He had this monstrous dispersion paper and the 'p' representation scattering paper; those things I think were the ones that he had on his mind. He could have been trying out some algebra. I wouldn't be astonished if the discovery of the matrices wasn't the thing that sparked the whole thing.

Kuhn:

Yes. It certainly looks that way, though, in the paper—as though it simply fell out with the matrices. One person you clearly knew then and about whom it is now obviously very difficult to get information is von Neumann.

Oppenheimer:

I think his interests and mine were not like this, but we got to be quite good friends. I liked him very much and he had a mind which really was in some ways not like any I have ever known. He had a free spirit. I imagine that we talked about physics and mathematics rather less than we talked about other things. I couldn't now tell you what paper von Neumann was working on, but I guess he was beginning to think about the generalization of Hilbert space and unbounded operators. I guess he and I must have known that some of my troubles had to do with some of his troubles, but I'm sure neither one of us could have helped the other.

Kuhn:

I think I can't ask you anything more about Göttingen at this point, and I think that rather than take you back to the Jefferson physical lab maybe the thing to do is to have a bite to eat and come back to it.

Oppenheimer:

That's fine with me; it's probably a good time for that. We have a few more years to cover, but probably spottily because most of what I was doing in those years was perhaps not directly related to this. It was in those years that I began to get interested in explaining and teaching. Yes, we'll come back to that.

[Pause]

Oppenheimer:

I think that I must have visited Leiden after I left Göttingen before returning. I left Göttingen fairly early and I don't think I came right back. I might have gone back to Cambridge for a few days too.

One of the things that I had heard about and which kept looking like a significant puzzle but turned out not to be was the fact that for the noble gases the electron cross sections get very low at low energies; as far as I know no other gases showed such a remarkable transparency so that it looked like something that had to do with the shell structure of the atom. I first just tried to see whether ordinary wave theory had room for such an effect and I talked about it a little bit in Leiden. I wrote a preliminary paper on it and sent it to Ehrenfest from Harvard. I was then also I guess thinking about the auto-electric effect. I don't know what else I was doing. I made a mistake in the paper I sent to Ehrenfest and he sent it back with the mistake corrected and a rather cross complaint that he should have to find a mistake.

Kuhn:

What sort of mistake was it? Straight in the mathematics?

Oppenheimer:

Yes, just forgetting a factor in the numerator or the denominator—just a blooper, something you would expect to get a cross on in an examination. But when I got to Cal Tech, apart from the transmission business, I went back to the electron collision thing and realized that exchange must have an effect because here the closed shell structure would fit in and I thought that that might conceivably have something to do with the Ramsauer effect. It has, if anything, very little to do with it; it is in fact fairly much an accident that there is a connection between shell structure and these cross section phenomena which are not deep and not interesting, but it did lead to a formulation in fairly general terms of exchange in collisions. Again I didn't know the phenomena which would be most interesting to explain in terms of this. That year I was on a fellowship and—

Kuhn:

Tell me when the fellowship came about.

Oppenheimer:

I think in the spring when I was in Göttingen they wrote to me and asked if I would like a National Research Fellowship and I said yes. I think I probably decided then, but certainly early, that I would like to go back to Harvard but that I would also like to go to California. I had never been to California except very briefly as a visitor.

Kuhn:

Was it primarily geographic or were you interested in seeing Epstein?

Oppenheimer:

I was interested in Cal Tech which was quite a place. Millikan knew that they were isolated and therefore he had a program of visitors, and I believe that it was at that time that Ehrenfest came to Cal Tech and that we did this really rather trivial thing on nuclear statistics.

Kuhn:

I think that's later. I think that's when you were on the faculty there about 1929. That actually was a paper sent in at the end of 1930.

Oppenheimer:

Well, I don't know then who visited there that spring. I came in January. I did get to know Pauling quite well and Tolman, though that began more slowly, and I liked the life. I forget if there was a relevant visitor; of course I knew Epstein and we talked to each other. I don't know that there's a record of any other great preoccupation, but I probably talked about both these things in seminars. I would think so. It was during the spring of that year that I got quite a few invitations, a couple in Europe, not permanent positions but "Assistant" and so on. I sort of sorted it out; I visited Berkeley and I thought I'd like to go to Berkeley because it was a desert. There was no theoretical physics and I thought it would be nice to try to start something. I also thought it would be dangerous because I'd be too far out of touch so I kept the connection with Cal Tech partly because I liked it but largely because I thought it would be a place where I would learn, there would be criticism, and that would be much better for me. I think I must have agreed to that before I left again for Europe. I think I must have accepted these. I don't think there's any publication beyond the two subjects I mentioned.

Kuhn:

No, I think not.

Oppenheimer:

But I would have been reading as assiduously as I could in the current literature, and I had some T.B. which affected my plans in the next year. It was never very serious, but I did go to the mountains for the summer.

Kuhn:

This is still before Europe?

Oppenheimer:

This is before Europe and I was a little late in getting to Europe because of this illness. I had thought to go first to Leiden and then to Copenhagen.

Kuhn:

I want to bring you back a little bit more if I can to the situation that you found at Cal Tech and perhaps also a little bit more as to what may have been represented in the effort to build up the Berkeley physics department by their interests. It is indeed a side issue, it's quite true, but the whole position of America has changed so radically in these years.

Oppenheimer:

Well, when I first went there Lawrence I guess perhaps had not been asked but certainly had not accepted, [S. K.] Allison was on the faculty, [R. B.] Brode, [L. B.] Loeb. The man who taught theoretical physics was a retired army officer, a self-taught man called [Wm. H.] Williams, and a singularly sweet man who loved what he understood of physics, which was not trivial but a little bit cold and bookish. I think maybe the impetus to my invitation came because Franck visited there and they said, "Whom should we get?" and Franck said, "Why don't you try—" I think Franck told me that or somebody told me that. Birge I think was not yet chairman—

Kuhn:

Not yet chairman, but I take it an important force—

Oppenheimer:

He probably was one of those who was involved in the invitation. At Cal Tech I don't know who was involved or how it happened. It could have been Epstein.

Kuhn:

What sort of force was Tolman at Cal Tech?

Oppenheimer:

Very great, but whether that was true at that time I don't know. He was rightly very highly respected; his wisdom and broad interests, broad in physics and broad throughout, his civility, his extremely intelligent and quite lovely wife, all made a sweet island in the Southern California (horror). I didn't get to know him well until the spring, perhaps March, when a friendship developed which became very close. Who all was there? Dubridge I guess, but among theorists maybe Eckhart was still there.

Kuhn:

I'm not sure whether he was.

Oppenheimer:

I think if he had been I would remember it. I knew Carl Anderson; I got to know Bowen; I met Lauritsen and we hit it off very well; but these are not directly the history

of quantum physics anyway. But I liked it enough to want to come back and enough to feel that it was a place where I would be checked if I got too far off base and where I would learn of things that might not be adequately reflected in the published literature. So I didn't get much of a look at Berkeley; I think I was there a week or less and fell for it but didn't see much about the physics. We may have talked about what sort of courses I would be giving but I had made it clear that I thought it would be good if I went back to Europe for another year [1928-1929] and I don't remember when that was settled or how.

Kuhn:

Your fellowship was extended for that?

Oppenheimer:

Then it was an International Education Board Fellowship and, as I say, originally conceived as Ehrenfest wanted me to come to Leiden. He was sort of short of people and I was a Fellow but I was also to act in part as his assistant. I would then have gone on to Copenhagen but for two reasons which we come to. I saw a lot of [A.] Rutgers whom you may hardly know because his interests have changed, but he was then Ehrenfest's assistant. I got to know a lot of people who were in the game and it was quite a lively life; Uhlenbeck, Goudsmit, Casimir, (Kuiper), Gorter were all members of Huygens, the scientific society which I attended regularly. I gave a seminar or two in Dutch; I don't think it was very good Dutch but it was (appreciated). I didn't do any instruction although there were some younger students whom I saw and worked with. I ought to remember what I was working on then but I'm just afraid I don't. There's probably no record or evidence.

Kuhn:

Unless your paper on electron radiation and Coulomb fields goes back to that.

Oppenheimer:

No, it didn't I had no further interest in that. Then I went for a month to Utrecht where Kramers was. Now there was physics going on and we were talking about it. I ought to be able to remember. It may have been a time when I was studying what other people had done. The literature was by then getting pretty big. I know that it was a time when I was working not too much because I wasn't well, but busy with physics, though not entirely, because I was reading very widely in other fields. I ought to be able to put my finger on it but I can't.

Kuhn:

I'm interested to know how you felt about Ehrenfest's grasp of the new physics and his attitude toward it at this point.

Oppenheimer:

I think that I appreciated it but I don't think that I appreciated it fully until somewhat later. I think that his interest in simplicity and clarity was really a very great thing but I probably still had a fascination with formalism and complication so that the large part of what had me stuck or engaged was not his dish. And some of the things that were his dish I didn't appreciate how really valuable it would be to have them in clean, good order.

Kuhn:

Had you seen anything of him while you were in Göttingen?

Oppenheimer:

I think in the spring I stopped by. As I say, I met him first when he came with Joffe to Cambridge and he and Blackett and I spent a long day together, and then again in Leiden in the spring of that year [1926]. But of course when I went back—it's puzzling—when I went back I speak of Huygens and of Uhlenbeck and all the rest but I don't remember any living relation between Uhlenbeck and Ehrenfest. Have I got two different times mixed up?

Kuhn:

Uhlenbeck could already have been at Michigan.

Oppenheimer:

Could he have left, say, after I'd been there a little?

Kuhn:

That's perfectly possible.

Oppenheimer:

I never was in Leiden except that time. I visited there, but I remember being a member of Huygens which implies something steady. I remember that it was Uhlenbeck that took me. Then I have no more sense of Uhlenbeck's being around. [Discussion of when Goudsmit and Uhlenbeck came to the U.S. is here omitted]

But the important point really is that there was not a great deal of life in the physics in Leiden at that time. I think Ehrenfest was depressed; I don't think that I was of great interest to him then. I don't think that he told me what was on his mind and I have a recollection of quiet and gloom.

Kuhn:

This is quite appropriate to the question I was about to ask which was really 'why did you go there'? You say you'd planned to go to Copenhagen in the spring.

Oppenheimer:

I went to Ehrenfest because he had asked me to and I was a very great admirer of his. I thought of him, and I think not wholly inappropriately in semi-Socratic terms, and I thought I would learn something from him and indeed certainly did; but there must have been some distraction, and it cannot have been only the fact that I wasn't too well, which spoiled this period from the point of view of physics because I would not otherwise have left Leiden to go to Utrecht.

Well, it was two things that deflected me from Copenhagen: on the one hand the rather strong feeling that I should get a moderately salutary climate—Switzerland, it turned out, was not a very good idea since Schaffhausen on the lake at Zürich froze that winter; and on the other hand, Ehrenfest's certainty that Bohr with his largeness and vagueness was not the medicine I needed but that I needed someone who was a professional calculating physicist and that Pauli would be right for me. I think he used the phrase, "herausprügeln."

Kuhn:

That's terribly interesting because so generally Ehrenfest sent people to Bohr.

Oppenheimer:

Yes, but he didn't think that was my dish, that I was ready for it or that I would appreciate it. He thought in other words that I needed more discipline and more schooling and I did see a copy of the letter he wrote Pauli, maybe later, I don't know, but it was clear that he was sending me there to be fixed up. I stopped in Leipzig and heard Heisenberg talk about ferromagnetism in his institute there on the way. I'm disturbed that I can't remember more of the time with Kramers because I know we talked physics very much but I don't remember what he was doing. Oh, I had done the capture of electrons, this other use of the almost orthogonal functions. This interested him and I think we talked about it. He and [H. C.] Brinkman did it over and did it better, but I don't remember the date on that. This probably was also something that I had in mind in Leiden.

Kuhn:

This paper that I asked you about before in which you do treat the question of the effect on absorption spectra of the heavy ionization in the interior of stars which you submit from Zürich—

Oppenheimer:

That's coming next.

Kuhn:

In spite of the fact that you take off from Kramers' formula it has nothing to do with your having visited Kramers in Utrecht.

Oppenheimer:

No. But the quantum theory of the capture of electrons was published in '28 and was probably something I did in Pasadena.

Kuhn:

You actually submitted it still from Jefferson in December '27.

Oppenheimer:

Right. Then there's a gap because the last thing is still about Ramsauer effect and auto-electric currents and the next thing is again the continuous spectra, "Über die Strahlung der freien Elektronen."

When I got to Zürich Pauli told me a little of his work with Heisenberg and I showed I guess more than a little interest in it. It was in a ghastly state and we did, off and on, work together on that; the part of it I got into was something it was at first thought the three of us should publish together; then Pauli thought he might publish it with me and then it seemed better to make some reference to it in their paper and let this be a separate publication. But Pauli said, "You really made a terrible mess of the continuous spectra and you have a duty to clean it up, and besides, if you clean it up you may please the astronomers." So that's how I got into that, and he had one trick, one integral that he had done that he thought would be helpful and otherwise it was not his dish at all. He asked me to do it.

But I was then very much interested in field theory, and of course quite interested because Bloch, apparently from a wholly different point of view from anything I had thought about, had got into the conduction problem and we talked quite a lot. I think Peierls came later that year. Rabi was there; we were friends but I don't remember what he was doing. I think he was rounding out his education. It was a very useful time. I got to be not only extremely respectful but also extremely fond of Pauli and I learned a lot from him. The time with Ehrenfest had seemed terribly inadequate to what was really in Ehrenfest and the time with Kramers had seemed good but not very substantial—very good personally, very close, but not very substantial. The time with Pauli seemed just very, very good indeed, but I did get quite sick and had to go away for awhile. I was told not to do any physics; I didn't do much, but that was six weeks and when I came back I had practically everything that I was going to do on the self energy problem, the fields, but I didn't have it written up. Perhaps it wasn't yet decided in what form to publish it. The other thing had a mistake in it but part of it anyway was right and it was very easy to get it completely right after that. I never had any pride or joy of it. But I was very much interested in the electron problem and relativistic theory.

Kuhn:

Were you still in Zürich when Dirac's relativistic electron paper [*Proc. Roy. Soc. A 117* (1928). Rec'd 2 Jan. '28] came out?

Oppenheimer:
I think that's where I saw it, yes, I think so.

Kuhn:
Yes, I guess it would have to be because it came out very early in the year, or at least it was submitted very early.

Oppenheimer:
Yes, I think we probably first had word of it and then I think a letter; I think maybe Heisenberg knew about it.

Kuhn:
How did that strike all of you?

Oppenheimer:
Quite magic, really, quite magic. Wasn't it incorporated into the description of electrons in the Heisenberg-Pauli paper, or did they not do that? Those papers are so (terrible).

Kuhn:
My feeling is not, but I'm not perfectly sure; I've looked at that paper but I'm going to have to spend quite a long time with it before I—

Oppenheimer:
There are two limiting processes. There are two papers too many, and two limiting processes too many, but that was not the aspect that I was involved with because really when you get it it's very simple; it is just the awful torture of not knowing what to do about redundant variables. Don't I say in my paper that there are also problems with the Dirac theory but they don't seem like the same problems?

Kuhn:
You do that in a paper that I think is a little bit later.

Oppenheimer:
Right, right.

Kuhn:
That is, you've got a paper that's published in '30 that went in from Berkeley in November '29, "Notes on Theory of the Interaction of Field and Matter".

Oppenheimer:

Well, that is the work I did mostly in Zürich. That was the third part of the Heisenberg-Pauli paper.

Kuhn:

But the only thing is that I think this we can't quite read back to determine how deeply the Dirac paper may have been involved with the Heisenberg-Pauli paper itself.

Oppenheimer:

Oh, it wasn't deeply involved; they were interested in the quantization of field and the unification of Coulomb and radiative fields and for them it didn't matter; as long as there was some relativistic theory of the electron it didn't matter. But I think that formally we were actually writing down the one-body Dirac equation, charge and current. I was. Anyway, it was in the air. Pauli, you know, was very resistant to the Dirac business.

Kuhn:

I do know that, and I wonder if you can talk more about the nature of the resistance, the reasons for it and the sort of things that then went on.

Oppenheimer:

The negative energies seemed to him an incurable illness. He didn't of course at that time know of hole theory, but he thought that any theory which had such a sickness must agree with experience only by accident; on the other hand, the algebraic properties were always pleasing to him; he probably got to work on it early. I forget when he did the Poincaré lectures on the Dirac algebra.

Kuhn:

Still many people apparently weren't much bothered for some time, and at least until after certain of the interaction terms were worked out, because this was also a classical problem.

Oppenheimer:

The Klein paradox is not a classical problem.

Kuhn:

No. Exactly.

Oppenheimer:

It may be possible to get into trouble in classical theory, but it's also easy to keep out of it. The square root is not against anything in classical relativistic dynamics but it's against causality, conservation, and everything else. This we talked about a lot I thought.

Kuhn:
Were you more favorable to it than Pauli?

Oppenheimer:
Yes. Let's put it this way: I didn't have the weight to be as unfavorable. I was worried about it and concerned and not as pleased by the charms of the algebra and the hydrogen atom as someone who had sweated more with it would have been.

Kuhn:
You make a remark in this paper that you say was largely done there that is interesting. It expresses the hope that as this thing is worked out the problem of the apparent arbitrariness of the Pauli principle will vanish. Was this something also much under discussion? Why anti-symmetric wave functions?

Oppenheimer:
I would think so, yes, I would think so.

Kuhn:
I know Heisenberg worried about it a good deal.

Oppenheimer:
I think the '30 paper was the one. Published early in '30 but maybe submitted in '29?

Kuhn:
Yes, submitted November '29.

Oppenheimer:
I think that there I used anti-symmetric wave functions. I had to really in order to have it work out. Let me say what I believe to have been the state of affairs. I think when I came back to America I had learned both something of the problems and techniques and some of the point of relativistic theory and also very great and open and dark questions, of which the infinite self-energies were an acute example and the negative energies were an acute example. Then the fact that there were the loose ends of the Pauli principle—the spin and all the rest of it. I would think it only reasonable that about these questions Pauli and I talked a great deal. I don't remember talking too much about the Bloch theory but I did talk with Bloch, especially about super-conductivity which he was beginning to gaggle at.

Kuhn:
Might you have gone on with the conduction problem further yourself if he and the Leipzig and Munich groups had not carried it off so far, or were you pretty clearly already committed to the idea?

Oppenheimer:

I think that the Bloch formulation of the conduction problem in a certain sense made me lose interest. It was too easy and it didn't seem to me to give a sufficiently pictorial view of how the electrons got about. Now this got all right in time, but instead of exciting me it made me feel as with the Ramsauer effect really—that what had looked like an exciting problem wasn't really one at all.

Kuhn:

How clear was it during the time you were still at Zürich that the problems of the field were really not going to come out the same way? This is, it was already clear that they were tougher than the other problems that were responding in this period, but was it clear they were not going to come out the same way?

Oppenheimer:

No, that was the point when I talked about infinite energies, about negative energies, about the exclusion principle, about the existence problem of the particles. It was clear that this was a new chapter and that it wasn't in the book. It wasn't clear that another Dirac miracle wouldn't work with this, that one couldn't explain these things and get them in order by something that was within the framework of quantum theory as the Dirac electron appeared to be. And then of course things changed.

I must have seen Dirac's note on electrons and protons shortly after it came out. I think that year [1929-30] I went first to Berkeley and came at Christmas time to Pasadena. My recollection is that I saw this in Pasadena. I guess the following note, or actually paper, on radiative transitions had something about the annihilation. You could then ask 'what did I think?' Well, obviously I thought that the proton system and the electron system were separate and in normal experience one had only the one sign of charge. I don't think that I thought about mechanisms which would produce pairs until the Anderson thing. I think that I had no opinion as to whether this conclusion of the theory would be born out. This may seem odd because if they could be annihilated they certainly could be produced, but it isn't the first time and it wasn't the last that one wondered really whether detailed balancing was right. This happened with the strange particles too of course and with the new meson and so on. It's always been right and I think it's probably one of the few things that will continue to be, but I would just say that puzzlement was it. I talked to Anderson about it—

Kuhn:

Before the positron?

Oppenheimer:

Sure, and he talked to me, but I didn't encourage him to think that this was a good experiment and he didn't look for positrons because there might be a place for them in

a theory of whose general rightness no one was at all sure. Pauli thought it was nonsense; you find that in the relativistic part of his handbook article. Bohr not only thought it was nonsense but was completely incredulous when he came to Pasadena. It wasn't until—not that he'd seen the picture—that helped—I could explain to him how naturally the pair production would have to come out if this was a correct view at all that he became convinced. He left Pasadena convinced that it was a consequence of the hole theory and that this was genuine progress. I think there was a World's Fair in Chicago and he went there and when he talked about it he talked about having become convinced of this. That I think went on in Pasadena not least because there was a beautiful photograph but primarily because he hadn't thought about relativistic theory and changing particle numbers and all such things and it was reassuring to him that the framework was there and that if there were troubles with it they were no worse than the troubles with light quanta in the hydrogen atom. They were the same kind of pushing a theory beyond what the traffic was good for.

Pauli had already said in Zürich when I was there, "We won't get over this until we can calculate the fine structure constant, until we understand the fine structure constant." So this was again a rather exciting time. It could have been exciting because of the neutron, but it was in fact quite exciting to see again that a formalism arrived at for very different reasons, validated in an entirely different context, had a content which was rich and which explained many things which people had been worrying about. In Cal Tech of course I constantly got bombarded with cosmic rays and was aware of the acute paradoxes of their behavior. That's a very natural thing.

I don't quite understand why there are two papers on the statistics of nuclei.

Kuhn:

I suspect that one of those is an abstract or something.

Oppenheimer:

Yes, it should be so labeled. It's short—maybe a letter. [phone interruption] I'd thought about it a little bit—

Kuhn:

The second one of those or the one that's you alone I just don't know anything about.

Oppenheimer:

The only one I know is with Ehrenfest; it's about five pages and the earlier one was one page. They're rather close together in time. Well, that's minor. Perhaps I'd had a look at it and given an argument that you couldn't get out of the nitrogen problem, and then when Ehrenfest came he said, "I'd like to see if we can't make this a little fuller." I think I introduced an idea of clusters, we worked with that and it didn't come out.

Kuhn:
Was there any thought at this time that the spin of the nuclear particles might turn out to be other than a half?

Oppenheimer:
Well, the neutron hadn't been discovered. I don't quite know what you mean.

Kuhn:
I may be off on this. You're not concerned here simply with the—

Oppenheimer:
The problem was, "Can some not-understood bonding of electron and proton make a nitrogen nucleus?" and this was essentially a proof that it couldn't, a proof that had nothing to do with the nature of the interactions which made the bonding but just with space-time.

Kuhn:
As long as the electrons have a half it's not really going to matter in this one—

Oppenheimer:
That's right. Of course the protons we knew; that was an experimental thing.

Kuhn:
I don't think it was quite yet.

Oppenheimer:
Oh, sure, from the hydrogen spectrum. It stands out like a sore thumb—the intensities in the band spectra. I'm sure of that.

Kuhn:
It's only that I've had this story that I haven't been able to look back at the literature on at all about Stern going around and getting everybody to bet on what it was going to be and everybody's being wrong.

Oppenheimer:
That was the magnetic moment.

Kuhn:
Yes, yes, o.k. That is exactly what I'm forgetting at this point. It at least explains my whole group of questions, if fruitlessly. Just a couple of things. I realize that one topic we've skipped over entirely in doing this—we got up close to it and didn't quite get it—

is—

Oppenheimer:

I'm still looking at this; I'm puzzled by a few things. I don't know what selection rules and angular momentum of light quanta was all about.

Kuhn:

Yes, that's one we missed.

Oppenheimer:

Could that have been when I treated the light quanta essentially with what we would call the Kemmer matrices, the linearized equations?

Kuhn:

This is one we didn't pick up.

Oppenheimer:

I think that's "Light Quanta and Electromagnetic Field" [No. 18] because it's a long paper. This is presumably just a brief abstract of it and it may be related to it; I'm not entirely sure. From here on I think everything follows pretty much from what we've already said. The positron, the disintegration of lithium by protons, the possibility of neutral magnetic objects in cosmic rays, the meson, magnetic moments of the neutron, neutrino.

Kuhn:

Can you say anything about the early neutrino idea? This is one you refer to and presumably from Pauli's verbal formulations of it … You speak in one of your papers about the possibility of an effect's being involved with Pauli's neutron at a period before there—

Oppenheimer:

—was a neutron?

Kuhn:

Presumably Pauli's neutron is the neutrino idea still without that name. The neutron then in '32 preempts the name and this becomes the neutrino finally. The early history is obscure because the idea kicks around in the profession by word of mouth for at least a couple of years I think before it gets into print.

Oppenheimer:

The first publication was Fermi.

Kuhn:

Yes, I think that's right.

Oppenheimer:

But you know this was in the Copenhagen "Faust"; the neutrino was the Gretchen. It was being talked about but as far as I know Pauli didn't publish it. But I knew something about it; he probably wrote me about it. Anyway it's still the question of whether such electromagnetic properties can be assigned to a zero mass particle consistently. Of course some it does have but they are of much higher moments and it's not settled. Well, there is a certain muddle here because I wrote one paper treating light quanta like electrons and showing how the positive-negative energy thing appeared for them. I wrote one paper on impact radiations—that may be earlier, yes. I don't see it here. Some of them I would sweat I didn't do.

Kuhn:

Maybe it's just that I saw the impact radiation thing on yours. Well, that one for the moment I think we can let go.

Oppenheimer:

It's all getting very late.

Kuhn:

Let's go back to a couple of things. One of the things I'd like you to talk about if you can is the uncertainty principle, the sort of discussions to which it gave rise, the way you and other people felt about it. Another thing which is not independent of this entirely is really the question of Bohr and the uncertainty principle. I'm interested that you were going to Copenhagen and that Ehrenfest suggested that wasn't it.

Oppenheimer:

I didn't really meet Bohr until he came to California.

Kuhn:

Well, you met him at Cambridge.

Oppenheimer:

Yes, but that was an introduction. Then I would say that it is improbable that I saw Bohr until he came to California in the '30's. He came I think maybe twice; the second time I saw a great deal of him and we talked very much. He was then interested in nuclear problems. He brought [Fritz] Kalckar and left him with us, but that was very late compared to all of this. There is one thing, if I could lay my hands on it. They made a syllabus of the course I gave in Berkeley which was quantum mechanics. I think that expresses pretty well how I approached these things. I don't think we have any left.

That one has not been translated and put into hard covers. The Japanese did the electrodynamics, but I don't think they did this one. Schiff's book is different and it's certainly very different in style, but it is largely based on that course.

Kuhn:

Let me go back if we can to the point where the uncertainty principle first breaks, that is, the Heisenberg paper. Where would you have been then?

Oppenheimer:

That was '27?

Kuhn:

Yes.

Oppenheimer:

So I would have been moving between Göttingen and the United States. I'm almost certain that I gave a seminar on it. I do not think that I reacted at all as Nohr did by saying that one should start in a much simpler way and a much deeper way, but I was terribly troubled by the fact that you couldn't localize an electron by these means and I couldn't think of any others either. That was for later reference one reason why when we looked into the pair business I felt very much at home.

Kuhn:

Most of your early work with that would have been in this country. What sort of an impression did the Heisenberg paper make here?

Oppenheimer:

In the United States? I think it made a very great impression; it excited a lot of people to take an interest in quantum theory who weren't professionally interested at all. I thought most of it was rather poor philosophy but—You see, I didn't have those problems. Certainly by the time I came back to the United States in '29 with the apparent resolution to stay here because I didn't go back to Europe for 20 years, I had for non-relativistic quantum mechanics a pretty good feeling, a pretty good understanding of what it was about, what it expressed in terms of the availability and limitations of measurement, a sense of what you could do in relativistic problems, and a sense of great gulf about the fact that it didn't look as though the round we had just lived through was going to fix this up, and a very great sense of alertness as to any clues as to what might be opening up there. That's why I came back again and again to the funny business with the cosmic ray. But as for quantum mechanics, I taught it starting essentially with wave-particle duality and the complementary nature of the experiments that you could do. I would just make the paradox as bald and inescapable as possible, then take quite a long time over that and give exercises on it in order that this experience would not be buried in the formal facility with the theory. Now whether people took other views—I would

assume so. It's certainly not an aspect of wave mechanics that interested or appealed to Epstein; there weren't a hell of a lot of theoretical physicists permanently around in California in 1929. Couldn't we think of anybody? In Stanford, or Cal Tech or Berkeley.

Kuhn:

In addition to this question about reception, you say that you taught this course from the state with emphasis on wave-particle dualism and complementarity. Now I'm terribly interested in the combination of those two because although they clearly combine for everybody today, complementarity is in the Heisenberg paper and wave-particle dualism isn't at all. And both seeing that and putting those two things together is the thing that Bohr brings into it. On the other hand it's quite clear that in the early days in a number of places, including some very bright people, Bohr made no dent at all with them. It was Heisenberg, so far as people were concerned. They saw what Heisenberg had said and they thought Bohr was groping.

Oppenheimer:

Well, you see, the time I spent in Leiden and in Zürich could well have been very helpful in this respect because there was no doubt that Ehrenfest knew what Bohr was up to. There was also no doubt at all that Pauli knew what Bohr was up to and no doubt with either of them that Bohr's formulation was the more general, the more inescapable. In fact Heisenberg's was not even quite right technically, but the business of devising experiments was a game because you could always really read it off from the formalism; but it was a remanding game because you had to read with great care. I forget who it was; was it Ehrenfest who first deduced the uncertainty relations in a very simple way?

Kuhn:

Well, he certainly did one of the early derivations. I think he first showed the wave packet as a classical—

[Break in tape]

Kuhn:

What was this transition like, for somebody who has been doing research at a number of the most important centers who now set out not to drop that but to combine with it the start of a school? How did teaching itself come to you at this time?

Oppenheimer:

I think that the whole thing has a certain simplicity. I found myself entirely in Berkeley and almost entirely at Cal Tech as the only one who understood what this was all about, and the gift which my high school teacher of English had noted for explaining technical things came into action. I didn't start to make a school; I didn't start to look for students. I started really as a propagator of the theory which I loved, about which I continued to learn more, and which was not well understood but which was very rich.

The pattern was not that of someone who takes on a course and who teaches students preparing for a variety of careers but of explaining first to faculty, staff, and colleagues and then to anyone who would listen what this was about, what had been learned, what the unsolved problems were.

I think from all I hear that I was a very difficult lecturer, that I started as a lecturer who made things very difficult. I had some help; I remember Pauling's advice, almost certainly in '28. He said, "If you want to give a seminar or lecture, decide what it is you want to talk about and then find some agreeable subject of contemplation not remotely related to your lecture and then interrupt that from time to time to say a few words." So you can see how bad it must have been. In Pasadena I taught all right, but it was never an important part of the Cal Tech curriculum except conceivably that first year in the spring of '30 when I was there a long time and where I probably gave a pretty good 'course of sprouts' in quantum theory.

In Berkeley I gave what was normally a graduate course and in practice usually a second year graduate course which had not been given before on quantum theory and quantum mechanics and which varied in content but was always all right for someone who had had some background in classical physics and preferably at least a qualitative introduction to atomic theory, though it didn't too much matter. I usually gave a seminar on one other aspect of theoretical physics, typically statistical mechanics and relativity, both things which I loved very much. But these were all with people who didn't have to learn these things but wanted to, so I was in a sense not that kind of a teacher at all. It was very rarely and only in quite different contexts that I ever worked with undergraduates. I think they didn't think I'd be any good for them and it didn't occur to me to ask to teach freshman physics or anything like that.

You live in the department and if it's a growing and active department as Berkeley got to be and as Cal Tech was, there are problems that arise because people are doing experiments. I found this a very great source of stimulation and pleasure and I think actually the beginnings of collaboration with graduate students came very early, but the students weren't very good and I picked rather exotic problems. It wasn't really until the positron and more or less the full shape of the relativistic debacle, the clues of cosmic rays came into the picture, that the collaboration with students began to take a more effective turn. But now we get into a manifestly later period, both in subject and the variety of things that I did—this problem in general relativity on stars, for instance, came from brooding about energy generation which we didn't do too much. I would think that the transition was rather from that of a person who had been learning and also explaining in European centers and in Harvard and Cal Tech to someone who couldn't much any longer learn from masters but could learn from the literature and from what he did himself; one who had a lot of explaining to do because there was no one else. I think it was not such a sudden transition. Living a life in which you lecture three hours a week and have a seminar, another lecture two hours a week, leaves you a lot of time for physics and for a lot of other things and I wasn't an altered character. I

was still primarily a student in terms of what I spent time on.

Kuhn:

Lecturing didn't take a lot of preparation time or energy?

Oppenheimer:

Well, it took energy, a great deal of energy, but I didn't have to look much up in the book and it was more a question of keeping the presentation fresh and making it sharper and richer. I would think that the big change was that I wasn't an apprentice any longer and I had decided where to make my bed. The fact is, really, I hardly left California until the war; I went to New Mexico summers but I hardly moved around at all. I came east to Ann Arbor summer school a couple of times. I gave a lecture on the positron at the Physical Society. But really I moved very little; I had really made a bed that I was content to be in. And this I think, the fact that I stayed in one place, that I was interested in spreading this part of physics and was really rather widely interested in physics as a whole, the fact that I felt responsible to my colleagues and for such students as came, and a lot of luck and accident, made it flourish pretty well. But I don't find much mystery in that. In a certain sense I had not grown up but had grown up a little, and I think if circumstances had been such that I had had to teach to make a living earlier it probably would have been better for me. I don't think it would have derailed my interest in physics but I think it would perhaps first of all have made it necessary for me to learn what I wanted to know.

Kuhn:

Do you miss it here?

Oppenheimer:

No. I could, you see, teach at the university; it would be clumsy, but it would not be impossible. I don't have a great yen for it anymore; I do like to work with physicists, I miss that, and I wish I were doing more of that here, but that's entirely up to me.

Kuhn:

If there is one respect in which people do have reservations about the institute it's exactly with respect to the two things you just said about yourself at Berkeley: one of them is getting their problems from experiment and the other one is the manner in which things get worked out in the teaching interaction or in preparing for teaching.

Oppenheimer:

Let me say that I think that in the world of theoretical physics the number of people who are better off here for life than elsewhere is not very large, and they do typically spend six months of the year here and six months of the year elsewhere. Dyson taught all last year, Yang is teaching all this year. Dyson will be at La Jolla next year, Yang is at Brookhaven much of the time even when he's here; and in these ways they do fulfill

those needs without which they couldn't survive. On the other hand, I think that for a postdoctoral fellow or for someone who is a practicing physicist that has lacked contacts or has become a little too enmeshed in teaching and responsibility at home, a year or, more typically, six months of the year with nothing to do but physics and plenty of physicists to talk to is wonderful. That's what this place is about.

If I were multi-valent enough, knowledgeable enough, and vigorous enough, we wouldn't need a faculty, if I could talk to all the members about their problems in all fields. We do need a faculty to provide the framework for temporary members, but as a place for permanent members we are not good for more than a very few people. There are one or two people who probably couldn't find the opportunities to work that they have here, but then they are not in physics or mathematics but in subjects in which research chairs are very much harder to come by. We are for the people who come in and out; that's what the place is for. I think that the charm went out of teaching after the great change of the war because I did teach at Cal Tech and Berkeley and for one thing I was always called away and distracted because I was thinking about other things, but actually I don't think I ever taught well after the war. I have a feeling that what my job was was to get a part of the next generation brought up and that job was done when I came here.

[End of interview]