American Meteorological Society
University Corporation for Atmospheric Research

TAPE RECORDED INTERVIEW PROJECT

Interview with Walter M. Elsasser

12 March 1986
Interviewer: Jeffrey T. Kiehl

Kiehl: What I thought we'd do--I wanted to ask you these questions, but don't take these as gospel questions. We can diverge or go any way that you would like to go. I just wanted some sort of framework to work with.

Elsasser: Where did you get your degree?

Kiehl: I was just as much a rolling stone as you were. My background is in physics, theoretical physics. I was working on high energy particle physics. Then I decided--

Elsasser: Where was that?

Kiehl: That was at Indiana University, at Bloomington, Indiana. Did you ever meet Emil Karpinsky? He was at Los Alamos with Hans Bethe.

Elsasser: I was never at Los Alamos.

Kiehl: O.K. I know you passed through that area at one point.

Elsasser: Yes. I was not connected to Los Alamos. I was teaching at the University of New Mexico, which is not the same place.

Kiehl: Yes, right. So I did my physics work in Indiana, and then I switched to atmospheric physics. I started working on electromagnetic scattering on particles. The person I was working with like to move around a lot. So I just followed him. He was from Czechoslovakia. My degree finally came from State University of New York at Albany.

Elsasser: Albany.

Kiehl: But I spent very little time there. I spent a lot of time in the Boston area.

Elsasser: Anyway, you have a background in physics, not in pure meteorology?
Kiehl: No.

Elsasser: That would be a little difficult…

Kiehl: Right, no.

Elsasser: [I have not been in touch with meteorology for years.] O.K., ask your questions now. I will try to give you answers.

Kiehl: For a while there I thought you were interviewing me. [Laughter]

Elsasser: No, I just wanted to know your background.

Kiehl: In your autobiography, you--just as I was stating, I was in physics--you started out in physics.

Elsasser: I still think of myself as a physicist.

Kiehl: O.K., good. That's a good point. When you came to the United States, you decided to go into the field of geophysics. Some aspect of geophysics--

Elsasser: Yes. Well, actually, I had thought of that as a possibility, but then it was almost thrust upon me. As I traveled to the United States I didn't have an offer from a university in this country. Two people told me that there are enough atomic physicists and nuclear physicists in the United States, and I wouldn't have much luck if I stayed in it. One of them was A. H. Compton--

Kiehl: Where was he at that time?

Elsasser: In Chicago. It was a small world. A young man could miss Compton in his office. He couldn't do that anymore; he would probably have to go through the FBI to reach him. The other man was Millikan. He said the same thing, and I was prepared for that, because I knew the conditions. And so I suggested to Millikan that I might go into geophysics. He was most enthusiastic about that. It turned out later that was the reason, in fact, that he told me that he was, during the First World War director of the Signal Corps laboratories. And then he found out that there weren't enough geophysicists. And then we were talking about the possibility and I ended up getting or he ended up getting a contract with the Weather Bureau. The Weather Bureau at that time was still part of the

Department of Agriculture. It was only later transferred to the Department of Commerce. I think it still is now. So I ended up with this little thing, and the choice of atmospheric radiation, that was my own idea, because I had substantial background by that time in quantum mechanics. You see, I had gotten my PhD in 1927. This was 1936, nine years later. I thought I might as well pick a subject to give me transition from physics to geophysics. So I suggested that to Millikan, wrote it up and applied for money and got funded. It was a very small salary, Millikan's. Very much of a Scotchman. [Laughter]

Kiehl: Why atmospheric radiation, though? Did you look around at different fields and things?

Elsasser: Because I thought that _____ spectroscopy. I knew quite a bit about spectroscopy, after all, growing up with quantum mechanics. I thought, well, it was quite obvious, and nobody had applied spectroscopy to atmospheric radiation. There were people who talked about gray atmospheres. That was about the state of the art at that time. That is besides the spectroscopy. So I proposed to study the spectroscopic structure of infrared radiation. This is what I did.

Kiehl: I think you've answered the second one which was--was there any significant reason for starting in the field of atmospheric radiation?

Elsasser: Oh, yes. And then in addition to that, I took a large course which was given at Caltech by a man named Krick. I don't know whether the name means anything to you?

Kiehl: Not to me, but I'm not a meteorologist.

Elsasser: He was a highly commercial meteorologist. And he made quite a lot of money out of this. He forecast when the clouds would break that caused high fog in Hollywood, and then the clouds would break in Hollywood, so that the studios could hire their extras at that time. We saved them a lot of money, some of which went into Krick consulting fees. And I was attached to him, but I spent most of my time in the physics department where I had an office, thanks to Millikan's exertions. But I was not a member, I wasn't made a staff member, because the physics department had revolted against Millikan, who'd been too aggressive in expanding the physics department. They said, no more. No more of these Europeans coming in loading down the physics department. Otherwise, they treated me very nicely. And I was technically under the meteorology department. What you call them now, you would call them research associates, something like that. The meteorology department was under von Karman. Probably that was not an easy situation. It required some delicacy. Von Karman wanted nothing to do with teachers, because Millikan pressured him. Millikan wanted to build up

geophysics [and] that was one of the ways he had to do it. And von Karman wanted his airplane work and he didn't care about meteorology. At that time, that was very flourishing because the war was coming, it was beginning to threaten and there were about twenty-five or more officers' candidates from the armed services who took the course as weather forecasters. So it was pretty lively. I took that course, too. It takes two semesters. It was very interesting. It was quite complete, small weather and big weather and everything. Now I shouldn't talk, just one of those--I should let you ask questions.

Kiehl: What was your perception of the state of atmospheric radiation at the time you began your work? You mentioned the fact that it was a gray, people were assuming gray gas, gray gases.

Elsasser: That was all, there was nothing else.

Kiehl: So when you began your work, did you do any analysis of what had been done in the past, or were you influenced by any other persons' works, or did you just start from scratch?

Elsasser: I had to start from scratch, because there was nothing but the idea of the atmosphere. And if you have that, you can write down an explanation ?function, that's all. Then eventually, after a couple of years, I invented this scheme of the periodic spectrum. That's simple enough, just straightforward analysis.

Kiehl: Were you aware of any work that was going on before you came to the country, such as ?Emden's work in Germany?

Elsasser: No, I learned about that later.

Kiehl: You learned about that after you started in atmospheric radiation.

Elsasser: Emden was a ?son of--

Kiehl: One of your best known contributions in atmospheric radiation is the so-called regular band model, with the lines--

Elsasser: That's the one I just mentioned.

Kiehl: How did you think of that approach to band modeling?

Elsasser: I don't know. Once in a while, you'd find out--

Kiehl: Did you know that it would yield a closed form? That you could do all the

integration, everything, exactly before you started? Or did you assume--?

Elsasser: Well, I had that in my mind. Whittaker and Watson. I knew about residues. And I figured out that if you could put a series of residues next in line you could get something on the reel.

Kiehl: So it was actually the mathematics--you knew about the mathematics before you started attacking the problem physically.

Elsasser: Oh, yes. I searched a little, but it was easy to find.

Kiehl: In your autobiography, you described so much your relations with Robert Millikan at Caltech as you've been mentioning. Did he remain interested in your research in atmospheric radiation after he suggested that you try to go into the geophysics?

Elsasser: He was interested in so many things. That was just one of them. I don't think he was especially influenced. I think he was interested in mostly administrating. He was always very nice, and it was always a pleasure to have an interview with Millikan. One went to his secretary and said, "Could I see Dr. Millikan?" And she said, "Well, let's see. Maybe you could come in tomorrow." It never took more than a day. "Come in at 10:30." And so I came in at 10:30, and there was Millikan showing out the previous visitor, and he asked me to come in. They he usually kept standing and remained standing, looking out the window, and he would talk to me. After about twenty minutes, he had a brilliant way of moving his visitor slowly to the door. And at the end of the half hour, he opened the door and you were gone. It was an amazing performance.

Kiehl: Was he, at that time, still doing science? Or was it mostly administrative work?
Elsasser: Oh no, Millikan hadn't done science for years. But he was involved in what was going on. After all, [it] was the government--[there was] great research going on. He took a very active part. But I mean he didn't get his fingers dirty.

Kiehl: At the time you were working on these problems, did you have any contact with any other people who were working in the field of atmospheric radiation? People in England, such as David Brunt, or Callendar--?

Elsasser: Eventually I got to know Brunt. I had very little such contact; I was very much on my own.

Kiehl: Was there much correspondence that you had with them? Did you have any correspondence with these people, or meet them in meetings?

Elsasser: Very little.

Kiehl: Were you aware of Møller and Mügge's work before you started to work on your atmospheric radiation chart?

Elsasser: I think I had read some of their papers. But that was about all. I don't think anybody else ever did a very spectroscopic--looked at this from a spectroscopic viewpoint.

Kiehl: The only paper that I could find, which is actually cited in one of your papers, was Fritz Albrecht had looked at the spectro-water vapor around 1930. But it wasn't quantitative at all. It was more of a verbal description of some spectra. Whereas your work is certainly more quantitative. You try to--

Elsasser: I always feel my atmospheric radiation work was something of a failure.

Kiehl: Why is that?

Elsasser: Because I thought I should be able to apply this to the greenhouse effect. But my results were always that the absorption goes only very slowly with optical mass _____ out of this simple model that comes from a square root law. I thought that's just too little. I always read papers by people who talk about the threatening greenhouse catastrophe. I thought, well I can't, I can't agree with these people. I don't find enough of a quantitative effect, and I don't want to ruin my reputation by sticking out my neck and saying, "There is not going to be any much significance to the greenhouse effect." I might be shown to be wrong and it would just come back to me.

Kiehl: That's one of my areas of work in fact, is the greenhouse effect on various trace gases. We keep putting more and more different gases in the atmosphere, and they all have the ability to absorb in the atmospheric window. So there should be some potential greenhouse gases. You met John Strong at Caltech?

Elsasser: Oh, yes. _____ [Garbled]

Kiehl: Could you elaborate on the influence that he had on your work?

Elsasser: Yes, he had a considerable influence because I had originally started to do some experimental work in atomic physics. But I gave that up because I found that it was just too darn clumsy and then I became a theoretician. Then I saw a great deal of John Strong during that period. He was really about as much of a genius as I've ever met. _____, doesn't do to most people. He had a way of doing simple things accurately. I did some experimental work under his

general supervision. I built the gadget.

Kiehl: So you actually built the instruments yourself.

Elsasser: I built it in my own hands. Because I didn't have anybody available. I had the mechanical department which was very helpful if I had something specific. But I neither had money, nor any assistant to do the work for me. So I had to do it myself.

Kiehl: Did you maintain contact with him after you left Caltech?

Elsasser: Yes, I still see him once in a while.

Kiehl: Yes, I've been thinking about talking to him as well.

Elsasser: He's almost blind.

Kiehl: He's in Massachusetts?

Elsasser: Yes, he lives in Amherst. He's about my age, you know. 81, 82 pretty soon. At that age--

Kiehl: It happens to everyone. It's a part of life.
Elsasser: Sure.

Kiehl: Besides John Strong, were there any other researchers at Caltech that had any influence on where your research went as far as----

Elsasser: No. Once I had staked out this field, it was pretty straightforward. It was just a matter of lots of numerical routines because _____ velocity(?) of numerical routines. At that time we didn't have computers.

Kiehl: Right. When you went to MIT in the late thirties-----

Elsasser: Just the summer.

Kiehl: Did you talk at all with Rossby?

Elsasser: Oh, yes. I got to know a great deal of Rossby.

Kiehl: Did you feel that he had an appreciation for the role of radiation at that time? Was he interested in what radiation would do to his ideas on circulation, or was he more interested in the dynamics of the atmosphere?

Elsasser: I would say that he was more interested in the dynamics, than thinking as much as a physicist. But his work, of course, was on the thermodynamics of the atmosphere. _____ somewhat heavy in abstract thermodynamics down to what anybody could understand.

Kiehl: So you had numerous discussions.

Elsasser: I saw him quite often, yes. He caused me a lot of problems; he was rather a difficult person.

Kiehl: In what way?

Elsasser: Well, he had a job lacking (?) for politics. Everything he made, he made politics. His colleagues at MIT weren't very fond of that. Let me come back to that later when I tell the story. Rossby was involved in that--

Kiehl: Do you remember anything significant about the classes that you had, that you taught at MIT? Any students that later--

Elsasser: I didn't teach any classes.

Kiehl: Oh, you didn't teach.

Elsasser: Oh, no. I was not a member of the faculty.

Kiehl: I thought you were visiting there in the summer and you offered--

Elsasser: Oh, at MIT, yes.

Kiehl: Yes, at MIT.

Elsasser: Yes, at MIT I think I gave some lectures. I don't remember any--

Kiehl: Were those on radiation?

Elsasser: I presume so, yes.

Kiehl: How did you first meet Charles Brooks, who was then head of the Blue Hill Observatory?

Elsasser: That involves Rossby, that's a complicated story. It caused me a lot of trouble. The war was approaching then. The feeling was in the air. And especially I had

every chance because there were lots of officer candidates [who] were in the meteorology course [ ]. And then early in--when was Pearl Harbor? It was 1941--

Kiehl: Was it '42? Maybe it was '41.

Elsasser: Mr. Rossby had become assistant chief of the Weather Bureau. He wanted to be chief, but I think the Navy and the Army thought that was not a good idea. They put Mr. Reichelderfer in. You might know that man. He was assistant chief and in charge of research. He went to pieces. It was too much for him. He thought himself the god of meteorology. One day, this is a very significant story, one day in early 1941, I was in Washington on some kind of business and I ran into Mr. Rossby and he invited me to dinner at the Cosmos Club and wined me and dined me in a very extravagant fashion. I thought something would come out sooner or later. It came out. And after awhile, he told me that he, Rossby, was not the king of meteorology research. It was a big domain, but it was a little too big for him. He needed an assistant. Somebody who would have taken care of the West Coast. He had picked me out to be that assistant.

Kiehl: Were you surprised?

Elsasser: Yes, I didn't know quite what to do. I thought, it will straighten itself out if I just say nothing. Mr. Rossby will forget about it. I didn't commit myself. I told him I wasn't particularly interested in his political affairs. But that didn't impress him. So the next thing that happened is, I found out later, a couple of weeks later, that von Karman was in Washington and Rossby had a talk with him and Rossby told him [that] meteorology ought to be improved at Caltech. This man, Krick, who was there, was purely a commercial man, and they should throw him out and make Elsasser the head of the department. In my own naivete, I hadn't foreseen this. That was in early, it was the spring, I think, in May or June. I was prepared to go on a vacation and I thought, "Well, had better go to Millikan's office and see whether next year's contract's in order so I can get my monthly salary." Millikan's secretary said to me, "Well, you had better see Dr. Millikan." So I went in to see Millikan, and he was very peculiar. He said, "I can't tell you anything." He seemed very embarrassed. I can't tell you anything." You'd better see von Karman.

Kiehl: Von Karman was higher up than Millikan.

Elsasser: No, Millikan was higher up than von Karman. Millikan was the president of Caltech. Von Karman was one of the people in charge of [one of] the departments. There was von Karman, there was Pauling, there was somebody running the physics department. So I went to see von Karman. Von Karman

says, "Well, I hear you have been conspiring with Washington to get rid of my faculty." He was glad to get rid of me because he wanted to, he offered me several times--I should (?) go into this.

Kiehl: So he was upset, because he thought you were working with Rossby?

Elsasser: He knew better. He was an experienced man. He knew that it wasn't my invention. He used that as an occasion to get rid of me. I'm sure he didn't think for a moment that I was really trying to do something so clumsy as to go to Washington---

Kiehl: Why did he feel, why did he want to get rid of you?

Elsasser: Well, that was a very good time, because with the war approaching, he didn't have to worry about my getting a job.

Kiehl: Oh, I see. So there was a financial--

Elsasser: No, no, there was this constant conflict that had been there. I don't know. Nobody told me about it, but I could feel it. It was a constant conflict between Millikan and von Karman. Millikan wanted to build up, he was always a great builder. He wanted to build up geophysics. They now have, it is the biggest in the world, [the] geophysics department, and von Karman said, he always needed money because Millikan didn't give him enough money. He was testing airplanes and having consulting fees, and _____. So he was very glad that he simply, after this Rossby incident, had happened, he simply had told Millikan, "I'm going to fire Elsasser now." And that was that.

Kiehl: He just took that opportunity.

Elsasser: He took that opportunity. He told me, in fact, I'm fired. But the end of the contract was the first of July or something like that. So I found myself, I had very little money, I had a wife, _____ and then I found myself all of a sudden, sitting there with no prospect of employment. I decided it was not very difficult because the war was in the offing and we would have to go east to find something. So I wrote a letter to Brooks.

Kiehl: Now why? Did someone recommend that you write to Brooks or did you know--

Elsasser: No. He was known reasonably well, I didn't know him personally. I should have told him I was in a desperate situation.

Kiehl: So that's when you went to--

Elsasser: So he gave me a little, tiny little bit of money. That's when I went to Blue Hill.

In the meantime, I had met a man for whom I had done some work up at Caltech. Colonel Maier. No, at that time he was a captain. When I first met him, he told me and a couple of other people, that when the war should break out..."I'll need you fellows, because we have to do some war research. You will hear from me," he said. He was true to his word; all of a sudden, when I was in Boston in the early days, I think it was February or March, maybe March or April, it was in 1942, I got a telegram from him. Signal Corps Laboratory, asking me to report for duty immediately.

Kiehl: Now, at this time you were working on you monograph on [infrared] atmospheric radiation.

Elsasser: Yes, right.

Kiehl: Was that Brooks' idea that you write a monograph?

Elsasser: No, that was my idea. He just gave me a little money.

Kiehl: So you could do anything you wanted.

Elsasser: Yes. He was a very nice fellow. Not a research genius, but he had a number of people he paid a pittance to. And he was a good climatologist, by the way. His specialty was climatology. He told me, I always remember Brooks when I quote him by saying [that]--"the climate ?velocity gradient along the East Coast of the United States is the biggest in the world." Which is quite true. If you go from Boston to New York and come back to Washington, you see how much the climate changes. It's [like?] nothing anywhere else. So he left me alone to do this work. He was very pleased that he got this little monograph. And then I disappeared suddenly within a few days--

End of Side 1

Interview with Walter Elsasser
Tape 1, Side 2

Kiehl: So this is when you went to, was it New Jersey?

Elsasser: Yes. On the New Jersey coast, near Asbury Park. I don't know whether you know this.

Kiehl: Yes, I grew up in Pennsylvania so I know the area.

Elsasser: I stayed there for, it seems to me, _____ for two years. Eventually they

transferred me to some electronics job. They thought I could be more useful in electronics. That was the end of meteorology, and I and thereafter I----

Kiehl: You eventually moved on to the University of Utah, under the recommendation of Robert Oppenheimer, is that not true?

Elsasser: He had nothing to do with it.

Kiehl: Once you went to the University of Utah, it appears you became interested in atmospheric radiation again.

Elsasser: No.

Kiehl: No? Weren't you working on atmospheric radiation at Utah?

Elsasser: I did some work in it--

Kiehl: There was a monograph you published with Gene King.

Elsasser: Right. He's still active in the Air Force. Yes, I did a little work, but it was just mostly routinely getting the stuff into better shape. There was no _____ or research involved. I felt this was still hanging there and I hadn't cleaned it out. That was all.

Kiehl: Did you, have you met John von Neumann?

Elsasser: Oh, yes.

Kiehl: Did you discuss any of his numerical weather prediction work?

Elsasser: No, I never did. I talked with him about anterior terrestrial magnetism. He didn't know anything about that.

Kiehl: Yes. The dynamo?

Elsasser: Dynamo, yes. And he was very interested and very helpful. We had quite a number of discussions. But we had our differences. He was a big man, he had his time well-organized and divided. I remember once or twice I tried to chat with him about other things. And he didn't like that.

Kiehl: So he was very one-track?

Elsasser: I was the man he talked with. No, no he was not a one-track, I was the man he

talked with about fluid magnetism. There were other people---

Kiehl: With other people, he talked about other things.

Elsasser: ---about his weather predictions. There was this peculiar cooperation between John von Neumann and Mr. Zworykin, who was you know the inventor of television.

Kiehl: No, no.

Elsasser: ---who was at RCA Laboratories. They concocted this thing, so he was interested in the computer aspects, and he tried to make money out of computers. He was an engineer, a very prominent engineer. Von Neumann was there between them---- [he] was also interested in computers ?for physics, but then they decided I had to have an objective to apply this. They decided _____. I had never had anything to do with that.

Kiehl: I just wondered if he talked to you as far as the role that radiation could play in numerical weather prediction problems that he was working on.

Elsasser: No, no, he never talked to me about his weather predictions.

Kiehl: In 1961, the AMS published your atmospheric radiation tables as a monograph.

Elsasser: Right.

Kiehl: Those tables were used up until the early 1970s. What's happened, of course, as you can imagine, the original idea with the tables was you wouldn't have to perform many calculations. The way computers now are large enough and fast enough that we can do those calculations over and over and over again.

Elsasser: Sure.

Kiehl: So we actually solved the radiative transfer equation millions of times. Whose idea was it to come up with the tables and publish them?

Elsasser: It must have been my idea. I don't think that anybody suggested it.

Kiehl: You used a computer to do those calculations, didn't you? No?

Elsasser: I had a computer and I used it but I managed to get a small one. I used it to learn what computers were all about. It wasn't good enough to use it for that purpose. It broke down. It was much more efficient to do it alone. That was the early

stages of computers--

Kiehl: I have a final question which is a very general question. From reading your autobiography, you have certainly had an extensive career. You had the chance to meet some of the greatest scientists in the twentieth century.

Elsasser: That's why I wrote that thing.

Kiehl: Is there any one person who stands out in particular as a scientist who had the greatest depth of knowledge, greatest understanding of different fields? Is there anyone in particular that you think of when you think of--

Elsasser: Well, I would think I still have the greatest admiration for James Franck. Does that name mean anything to you?

Kiehl: Say it.

Elsasser: James Franck.

Kiehl: Yes, yes.

Elsasser: I started out with him. I gave up and became a theoretician. Millikan was a very remarkable man, very remarkable.

Kiehl: You did meet Oppenheimer, did you not?

Elsasser: Oh, yes. In fact, he more or less supervised my PhD thesis. He was a fellow student; we got our degrees just about the same time. A couple of weeks or so. Of course, he knew twenty times more than I did. The man we worked with was Max Born. He was rather something of a stuffed shirt.

Kiehl: Born was?

Elsasser: Yes, I mean at least he taught me. He might have not been quite a genius like Oppenheimer, but it was always an effort to get an appointment, you see. You would have to go to his house and wait until he was ready. You would go in, study, and walk out again. It was very formal. I could talk with Oppenheimer informally. If I had any trouble I could go to him and he would say, "Oh, yes." He would look that up in page 503 in Whittaker and Watson.

Kiehl: So he had a tremendous memory.

Elsasser: Oh yes, tremendous memory.

Kiehl: What differences did you see in the educational systems. You did your graduate work in Germany. Then you came here. You visited a number of universities. Did you perceive a tremendous difference in the educational systems?

Elsasser: Oh yes, the education system is quite different. The German system is very bad. It's mostly for geniuses, graduate geniuses, except in fields where mass production is necessary, such as chemistry or engineering. You have additional sets of requirements, examinations that are fixed. At that time physics was not yet a recognized field. There was just a professor of physics, or maybe two professors of physics. You could take your classes or leave them and nobody bothered.

Kiehl: What are your interests now?

Elsasser: I'm _____ a little philosophy. That's about all I'm still able to do. It gets pretty hard. After 75 years I could still follow some of science, but now it's very hard.

Kiehl: Is that when you wrote your autobiography, or was it before--?

Elsasser: Yes. It was somewhat earlier. Do I have a copy here with me?

Kiehl: You've made the radical transformation from the physical sciences to the life sciences at some point in your career, did you not? Into biology?

Elsasser: Well, I didn't make a radical transformation. It was just a sideline. I'm still ?playing with the computer--? Sometimes it seems a little difficult. It's a matter of epistemology. Yes, that's what it is, my main interest is not the life sciences as such. I mean I see them and I see them several times a week on my t.v. I see the birds and the bees. But I'm really interested in the epistemological. I was always interested in that. I always thought of terrestrial magnetism as epistemological _____.

Kiehl: In what way?

Elsasser: Well, you know that Einstein said there must be some extra _____. In electrodynamics, that makes a rotating body produce a magnetic field. Those of us who worked on the dynamo theory showed us that that's just not so. It's a matter of classical electrodynamics, magneto hydrodynamics. I think that's very important. Some of the extra _____ does not exist.

Kiehl: You were at the Advanced Institute, weren't you for awhile?

Elsasser: No, I wasn't.

Kiehl: Did you ever meet Einstein?

Elsasser: Yes, I met Einstein, and I was ?sorry. I had a good time. There was a man at Princeton University by the name of Ladenberg, a physicist.

Kiehl: Is this of the Ladenberg-_____?

Elsasser: Yes, that's the same man. He insisted that I ought to see Einstein if I wanted to---- ---- I said, "This is useless. Why should I?" He finally insisted that I do it. And I did. I made an appointment and [we] talked for about an hour. Einstein was very nice, very polite, [and he] had a very concrete way of talking, like an engineer. You'd never think that [he was] a theoretical physicist. Then I left him [again]. He didn't judge me, didn't make any judgement about my comparison of ideas to mine. A couple of weeks later, I asked Mr. Laudenberg, when I saw him next time, "Did Einstein say anything about my ideas about terrestrial magnetism?" "Yes," he said. "He doesn't believe it because this is a complicated theory and terrestrial magnetism is a simple phenomenon." _____ read the papers, looked at the data, you know. There is nothing simple about this, higher terms and everything.

Well, I guess we covered it pretty much.

Kiehl: Yes.

Elsasser: I don't think it was my, I don't have any feelings that this was my most successful activity. This period. But I did learn one thing. I learned enough meteorology to be ?happy. Quite a feeling for the Coriolis force. Several people have told me that without this it wouldn't have been possible to develop the theory of terrestrial magnetism. And I think this correct. During the war, I was drawn into electronics _____ and meteorology.

Kiehl: So the ideas you gained from those fields helped you later?

Elsasser: Oh, yes, they helped _____. I will show on a map.

Elsasser: Does this look like a simple phenomenon?

Kiehl: No.

Kiehl: It looks somewhat like a weather map.

Elsasser: It does. Yes, sure. That's exactly the point I'm trying to make. It is a weather map. _____

Kiehl: O.K., I think that's [it.]

END OF SIDE 2