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Interview of Eric B. Kraus
October 28, 1987, November 6, 1987

Interviewer: Chester Newton

Newton: This is an interview with Professor Eric Bradshaw Kraus. The date is 28 October 1987. We are interviewing at NCAR, and I am Chester Newton.

Eric, I would like to first ask you some questions about your youth and formative years. I know you were born at Liberec, Czechoslovakia, in 1912, but I don't know anything else about your family. Who were your parents, and what did they do, especially around the time of your high school, or gymnasium, years in the latter 1920's?

Kraus: My mother died when I was very young, so she didn't survive long enough to see me in high school. My father was a businessman in Liberec.

Newton: What business?

Kraus: Textiles and textile machines. He was a manufacturer.

Newton: What kind of education did your parents have?

Kraus: Well, it was not usual at that time for many people to go to college unless you went into the professions. Neither of them, I believe, ever was in college, and my father probably learned his job in his father's business. My mother was a pianist, but I'm sure she didn't go to college, either.

Newton: Did you have brothers and sisters, in Czechoslovakia?

Kraus: Yes, I had a sister and she remained in Czechoslovakia, and she was in a concentration camp as a political prisoner during the war. Her husband was a Communist. After the war, she came to America.

Newton: Your middle name, Bradshaw, is probably not an average Czech name. Would you care to comment on that?

Kraus: Well, I acquired it during the war. I served in the Royal Air Force, and it so happened that there was another Czech officer, of Czech origin, with the same initials as me, "E. Kraus," in

the Air Force. He was a dentist, I believe. I never met him. But his paychecks got mixed up with mine, and at some stage I was charged with his debts. So I thought the time had come to acquire a distinctive mark from him, and I adopted my wife's family name as my middle initial.

Newton: Were there any secondary school teachers or perhaps other people who had a particularly strong influence on you in science or in other fields?

Kraus: Yes, obviously some people had. I had a geography teacher who was, in his way, in high school a rather remarkable man. He was very knowledgeable. He was both a history and geography teacher, and he gave me quite a strong grounding in history. He was _____ and you had to know lots of dates, but it's useful to give you a framework.

However, I was much influenced by a close friend of mine who was several years older than me, Hans Sigmund, who was probably one of the best skiers of his time. He didn't compete very often, pioneering new techniques. During the war, as a Sudeten German, he was recruited in the German Army and he helped to feed some Russian Jews quite legally. As a result, he was condemned to serve in the punishment battalion, which somehow he survived. But he was a remarkable character, rather a leader of men. He had an interesting death, a good death: he collapsed on his skis in the 1950's.

Newton: A fine way to go.

Kraus: A fine way to go, Chester.

Newton: Did you expect from an early age to go to college, and would you tell me something about your schooling?

Kraus: Well, I went to school in northern Bohemia, in Liberec; Reichenderf was the German name for the place. After my mother died, my father re-married and I was sent to boarding school in Switzerland. I went to high school in Switzerland for several years. After graduation, I was sent to business school in Vienna.

Newton: At what point did you decide to major in meteorology? How did that happen?

Kraus: Originally, I was supposed to have a business career. I went to business school, then I had a few years in my father's firm, but I was rather bored by business and I was a pilot and did a little mountaineering and was interested in meteorology. And when I was 24 years old, I decided to go back to university. My decision to do so actually came during a business journey through the Middle East, when I spent some time with the Bedouins, and also I spent some time with an American expedition, an expedition by the University of Chicago in Persia. The expedition pilot took me out on the morning flight and showed me the outline of an ancient city; in the morning light the rays of the sun coming practically horizontally--you couldn't see it later. I thought, that's the sort of thing I like to do. I decided to chuck business, not necessarily to go into meteorology.

Newton: Not necessarily to go into meteorology.

Kraus: No. I drifted into meteorology.

Newton: But it played a very important part--

Kraus: In my decision, to go back to university.

Newton: What sort of life, when you did undertake meteorology, what sort of life did you expect to lead? Was it doing research that attracted you, or being a teacher, or forecaster or what?

Kraus: I really was pretty innocent. I really didn't know. I knew that the subject interested me and I really came to it out of general interest in the subject. Presumably, I wanted to go to university, but in a small country like Czechoslovakia, you have to wait until the incumbent dies in order to get a university job. There aren't many jobs like that, and I had no very clear ideas at that stage where I would be going.

I had some idea of becoming an explorer, actually, and I started to study it. I went back to the university and I had some contact with people. I had lived with a Bedouin tribe, and knew a well-known Czech archaeologist or rather anthropologist called Mousient. He told me it would be important for me to know something about geography and meteorology, and that's how I drifted into the subject.

Newton: What was your family's attitude towards your choice of career?

Kraus: Rather hostile, and they were less than enthusiastic. My father didn't oppose it, he told me, "You can do what you want to do." But he didn't think much of academic life. I remember his warning: "Eric, it's worse than the ballet."

Newton: Were there any of your undergraduate teachers or any course or any books that made a particularly strong impression on you?

Kraus: When I started to study meteorology, I knew very little of physical sciences. I got some credit for my years in business school, and I retired to my mountain hut and I tried to read some rather fundamental books. I remember I was very much impressed by Plant's **Thermodynamics**. Prague at that stage had two universities, a Czech one and a German one. You could register in both of them simultaneously, which I did.

My teacher, the chairman of the department of the German university, was a chap called _____, and who was a statistician, and he was the first man to use a punch-card machine, Hollerith machines, to process meteorological data. He later migrated to Ireland, to the Institute of Advanced Studies in Dublin, and he was the teacher of several well-known meteorologists there, including Ray Bates. At the Czech university, the chairman was Hans Lic, and one of his assistants, with whom I had a fairly close relationship, was Denis _____, who after the war became a professor in America.

Newton: At UCLA.

Kraus: Yes.

Newton: Can you tell me a bit more about your undergraduate years?

Kraus: No. At that stage, I really worked mainly for myself, and by myself, maybe because I was under no obligation to listen to lectures. I'm not a very good listener. I can work more easily from books. In the summer of 1937, I went to England to do some soaring in Dunstable, and Hans Lic, with rather good connections there, gave me introductions to among others, Professor Lindemann at Oxford. Lindemann became—was already then Winston Churchill's main scientific advisor. He was very influential during the war, and he became Lord _____. I also saw Gold, who was the deputy director of the Meteorological Office and Dobson, who was probably the leading expert on ozone studies, including--he developed the first instruments to observe ozone. I remember visiting Dobson(?) in his laboratory, which was on a farm. There was the man himself outside Oxford, his window wide open and couch, sticking his neck through the window where we were talking. I also met with Sydney Chapman at this occasion for the first time.

Newton: You mentioned in an earlier lecture that was also recorded, that there was a time in England when there were "gentleman scientists."

Kraus: Well, there were not all of them, but there were quite a few people who did science at their own expense, as gentlemen, amateurs if you like. But there was a tradition in England for doing this. Dobson was one of them. (inaudible) another one.

Newton: Ebulia Dines(?) was another who made radio sound maintenance(?) from his pond.

Kraus: Yes.

Newton: Can you tell us about your graduate studies?

Kraus: Well, there's no bachelor's degree. You pass an examination called a state examination in Central European universities, but we had no formal bachelor's degree, so there are no sharp distinctions between graduate and undergraduate studies. Anyhow, Zackera(?) who had been a post-doctoral assistant in Norway for two years arranged for me to go for graduate studies to Norway as an assistant to Jac Bjerknes. And I went there for the first time during the summer of 1938, and I went. Went a couple of times to Czechoslovakia during the Munich crisis, and just a few weeks before the rest of Czechoslovakia was occupied by the Germans, but after that happened I just didn't go back anymore.

Newton: During your time in Bergen, who were the historical figures in meteorology there?

Kraus: Well, Bergen was past its real glory days, but it still was a very interesting place. I was Bjerknes's assistant, but I met with his father. Actually, I went skiing with Vilhelm Bjerknes, who was then well over eighty years old. And I spent a fair bit of time with Sverre Petterssen, became a professor in Chicago. There was a whole series of visitors who came regularly there, including Palmén, Rossby, Wexler, Bergeron. So it was an interesting and exciting place for me. While I was in Bergen, I was of course still registered as a student in Prague. You could work it that way at that time.

Newton: So what you did in research there counted towards your Ph.D.

Kraus: Yes.

Newton: Which you received at a much later date, because of the war.

Kraus: Yes.

Newton: Could you tell me something about the characteristics of the figures that you've met at Bergen and perhaps a bit about their personalities and attitudes?

Kraus: Well, I can try. Bjerknes, of course, was my main contact. Bjerknes was a rather shy man. He had to give one lecture per week, for one hour, and he was impossible to talk with the whole day before. And he had a remarkably good physical intuition. He was probably not a first-class mathematician, but his physical insight was very good, and I wasn't the only one who commented on that. I spoke about our common experience with Jac Bjerknes. I spoke about it with Jules Chumley(?) just before he died, and he felt very strongly that he owed a lot to Bjerknes's grasp of the physical aspects of the subject. And Bjerknes, like his father and grandfather, also had a very strong aesthetic sense, and he loved to draw maps by hand, and he felt that if it looked good then it was much more likely to be correct than otherwise. And somebody during my lecture, or after my lecture two days ago told me that Bergeron had the same sort of attitude, that Bergeron told Alex Richardson(?) that what he did on his machine must be wrong because it didn't look good. But other people, Rossby was a visitor there and Rossby was always exciting. Actually, Rossby at that stage offered me a job at MIT, invited me to come to MIT.

Newton: This was in what year?

Kraus: That was in 1939, probably. Spring of 1939. But I couldn't get a visa quickly enough for America and when war broke out I wanted to do something. It took so long so I did something else, and I think when Rossby invited me, he probably was more interested, piqued his imagination to have somebody as a pilot there who could fly, a little bit above meteorology rather than... He thought that would be a nice thing to have at MIT rather than -- he didn't invite me for my knowledge of the subject. It was pretty rudimentary at that stage.

Newton: Well, he had a practical motivation there since those were the days when airplane observations, or APODs, were the only kind of soundings that existed, except for kites I believe.

Kraus: And he and I spoke about that, spoke of the possibility of using a soaring plane to study cold fronts. I remember this discussion which I had with him. Patterson was there. Patterson was the director of the weather forecasting service, and as such he really didn't form part of the scientific (inaudible) group in Bergen. He was a bit on the sideline during that time.

Newton: So there was no strong communication between the forecasters and scientists?

Kraus: There was strong communication between the forecasters and the scientists, but some were more equal than others in this case.

Newton: And what did you do after the war broke out, World War Two?

Kraus: Well, Bjerknes actually left for American in the summer of 1939, so I was on my own. And I got very restless when the war broke out. I wanted to get in, and I nearly got involved with the Finns and their war against Russia, but I finally went to France to join the French Air Force. And I got to France, I left Norway just a few weeks by accident before the Germans occupied the country. I didn't expect that occupation. And I got in France just in time to participate in the rout of the French and the occupation of France. I (inaudible) three months in France and after three months I was ordered to some castle in southern France to await the arrival of the Germans.

Newton: This is after you had joined the French Air Force.

Kraus: Yes. And we retreated. I didn't do any flying. We retreated progressively. I had some contacts with the meteorologists then and I met a British meteorological liaison officer, Flight Lieutenant Paul Pass(?), and we retreated progressively further and further south, until we got to a castle not very far from Bordeaux and we were ordered to wait (inaudible) the Germans and then to surrender. And at that stage I decided I'd had enough of this show, and liberated a French Army truck and took off on my own towards the Mediterranean. I don't know whether that fits into your interview here, but I still remember it was a very eerie journey. I was all by myself in this big Army truck traveling through the summer countryside of southern France. I had three hand grenades placed on the dashboard in front of me and I was wondering what I would do if I ran into the German tanks around the next corner. Well, they were only about 200 miles away, but you didn't know that. You had no clue. I'd firmly expected them at any moment. Then I got shipped from Port Cote-sette(?) near the Spanish border, and went first to Gibraltar and then to England.

Newton: Well, after leaving the French in this manner, you went by ship to England, and there you joined the British Royal Air Force, in which you served until 1946 and achieved the rank of squadron leader and for a time, wing commander. I might mention that since you were so modest that for your service during the war you received the British Air Force Cross and the Czech Military Cross and Medal of Bravery. Would you tell me about your activities and experiences while you were in military service, starting with your -- well, you've already given us your motivation for joining.

Kraus: Well, when I got to England, I had no papers or nothing, and so I got in contact with Gold, who I had met in 1937, and he sent Paul Pass(?), whom I had met in France and had mentioned earlier, to identify me. And soon afterwards, I had the possibility either to join the Czech army as a private, or to get a commission in the Royal Air Force as a pilot officer and to become a meteorologist there. I wanted to be a pilot. I wanted to fly. But the Battle of Britain had already started. There was no time to train anybody at that stage, and I was known to -- Gold knew that I had been a meteorologist, and he was very short of people. But I had been trained, so that was my first job. I was first then to Dunstable, to learn the rudiments of the trade, and then I was posted to York to a bomber group, where Richard Variott was the chief meteorological officer. It was pretty obvious that one of the things that was lacking to us to make any forecast was the absence of any data from the oceans, or from Europe for that matter. But we had nothing over the oceans. Actually, we got some data from Europe because the English had broken the German code, but we had nothing from the sea.

Newton: Of course, there was radio silence at that time, so you couldn't receive ship observations.

Kraus: There were not that many ships around. Just convoy, (inaudible) data (inaudible). So I suggested that we should install meteorological reconnaissance flight, and I wrote a letter about that together with Variott, which had the support of our commanding officer, and somehow the idea seems to have been there. They very quickly accepted, to my surprise, and I was ordered on to London and I was asked what about it, would you like to start these things? I said yes. But I want to be a pilot in order to do that. No, there is no time, we can't do that, but we send you to a navigation course if you like. I passed, with some difficulty. I failed in meteorology!

Newton: And why was that?

Kraus: There was one question which I complained about, which was later ordered -- it was rather silly. A question, I remember, said "What would be the state of the atmosphere if you had a lapse rate of 10 degrees over 100 meters?" And I said more or less in my answer, "Don't be silly, the atmosphere doesn't behave that way!" And they (inaudible). (laughter) It was 100 feet, and this was not the right answer to give. I should have said it would be very unstable.

Newton: In spite of that disability, you were able to...

Kraus: To become the...

Newton: To become, to go on at this time and become distinguished in the field of boundary-layer meteorology later on...

Kraus: There were some difficulties, but I was finally told don't be a bloody nuisance and (inaudible) and give us peace. That was more or less the end of it. (laughter)

Newton: So that made you a pilot officer?

Kraus: Pilot Officer in the Royal Air Force was a rank, which corresponded to Second Lieutenant. It had nothing to do with being a pilot. I also became a pilot, but that was not the same thing. You could be a pilot officer without being a pilot, without flying.

Newton: Now, will you tell us something about the Med flights, about your experiences and what they accomplished? (inaudible)

Kraus: First of all, it was probably that my earlier connection had played some role in my getting this, because when I was summoned on to London, I had lunch with Chapman, who had nothing to do with my being in London for the Met flights, but at the place where we had lunch, I believe it was the Royal Society, we run into Lindeman again, Lindeman Somerhall(?) had seen or heard about my letter and told me it was a good idea and he was behind it. And he was very influential at that stage. And we had to do this thing from the very scratch. We had to get instrumentation, or I had to get instrumentation, and we had to develop a code, and plan of flights. What we did, what I suggested we do is fly out at the low level to as far as we could go, then make an ascent to 500 millibars and (inaudible) millibar and come back on the glide pass to get the maximum distance. I had a bitter argument with Gold about what code to use. Gold was one of the most brilliant men I knew. He was very clever, he was very good. He had a very sharp tongue, but as a result he didn't -- to be clear, he was a deputy director of the British Meteorological Service, but he never got right to the top of the civil service because of his very sharp tongue. But during the First World War, he became known by making false weather charts. The Germans had broken the English code, and the English had known that, and Gold fabricated false weather charts for the benefit of the Germans, which had all the wrong information, the wrong data. But he was a very bright man. After the preliminaries, I was posted to Northern Ireland, where I found myself the proud possessor of three Blenheims which were not very powerful aircraft. I had no other pilots, and no air (inaudible) whatsoever. It was just after the Battle of Britain. I had ground crews, so in order to get these Met flights going, I came to an arrangement. We made an arrangement with another squadron's commander at my station. He said I'm going to lend you pilots if you do some convoy navigation with our people, because I'm short on navigators. We did that, and that's how I first met (inaudible). Soon afterwards, this network of Met flights was extended, started off in Northern Ireland, flying northwest over the Atlantic. We then established on the second one in East Anglia, flying out over the North Sea, and later a third one from Cornwall flying over the Bay of Biscay. The last one which I was involved with was one from Iceland, going towards (inaudible), general direction, but that came much later.

Newton: At that point, the flights were still in the Blenheims?

Kraus: At that stage, the flights were all in the Blenheims, which barely managed to get up to 500 millibar, which is the ceiling we are supposed to get during our ascent. It was really a struggle to get up there, and there was some problem with some of my pilots, because most of them were retreaded fighter pilots who were a bit bored by this Met flying business, and one of them became the flight commander of the flight over North Sea, (inaudible), run into three Junkers-88(?) who together must have had ten times the weight

of armament which a Blenheim had, and he turned around and stalked one of them and shot one of them down. He got a D.F.C. for that. It was amusing, interesting time. I made my home headquarters in Sandeval(?) and I was in charge of the three flights and at the same time a commander to fly in Sandeval itself. So most of my flights were over the (inaudible) west.

Newton: And how far out did you fly? I'm trying to get a notion of how big an area you could draw an upper air turret over as a result of the reconnaissance flights going on in three directions?

Kraus: (inaudible) was 300 miles or 500 miles. The flight from Northern Ireland, went regularly as far as Rockle, Rockle Island(?). It was an adjacent experiment, around Rockle, so I could reconstruct how far that was. But that was our end point. I believe it was 500 miles.

Newton: So except for a lot of extrapolation, you had direct information on the weather for the next few hours, if you marched it along.

Kraus: Yes, yes. We made observations roughly every 50 miles or 100 miles -- 50 miles, I believe -- during our inward and outward legs. And then we did data from the SF(?).

Newton: Well, you flew under terrible weather conditions. You must at some time have had some scary adventures.

Kraus: Well, we are supposed to fly under all weather conditions. But we also liked our comfort, and we liked to sleep in our own beds. I certainly did. In Sandeval there was a rule that no aircraft was to land in Sandeval if the ceiling was less than 400 feet. But I had an arrangement with the local Met offices that when the Met flight came back, the ceiling had to be 400 feet, whatever goes, and we would make up our own mind because we didn't like to be diverted to a different (inaudible). Anyhow, one day I came home myself, and so I only had myself to blame, and had broke clouds over something I believed to be the English Channel, and the first thing which I saw was a hill in front of me. It was Lizzon(?), which is a cape in Cornwall. And I tried with my Blenheim to climb up this hill. I was just over the surface of the turf. Actually, the propellers churned the grass and knocked over a fence. I nearly killed a man who just got away, and finally the hill became less steep and the Blenheim got away. But when we landed, there was lots of grass. Not grass, (inaudible), and in the leading edge of the aircraft, and the lights in front had been smashed by something.

Newton: Close enough!

Kraus: It was close enough. Another interesting thing, I actually never saw it myself. The Germans, by that time, also had a Met flight. And I was told that on one or two occasions, they made their ascents within sight of each other.

Newton: (inaudible) sounds embarrassing.

Kraus: (inaudible) are embarrassing. In any case, the meteorologists, they had the same men in both charter, both flying and meteorology. The meteorologists, the meteorologist in charge, was Schwerfager(?), and a long time after the war, Schwerfager, who became a professor in Wisconsin, this country, later in his career, he came to visit us in Australia, and he and I fought the war all over again on that occasion. (laughter)

Newton: We've heard that the capacities of the airplanes were very limited, certainly compared to what we have now. Can you tell us something about the instrumentation and the various ways of making measurements?

Kraus: Well, we just took instruments which were available on the ground around the ship, and played around with putting them in various places. We had a sagrometer(?), essentially a wet and dry thermometer which was stuck in front of the aircraft, an obvious place to be. We used pitometer(?) for the pressure and we measured wind by drift measurements at the bomb site, flying at an angle in two different directions. And we also collected and kept the visual record of the weather.

Newton: Clouds and precipitation?

Kraus: Clouds and precipitation, yes.

Newton: During the time in which you were in the RAF, you met your wife Heather. I wonder if you can tell me something of the circumstances of this meeting and tell me something about Heather herself. Did she also have a career? What was she doing at that time? A little bit about her background, and so on.

Kraus: Well, let me tell you first how I met her. I went to London and I had several interesting visits to London when I was in the Air Force during these years. I went to London and I remember New Year's Eve of 1940, and I had a girlfriend then who was watching. She was one of the RAF bomb watchers, (inaudible) watchers, and (inaudible) so I went to seek her company. It was a big German raid. When I came in at four o'clock, she sent me home because the raid was over and I was going to go home. So reluctantly I did go home, through the really (inaudible) streets of London, and I came to my hotel, or club where I was staying, and it was no longer there. It was all gone. Nothing left. I had no proper clothes or nothing. I went to Andrews the next day and said what will I do now? There was a whole collection of other officers in similar circumstances there, and at that stage there was not much red tape. They each gave us forty pounds, which was a lot of money at that time, and said "Go and get yourself a new uniform." We did. And I came the next time to London a few weeks or months later and thought I'd be much more careful, and I came there with nothing but a toothbrush. Toothbrush was the only item of luggage which I had, because I didn't want to lose everything again. And I was in London for one day. I got a telegram, a signal from the adjutant of my station that said, "Your room destroyed. No sandwich." He just couldn't wait. Anyhow, you asked me about Heather. On yet another visit to London, I run into a Canadian acquaintance of mine, with whom I had flown on the convoy patrols which I had mentioned to you earlier. And he told me, well, I have a date with a girl, and she brings a friend along.

Why don't you come (inaudible), meet in a pub. Why don't you come out with me and meet her? And I said OK, and he had to go back to his station so I was left with these two girls alone. And at the end of the evening, they tossed a coin who of the two I should accompany home. And the one who won the coin was Heather. And well, not very long afterwards we married. We didn't know each other very well when we married, but we'd been living together ever since. What she was doing then, she had actually (inaudible) then to be a dancer from Australia. She was a native Australian but her family didn't approve of that either at that time, and she changed to be a secretary, and through a sequence of circumstances just before I married her, she had become one of Winston Churchill's six secretaries. He had six secretaries there who were in attendance for 24 hours. He was a night worker, spent a lot of time working at night. But she was very secretive about her job. Didn't tell me much about it. (laughter)

Newton: I'd never heard her mention it to me. Well, of course, I know Heather but perhaps you can tell me something about her later avocations, things that she liked to do and so on?

Kraus: She had came to England as a dancer and I think that was her first passion, and I think when the war was over she was rather sorry that she didn't become a professional dancer, and I think that's still her first and strongest love, dancing. Because of medical reasons, and because she just got (inaudible) this time, she couldn't do it professionally. She was a dancing teacher in Austra... (break in tape)

END OF TAPE 1, SIDE A

Newton: This is side two of tape one of the interview with Eric Kraus. The date is 28 October, 1987. (inaudible) At the end of the other side, you had told me that dancing was Heather's great love. Could you tell me something else about her avocations, and things she liked to do?

Kraus: Well, I still think she likes dancing better than anything else, but she was trained actually also as a designer and she became a painter. She's not a professional painter.

Newton: What sort of family did she come from? Were they in the city? Or in the outback? Or the (inaudible)?

Kraus: To some extent, both. They lived in the city and her family had been in Australia for a very long time. One of her ancestors was the first clergyman in Australia, which was a rather thankless task because people there weren't very religious. The first settlers in Australia weren't very religious.

Newton: But a great challenge.

Kraus: Yes. And incidentally, while we're talking about her family, another ancestor was a judge who condemned King Charles II to death, and one of the favorite stories of my children was that one of their ancestors dug him out of his grave and hang on the gallows after he was safely dead. But her family lived in Australia since the inception of

Australia, really, white Australia, and they were mostly lawyers and some politicians too. But they lived in Sydney but they usually had country properties at least before Heather's time. By Heather's time, all that money had disappeared and there was much left.

Newton: A pity.

Kraus: Yes.

Newton: To return to your RAF career, what happened to the Met flights?

Kraus: Well, I was with the Met labs in their early stages, but after a year or so the bureaucracy caught up with me. And it was a pretty freewheeling time and we started it and I had nobody to tell me what to do but that couldn't go on forever. After a year, wing commander coastal command was put in charge of the whole thing and on the meteorological side P. A. Shepard became a professor in (inaudible) College later and was in charge. I didn't like this development very much, so I was given my decoration and told, time to have a -- Kraus, you had a long operational period, it's time for you to have a rest. And I was sent to northern England and then Stratgaart(?). But before that, before I could marry Heather, actually, I was told I have to go to Iceland to start the meteorological reconnaissance flight there. And I remember I was on a flight up to Iceland when I heard about Pearl Harbor and that America had just entered the war, which put a different face on the whole feel of the war as far as England were concerned.

Newton: How did that change their feelings, or affect their feelings?

Kraus: Well, it's difficult to say. Clearly, when England was fighting alone, things were pretty hopeless. They were not hopeless, but they were pretty dangerous. But there was a certain exhilaration about it, and the very fact drew people together and it was an interesting time in England during the Battle of Britain. After America came in the second half of the World War, it became no longer as hopeless. It was obvious that we probably would win and people also got more tired and they realized more somebody else doing more, and they having to do less perhaps, and the feel of the thing changed. It's difficult to describe in an interview briefly.

Newton: What did you do during the rest of the war, Eric?

Kraus: Well, after a period as an instructor in the north of England, there was Rockden(?) as an auxiliary -- Rockden for some bombing raids on Germany because there were not enough crews available otherwise. I was posted to photographic reconnaissance in southern England. The P.R.U., photographic reconnaissance unit, were the predecessors to the U2 flights in this country and they flew at a very high elevation and (inaudible) Mosquitoes.

Newton: How high was it?

Kraus: Well, high elevation for that period. We flew between thirty and forty thousand feet. Forty thousand feet was -- say between thirty and thirty-six or thirty-eight thousand feet. We tried to be -- we were stripped of all armaments. We relied entirely on speed and

when we saw something, we run as fast as we could. And most of the time in the P.R.U. flights had an advantage in speed over anything the Germans could pull up, and towards the end of the World War, the Germans had jet fighters and they could fly circles around us, and they only had a few of those, so they didn't expose them to heavily armed bombers. An unarmed photographic reconnaissance plane was a fair target. I was nominally in charge of meteorological services for the P.R.U. but I was a pilot and had an arrangement that I could go on occasional flights, which was good enough to keep me in recreational gas for my car, which was very important at that stage. If you want to have another experience during that time, it so happened that I had to force-land in Brindi(?) in Italy, after photographic reconnaissance flights, and we thought we'd have to surrender as prisoners of war because when we did Italy was of course on the other side, and we happened to land there on the day the Italians changed sides. So instead of becoming prisoners of war, my co-pilot and I became the liberators of Brindi. Rather interesting. I got once more into a meteorological reconnaissance flight right at the end of the war, and after the end of the European war another friend of mine and I ferret a Mosquito out to India, because we really had not much else to do in England. And while I was India, I was summoned down to Colombo in Sri Lanka, and I was asked to go to China to investigate the Chinese meteorological facilities and to consider the possibility of starting a Met flight out over the Pacific or over the South China Sea from unoccupied Chinese territory. There was some plane, a bomber command, taking part in the war against Japan and I was again in the aircraft between Kunming(?) and Chungking when I heard about the atom bomb being dropped on Japan. After that, my main concern was to get back to England as quick as possible. I didn't want to get stuck in India or in China.

Newton: But you did, is that right?

Kraus: I made my way back pretty fast, yes.

Newton: And then what did you do then?

Kraus: Well, I had to prepare to demobilize according to a point system, and gradually. And at that time, Zackera(?) came to England, a member of a Czech delegation to renew the Czech meteorological services, and he asked me to come back to Prague to get my Ph.D., and told me that they would recognize the work which I had done earlier for this purpose. I borrowed a Mosquito. I went to my CO and said can I borrow a Mosquito? The Czechs wanted to give me a Ph.D. I wasn't really that much interested in a Ph.D. at that time. There were hundreds of Mosquitoes on the ground at that stage. I flew a Mosquito to Prague, but I had quite a good Czech decoration. I had seen the president, had been one of I believe was the first pilot to fly over Prague, to photograph Prague during the war, the first Czech to do so. And I've got a Czech decoration and when I got to Prague I found out that I was the first Ph.D. which they'd handed out in six years, because university had been closed during the war. And there was just no way I could have failed.

Newton: They did give you an examination.

Kraus: They did give me a public examination, but whenever I stumbled, and I really knew very little, there was some professor there who said, "You mean so-and-so, Mr. Kraus, don't you?" (laughter) And yes, that sort of thing, of course.

Newton: And did you do that later on with your own students?

Kraus: (laughter)

Newton: Don't answer that!

Kraus: Anything that I did learn about the subject, I learned after I got my Ph.D., sometimes a long time after I got my Ph.D.

Newton: You next went as a senior research officer to the Commonwealth Scientific and Industrial Research Organization, or CSIRO. How did that come about?

Kraus: After the war, towards the end of the war, a number of different possibilities. The Czechs asked me to come back to Czechoslovakia and Zackera(?) asked me to come back to Czechoslovakia as part of the restaffing of the meteorological service there and I was fortunate that I could do that because anybody, any Czech who served in the Royal Air Force had a very rough time in Czechoslovakia after the Communists took over, and I had some friends who were shot, and disappeared afterwards. There was some possibility of staying in England.

Newton: Excuse me, did you know that at the time? (inaudible)

Kraus: Well, of course I didn't know this.

Newton: Well, I mean that there was a hazard.

Kraus: No, it didn't occur to me at that stage.

Newton: So that was not a factor in rejecting the position.

Kraus: That was not a factor in my decision, no. The main factor was that Heather didn't know a word of either German or Czech, and I had what seemed to me a more interesting offer from Australia. How did this come about? The operations had a fair amount of contact at P.R.U. with the operations research officer who was C. H. Woddington (inaudible), who was a rather distinguished biologist and through him I met a number of other English scientists, Blackett Burnell(?), the crystallographer, Appleton, and Tizzard(?), and somehow they arranged it for me to get this offer(?) to Australia. When I was offered a job at radio physics, a radio physics position of CSIRO, and before -- yes, I'm sorry?

Newton: Was that job attractive to you because there were certain research activities going on there?

Kraus: Well, the only other job which I'd had offered besides this Czech job and a faint possibility of staying in England at that time. But you have to understand that as a rather young man, with not much formal education, at a position of considerable authority during my wartime years, and anything afterwards was rather a comedown. When peace broke out, it rather hit me as a bit of a catastrophe.

Newton: Certainly, going from a squadron of airplanes to you don't know what.

Kraus: Yeah, exactly. But that was one of the offers which I had and it seemed the best one, particularly as Heather was Australian and had family connections there. She actually was not at all anxious to go back to Australia. She had escaped Australia and she wanted to stay in England if possible. Yes, I had another opportunity. Teddy Bullard(?), Professor Bullard, Dr. Bullard of Cambridge University who was a well-known geophysicist offered me to come to Cambridge as his assistant. I think it was wise, and a, I got paid much better which was of some interest to me, and as a research assistant in Cambridge, and in retrospect I think I wouldn't have been clever enough or well-enough educated enough to have done well in Cambridge at the time. My schooling was too disjointed to really make a name for myself in Cambridge at that stage.

Newton: Also that would've involved going into an entirely different field of solid earth geophysics.

Kraus: It would've been solid earth geophysics, and it would have required more high-powered mathematics than I was really trained in.

Newton: But still a wonderful opportunity.

Kraus: Missed opportunity. CSIRO and the people at CSIRO...

Newton: But that was a possible turning point in your life.

Kraus: That was a possible turning point.

Newton: You took a path that set you out on the rest of your course.

Kraus: The people of CSIRO arranged for me to travel to Australia in 1947, after I was demobilized, via the United States. And that was my first visit to this country. Would you like me to tell you a bit about my visit at that state?

Newton: Yes, I would.

Kraus: I think it was partly scheduled for Henry Horton(?) at MIT, and I remember I visited Halberts(?) in New York at NYU. I spent some time at MIT, where I met Ed Lawrence(?) for the first time, and we did a lot of (inaudible) learning together ever after. I was a house guest for a few days of Margaret Mead and her husband Gregory Bateson, who were friends of Woddington(?) whom I mentioned before. And if you want personalities, I came from England where people did(?) things wrong but they didn't talk

about it. And I remember how shocked I was when I came to this country and offered me a steak, a very nice steak for dinner, and burst at how she had got it on the black market. The English may have dealt on the black market, but they were very careful not to talk about it! But I remember this occasion rather well. Also, it was arranged for me to see Langmuir(?) in Schenectady, but he was away when I got there, and I had a long talk with Schafer(?), and Schafer showed me his experiments, actually in a (inaudible) freezer, an ice chest, where he had a supercooled cloth(?) which he precipitated by putting dry ice into it, and (inaudible) that played some role in what I did when I got to Australia.

Newton: The turned a small universe into glittering ice crystals.

Kraus: Yes. Other places I saw during that visit to the United States, and was University of Chicago, where I missed Rossby. He had left. He wasn't there when I got there. But the person who looked after me there was Joanne, Joanne Starr(?). I don't know whether she was already married or not. I can't remember. But I remember that she was (inaudible). Finally made our way by train to the West Coast, spent some time in Los Angeles and went on a troop ship.

Newton: Where you saw Bjerknes again?

Kraus: Bjerknes, actually, Bjerknes, I saw both Bjerknes and Patterson during the war in England. Bjerknes came to stay with us in England, when I was with the P.R.U. We had a little house off the base, and he stayed with us.

Newton: So after this visit to the United States, you mentioned that Schafer's experiments in the ice chest influenced your further career, and that went on to the part of your career spent at CSIRO. Would you...

Kraus: Well, what happened really, when I got to CSIRO, my chief was Taffy Bowen(?), radio physics, and his suggestion to me, or request to me, was to build a camera to photograph cloud dropping. He was interested in radar reflection, and he was rather excited at that stage by radar meteorology which was a very new thing at that stage. I was already a square peg in a round hole, because I really didn't have much experimental experience. My experience in Bergen had been the drawing up of charts, and what became my thesis was based on a synoptic analysis of depressions, which so-called 5B depressions which form over the Western Mediterranean and then cross Italy and move northwards over the Balkans and Russia. So I really didn't know much about that. Anyhow, after a few weeks, months there we formed a little sort of meteorological section there together within radio physics, together with Pat Squires. I read in the paper, some journal, about Schafer's latest experiment in dispersing fog. He had flown an aircraft over low-level stratus of fog and had thrown on some of his dry ice and had cut a cloud-free lane or swath through this fog. It was obvious, didn't need much imagination to put this together with Mr. Bergeron's theory and to say, well, if you can do that to fog you can at the same produce drops of precipitable size if you would drop the stuff into a supercooled cumulus cloud.

Newton: I believe you said earlier, I mean the other day, that you were not sure whether Schafer even knew about Bergeron's...

Kraus: He certainly didn't mention that to me. But when I visited Schafer (inaudible) to decrease his role in that and his idea of using carbon dioxide and so forth, it was all his idea. I doubt whether he was aware at that stage. He was not a meteorologist. He was a physicist, and (inaudible) Bergeron's theory and I think we probably put the two together in Australia, but it was the obvious sort of thing to do.

Newton: The object of their experiment in the first place, the one that you mentioned about the dispersing of clouds, was to do just that. Not to stimulate cloud growth, but to stimulate cloud dispersal.

Kraus: No, the object of their experiment was the dissipation of fog over arid crops. That's what they were supported for. They were supported to dissipate fog.

Newton: Which has in later years, I believe, been the greatest surefire thing in weather modification. Reno Airport and places like that.

Kraus: I didn't know that Reno gets that much fog! Well, in any case, we started to do some flying and do that, and it so happens that one of my first flights, Pat was on the ground, I was in the air, was spectacularly successful and the examples, the photos which we get at this stage are still shown by enthusiasts of this field. So at least (inaudible) told me before his death. What happened is that there was a field, a high level of cumulus clouds propped at about 22 or 23,000 feet over New South Wales, and we seeded two of these and these two that were seeded shot up to 40,000 feet. So it was really a quite unusual and spectacular sort of experiment. The trouble, which became pretty soon clear to me and to Pat I think was that it was a very unusual sort of thing. What must have happened was, we didn't know, that the lapse rate above the cloud cover was just unstable enough so that the little additional heat input which was due to the sublimation of the supercooling droplets at the top of the cloud was sufficient to allow this further development which is a pretty rare sort of combination of circumstances. And I (inaudible) was very much more optimistic about the future of rainmaking than I was. I was skeptical about the practical value of it.

Newton: I'd like to return to that later on, but first, before we leave the subject of CSIRO, I would like to ask you what sort of working conditions you had there, what sort of attitude there was towards pressures to do research. Was there a big-project or small-project atmosphere, or no project atmosphere at all?

Kraus: Well, the main thrust of radio physics was of course in the direction of radio physics and the deputy director of the show, Paulcy(?), was a very good scientist. I think on the whole condition of CSIRO, at that stage, was very good. The head of CSIRO, John Rivers(?) I believe, I can't remember his first name, was a scientist of very considerable vision. He was a friend of Appleton's, and that was partly the reason why I came there. The fact that Gold and I didn't hit it off, really doesn't affect the fact that CSIRO started off (inaudible) at that state one of the most, possibly one of the most liberal and advanced

research institutions in the world. The trouble with any professional research institution is, particularly small ones, that people stay together too long, that there's not enough, particularly in a small country like Australia, there's not very much of a throughput of people and that leads after some time inevitably to an ossification of the group, to some extent. There'd have to certainly be some division in Australia. Another problem in Australia was that at that stage politically it was run by a Labour government. Most of my time in Australia, Labour was the governing party, and they tended -- I don't know if it was for political or other reason -- to favor CSIRO over the university. The CSIRO was not strictly speaking a civil service, but it was really close to being civil service, and practically all research money in Australia was funded to CSIRO rather than to the university, which again, narrowed the focus possibly unduly.

Newton: So the universities and CSIRO were in effect put into competition with each other by the funding?

Kraus: They really weren't because there was very little research money at the universities compared to what -- the government mainly supported CSIRO for research, and the standard complaint of university people were that they had no opportunity for research, or relatively little opportunity for research. That didn't mean that there were not some rather distinguished researchers at the universities, but it was more (inaudible). Bullard(?), Bullard the seismologist, Bullard (inaudible) for my work at Sydney University at some stage. It was very well known work there. Other people, McFarland and others. But on the whole, the support of the government went to CSIRO and for some time to the Australian National University which was also directly funded by the federal government. But the state universities were not really funded in Australia.

Newton: I think I'm extrapolating from what you said, that there wasn't much of a conception at that time of an exchange between the university community and an organization like CSIRO. And you mentioned the crippling effects on science, of the ossification of the staff when there's no throughput.

Kraus: There was some. Some of the CSIRO division, they originally established on university campuses. Radio physics was one of them. Later on, things developed. That got a link away from Sydney University. But the fact that a research institute is on a university campus doesn't necessarily guarantee that the two sides will speak a lot with each other. I can't really be too specific about these things. There was clearly a feeling of some envy amongst the university community, comparing their own status with the greater research freedom, with privileges behind the CSIRO.

Newton: So you were free within a given subject to follow your own nose in doing research, were you?

Kraus: When I came to Australia, yes. I believe it has changed since then.

Newton: It's gotten (inaudible).

Kraus: When I came to Australia, that was Rivet(?) who was the head of CSIRO then. That was

certainly his philosophy. Later it became more institutionalized.

Newton: And grew with greater resources into greater projects?

Kraus: I wouldn't like to answer that, because I left CSIRO in 1949 after three years, so (inaudible) I was an outsider and I wouldn't really like to say. There are other people better qualified to say.

Newton: So the time when you were there was the time when CSIRO was very young.

Kraus: When CSIRO was young, when it was a liberal and enterprising organization, the fact that I have troubles there were my own fault rather than that of the organization.

Newton: You mentioned Pat Squires and some of the other people that you worked with.

Kraus: Jack Warner was there. He was more interested in the instrumental and technical aspect. Pat was my closest associate or collaborator, and we (inaudible) met this cloud physics experiment out of Richmond Aerodrome which is about fifty miles west of Sydney, and as a result we both bought farms in that area, lived on our farms. When I resigned from CSIRO after three years, I first tried my hand on a farm, and combined that with being a lecturer on the (inaudible) at Sydney University, a lecturer in fluid dynamics, and I had some rather good students there. Among my students was Owen Philips and Stuart Turner who both told me that their interest in fluid dynamics was at least encouraged by my lectures.

Newton: Oh, that's a remarkable score! So, then you went on and stayed at Sydney University for a period of three years. That had something to do with the disagreement as to policies of science between you and Taffy Bowen.

Kraus: Well, I was a part-time lecturer, adjunct (inaudible).

Newton: (inaudible)

Kraus: (inaudible) at Sydney University. And I resigned from CSIRO because of that quarrel with Bowen in 1949. I tried farming and lecturing for some time. I also wrote a book which was never published. And then I became an operations research officer for British Commonwealth's Pacific Airlines which was soon afterwards swallowed up by Qantas. They were the main airline which flew from Australia to America at that stage.

Newton: And how did that set with you?

Kraus: That was interesting. I mean, I liked operations research and I did to some extent similar work at (inaudible) authority. It can be quite stimulating to have a variety of different problems thrown at you. And I found my period with BCPA not uninteresting.

Newton: So your appreciation of that might have partially conditioned your going to your next position, which had a strong scientific side of it, but also a strong practical side?

Kraus: Well, it just was another job, really. At BCPA I was a very small cog in a relatively big machine, and I got a job offer to become the authority meteorologist for the Snowy Mountain Authority, which had a number of attractions. It made me a little bit more independent and I liked the mountains. The Snowy Mountains are the main snow and ski area of Australia, and it was a very interesting project, project of the same size as the Tennessee Valley Authority.

Newton: You were there during the -- you were the Authority meteorologist of the Snowy Mountains Authority from 1952 to '61. Could you tell us what the mission of the Authority was, and something about your role in it?

Kraus: Well, the mission of it was to build a network of dams and tunnels which diverted two streams from the eastern side of the mountains where they flew through very wild country down to the Pacific and into the interior of Australia to the Mary(?) River system where they could be used for irrigation. In doing that, the water was used to generate electricity. The scheme was supposed to (inaudible) of electricity. They worked closely with the Bureau of Reclamation. Quite a few engineers came here to be trained at the Bureau of Reclamation.

Newton: By the US?

Kraus: At the U.S. Bureau of Reclamation, but it was a big organization. The scheme was initially assumed to cost about a billion dollars. In early 1950, a billion dollars were more than it is now and it happened to be almost exactly the same what the TVA had cost ten years earlier. Again, you've got some rather interesting problems thrown at you. I remember working on the icing of transmission wires. I had to investigate the case of a man who was killed inside a tunnel by lightning and we found out what happened was he was a miner underground when he was killed by lightning. But the lightning had struck the rails which led into this tunnel, and the electricity just -- the charge was right along these metal rails. He was killed by straddling these rails.

Newton: You're not safe anywhere!

Kraus: Yes. They always wanted me to initiate a forecasting service and I spent quite a bit of time at Sydney University, back at Sydney University at their computing department trying to establish statistically based rainfall forecasting system. I used to first -- it was my first encounter with major computers, so SILLIAC, the Sydney version of the ENIAC, University of Illinois computer.

Newton: And how did you make out with that experience with that computer at that time? It was very different from going to a keyboard now and punching things in.

Kraus: Oh, very different. You had to program it in machine language, and I'm prone to making minor errors and I never could find them. It would take me a long time. So it wasn't very good. In 1955, while I was with the Authority, I went for a year to Africa on behalf of the United Nations.

Newton: Yes, and what did you do there?

Kraus: Well, I was on leave from the Snowy Mountain Authority, and nominally I was going to Africa to study the effect of meteorology or to make recommendation about the use of the meteorological services in combating locust plagues.

Newton: I see. And tell us a little bit about the locusts. How in the world are they related to meteorology?

Kraus: Locusts are the biblical locusts. They eat everything when they get there. They come in enormous swarms at very irregular intervals, and they drift with the wind. Locusts have a flying speed of about seven miles per hour, but a swarm stays together. The insects on the outside of the swarm fly inwards, so that the whole swarm, the center mass of the swarm drifts downwind, and they can cover enormous distances. Traveling with the wind obviously have meteorological implication. The possibility...

END OF TAPE 1, SIDE B

Newton: Tape 2 of an interview with Eric Kraus, and we are continuing on the subject of locusts. While Eric was associated with Snowy Mountains Authority and (inaudible) to Africa to study this problem.

Kraus: Well, we were talking about locusts and we were in the middle of it. To some extent they follow the monsoon flying between Southwest Asia and Africa, migrating between India and Kenya and the East African countries. They have reached as far as Siberia and England, after very extensive flights over the sea. The movement of those, in the transitions over there in the Arabian Sea they have followed the same routes as their classic Arabian boats, the dhals(?). The movement of those dhals and the locusts was first described in a fascinating book, an ancient Greek book called the Periplus of the Arabian Sea. That's 2,000 years old. There's a mystery why locusts always arrive -- sorry. Locusts lay dehydrated eggs. Their eggs have to take twenty time their own weight in water before they can develop. And they have an uncanny ability to arrive in the desert at a place when it rains, when you have an isolated rainstorm there. And then they lay their eggs in wet sand and out come the locusts. That was considered the mystery until an English scientist, Rainier Redreni(?) pointed out that if you drift with the wind in the subtropics you ultimately must land in the convergent zone, and it rains in the convergent zone, and that's how the locusts are programmed to find rainy patches in the desert. My job was to find out whether or not meteorological forecasting could be used to make the locust prevention and prediction more cost-effective. When I got there, there were about eighty aircraft distributed, partly supported by FAO and distributed over all the country of northern Africa and southeastern Asia, and there were stores of poison and (inaudible) available in all these countries, waiting for locusts to come, and they may not come for ten years or so. Question was, if you had a central organization that could dispatch combat teams into regions where locust outbreaks were likely to occur, whether it would be more cost-effective, and whether meteorology could be used to predict that

sort of thing. It appeared to me after I looked at it for a year that meteorology is only of very limited use in that context. They do affect the movement of locusts. It's not only this movement into the convergence zone, but for example the way they move northward across the Sahara, against the prevailing north wind, is another case in point where meteorology plays a considerable role. So it happens that they only fly, they only take off in these big swarms and if the temperature is above a certain threshold, is relatively warm. When it's cold, they just don't fly. They sit in the ground and eat what's there. In winter, in north Africa, you have a series of cold fronts, depressions, sweeping along the southern edge of the Mediterranean and in front of these fronts on the eastern side of it you have a south wind which is warm and they fly northward with it and then behind it and the front overtakes them. It's cold, and they just sit on the ground. And that's the (inaudible) effect. They make their way northward to Israel and Egypt, where they're one of the seven biblical plagues -- plagues of Egypt.

Newton: So the locusts survive because of the sort of general circulation aspects of the ITCZ. They're transported by an eddy process, or they take advantage of an eddy process.

Kraus: They take advantage of an eddy process, yes. They certainly take advantage of the convergence process. But in order for forecasts to play a role in their control, you would have to be able to forecast the weather and to forecast things like individual storms one or two...

Newton: Months ahead.

Kraus: Yes, which is clearly not practical. So when this was pointed out, my report was not very popular with the authority. But the problem, the practicality, the economic problem is not unlike the rainmaking problem. Rainmaking, you can produce artificial rain occasionally, but it only makes economic sense, can only be used economically if you could forecast what you are doing. We hadn't learned to do that.

Newton: Well, anyway, as a result of this, this venture into the combined meteorological and biological processes that you described, which is a fascinating problem. That must have heartily stimulated your further work in the general rainfall regimes(?) over the earth, particularly in the tropics I believe, your relationship to the ITCZ and its changes, and linking that with other processes in higher latitudes, along with the basic interests of the Snowy Mountain Authority itself, which were (inaudible) climatic I believe, I mean climate-affected. These problems must have stimulated your interest in further examining these things in later years.

Kraus: Yes, (inaudible).

Newton: Climatic layers. (inaudible)

Kraus: It wasn't that directly related to the locusts, more to the work of the Snowy Mountains authority, because a lot of the work there was in hydrology of course. Hydrology, meteorology was in the framework of the authority. A section was in their hydrological

branch, and in order to forecast how much water will come down a stream, how much you lose by evaporation and so forth, obviously meteorology comes into that. But it so happens that there are few records available, stream flow records in Australia, they don't go back. Only a very few went back all through the century, and much more data were needed for the authority so they were all brought up by correlation to a standard period which believe was, if I remember well, was from 1905 to 1943. In other words, you may have only have had a record for one particular little stream from 1930 to 1943, but you could correlate it with other streams and with actual rainfall data, which went back further. So it was all (inaudible) to this standard period. I looked at that at some stage and I found that by extending it further back you seem to get more rain. There was more water available before 1905 than there was afterwards, and that actually led me to the -- that happened to be of considerable political importance to the Snowy at that stage, and that led me to study this area.

Newton: So it was to the advantage of perpetuating authority (inaudible).

Kraus: It made it easier for the engineers in charge to sell this scheme to the politicians.

Newton: You have mentioned forecasting several times, but the forecasts involved there really are seasonal forecasts, aren't they? Precipitation (inaudible)?

Kraus: Not only. When I was there, it was a question of construction and hydrological, sending teams of hydrologists to very remote and difficult regions which are very difficult to access, so it was not just seasonal. It was more than that.

Newton: I see. In the long run, as I believe you've pointed out, what happens to a dam isn't really what happens -- in filling a dam, it isn't really a one-season matter, but involves repeated events over a period of time.

Kraus: Yes, of course it is a statistical factor. And perhaps I should talk here more if you want me to, but my second contact with CSIRO was rainmaking. At that stage, CSIRO proposed formally on (inaudible) my former organization proposed formally to the authorities to carry out a major rainmaking experiment out over the Snowy Mountains. And they felt very confident. (inaudible) felt very confident that they could actually increase the rainfall over the Snowy Mountains area. Again, you have to be able to forecast what you are doing because in a larger engineering organization, in the case of the Snowy, if you could have produced a few percent more rain, it would have been worth a lot of money, but in order to do so you have to allow for it by building bigger storages and bigger tunnels and so forth, so it involves a large capital investment, and you have to know beforehand before you do that probably you'll succeed. So they proposed to carry out a major experiment over the Snowy Mountain and (inaudible) authority meteorologists. The authority gave us our own money for that purpose. In other words, we were the granting agency, CSIRO was the scientific organization to do that. And I was the supervisor, the monitor of this grant on behalf of the Snowy Mountain Authority, and that brought about our second clash. (laughter) (inaudible) because my interpretation was very much more skeptical than theirs. They claimed very large success

for the experiment, at least Bowen did, which was not warranted. And my position was first by a professor to National University, and later -- well, it wasn't that quick. At some stage, the CSIRO, the head of CSIRO was then Fred White. Tried to silence me in a very unscientific way. They sent a telegram to the commissioner of the Snowy Mountain Authority and asked him to dissociate himself and the Authority from the opinions which this fellow Kraus had expressed. My boss didn't want to do that. A, he had asked me to express an opinion, but he had other reasons as well. The CSIRO claimed really quite outrageous success with their rainfall scheme, and one of the contractors to build a big (inaudible) was an American fellow, American group of fellows, Kaiser Walsh Perrini, and they were behind their construction. And they faced a large financial penalty for being behind, so they claimed, well, CSIRO has said to make orders there's rain. It's an act of God, or an act of CSIRO. Therefore we couldn't finish this dam, and OK, it cost you a million, that's just too bad. So the Authority had an interest to show that rain was falling, and it was falling, and the success which was claimed was very strongly stratified to occur. The target area was much lower than the control area. And in certain weather situations --

Newton: Much lower in elevation.

Kraus: Much lower in elevation, yes. And had a different rate for (inaudible). And so this thing was dependent on the weather situation, and you could show that the results could not hold up statistically. So on one side was the national university and on the other was CSIRO. We couldn't agree, and finally Roscoe Brane(?) was invited from the University of Chicago to come and investigate the method and adjudicate between us.

Newton: And who won?

Kraus: Well, I thought Roscoe's report was rather careful. He was very traditional in his report, but I think he agreed that the statistics were doubtful.

Newton: What was the name again of the professor at National University?

Kraus: It was Pat Morin(?). He was a professor of statistics, applied mathematics.

Newton: And he took the side opposite to the enthusiastic view of... the wishful view.

Kraus: He just showed that there was some false premises in the statistical procedures that had been used.

Newton: What happened to the Snowy Mountain scheme after this?

Kraus: Well, like all these organizations, they become more bureaucratic with time. It was a pretty wild bunch to start. It was a very interesting scheme in the beginning, the design and investigation phase. And ultimately, the whole scheme was built. They didn't build everything which they wanted to build, but most of it. It's a complicated system of tunnels and lakes. When it was finished, which was after my time, after I had left, it was split into two, one organization which runs the scheme on behalf of the various electricity

commissions of Australia, and the former scientific division of the Authority, of which I was a part, became an independent government-supported research organization, which still does mainly hydrological research and investigation, but also a (inaudible) engineering all over the world on behalf of the Australian government as part of the technical assistance scheme. They work also in Australia, but they work a lot in places like Indonesia, Malaysia, and places like that.

Newton: So the influence spread far, and the actual physical apparatus does in fact deliver a lot of water to the interior.

Kraus: Yes, it does.

Newton: And generates a lot of electricity.

Kraus: But that is just a routine operation these days.

Newton: Oh, yeah, that was the objective. (break in tape) We're resuming a second session of the interview with Eric Kraus on the 6th of November, 1987 at NCAR. Eric, would you tell me something about other involvements you've had while you were at the scientific world while you were in Australia.

Kraus: Well, I believe I told you already about my very short period as a lecturer at Sydney University. At the Snowy Mountains Authority, my job was mainly operational. It involved both the creation of an observing network and forecasting including long-term statistical type of forecasting of the amount of water that would be available for various parts of the scheme. In addition, you had a good deal of purely ad hoc sort of questions thrown out to you. For example, one of the more interesting ones was how transmission wires broken by ice loading could turn out that this is due to a twisting effect because the ice builds up into the wind, rather than by the sheer weight of the stuff which accumulates. During the last few years in Australia, I also became involved with oceanography. I don't know how, because it certainly had nothing to do with the work of the authority, but I was appointed to the Australian National Committee for Physical Oceanography and I forgot whether it was all oceanography or just physical oceanography. And through some curious circumstances I became the chairman of it, so I had to learn something about oceanography, and that was the first time I really learned something about it.

Newton: And was that what brought you to the United States, to its whole oceanography institution?

Kraus: Not exactly. I had written up a paper in the late 1950's which dealt with fluctuations in the trade winds and associated fluctuations in the intertropical convergence zone. That paper was published in Telos(?) after it had been reviewed by Klaus Ruth(?) and I believe on the strength of this paper either Klaus Ruth or Norman Philips -- I believe it was Klaus Ruth, they are both on the committee -- put me up for the Rossby Fellowship (inaudible) in the first instance to (inaudible).

Newton: And what were your impressions on arriving in America for the second time?

Kraus: It was my third visit, actually.

Newton: Third visit.

Kraus: My first impression was the variety of jobs you can get in a big country like America, as compared to countries like Czechoslovakia or Australia. On my way to America, I traveled a few weeks ahead of my family and I stopped first in Tahiti and then I stopped at Scripps. And I gave a seminar, or perhaps two seminars at Scripps. I met Roger Ovell(?) and I was very impressed, and I'm still impressed today by the level of his scientific curiosity because he was a much more senior person and in a much more influential position than I. But he was genuinely interested in finding out, in clarifying in his own mind what I had to say. Anyhow, the discussion finished primarily by him asking me, "Well, why don't you stay here, leave this job, and we'll all stay where you are," which was tempting but I didn't think I could do so at this stage, so I traveled on towards Woods Hole. In Woods Hole, I was a bit of a loss to start with. I was not well trained. I was not really well qualified. And you had to find your own way, of course, of what you were doing. At that stage, they had this famous series of joint seminars with MIT, later expanded to include Harvard and Yale and other institutions. But at that stage it was strictly MIT and Woods Hole, and it was alternately at either institution followed by a dinner and it dealt mainly with fluid dynamics subjects. They were very hot and often very interesting, these discussions, and people like Chalney and Malkos(?) really went at each other. Hammer and nails. I don't know, is that the right expression? Tooth and claw. But a lot of it was over my head. I really didn't know the vocabulary. It's very difficult in science if you don't know the right vocab-- you may even know the meaning, but it's very bad if you don't speak the right jargon, and I didn't at this stage. So that was my first impression of Woods Hole. Later I thought, well, at a place like Woods Hole, the thing to do, if they had only limited -- I came to Woods Hole essentially as a meteorologist, not as an oceanographer.

Newton: But with no particular charge? I mean, did they expect you to take up any particular piece of research?

Kraus: No. I mean, I was as a Rossby fellow, it was left entirely to me, but I felt I should at least tender some sage advice. So I wrote a letter to Fry(?), who had just been appointed director of this institution after being there for a few months, and pointed out that they want to focus on any particular aspect the obvious thing to focus on was air-sea interaction. And that letter had an extremely bad effect on my meteorological colleagues, particularly Joan Simpson, eventually(?) was Joan Malkos(?), and Woodcock, and Joe Levine, where there's cloud physicists. They were basically interesting in cloud physics and they thought that this traitor, this interloper tried to cut the ground under their feet. I was genuinely convinced that if you wanted to push a subject in a place like Woods Hole, it's not so much cloud physics as air-sea interaction, which should be pushed. And it led to quite a strong personal clash, which had further consequences for me. I thought I made it up with John, but in fact I didn't actually. He bore me a grudge for what

happened. I got a joint appointment with him, but perhaps I should say that first I came to Woods Hole as a Rossby fellow during this year. While I was in the Woods Hole, I was a Rossby fellow, and I was offered an appointment, permanent appointment at Woods Hole which I accepted. So I went briefly back to Australia to pack everything up and came back again to America immediately afterwards. Doing this first here in Woods Hole, I did a fair bit of skiing, which is a (inaudible) fair distance from Woods Hole, but I remember my son who had been left at a boarding school in Australia came to visit us just for the Christmas holidays and he was then twelve years old and he came directly from a heat wave in Sydney where it was over a hundred degrees, and he wanted to go skiing. He was quite a good skier in the Snowy Mountains, and we went skiing up to New Hampshire on one of the coldest days which was known there, minus thirty degrees, and the little boy didn't know what had hit him on that day. During the same winter, I went skiing for the first time with Ed Lorenz. It was the beginning of an association which not only involved a couple of papers but many ski excursions and mountain walks over the following years. After my Rossby fellowship was ended, before it was ended actually, the Woods Hole institution offered me a permanent position which I accepted, so I just traveled back to Australia to pack up the house and came back again. The family remained in Woods Hole at that time. They didn't remain in Woods Hole -- this was the first summer which I spent at NCAR. NCAR was then an entirely new organization and I was the first visitor, probably the first scientist which made its appearance at this fledgling institution. The second one, who came two days later, was Ed Lorenz, and we both were here for a few days without any permanent member to NCAR having as yet made any appearance. The first of those was Phil Thompson.

Newton: That was back around 1961.

Kraus: It was the summer of 1961, and when I came back, I went back to Woods Hole in fall and at the same time became an adjunct professor at Yale University, New Haven. Spent my time in Woods Hole, and every week I had to spend two days in New Haven.

Newton: That was in the department of geophysics?

Kraus: That was then in the department of geology. They planned already to expand it. There were some geophysicists there, people like Touriki(?) and a number of people interested in climate change. And they planned to expand it, but it was the geology department which (inaudible) chairman. I think because of this difference of opinion which we had, Joan(?) was a bit -- Joan Simpson, or Joan Malkos was a bit carried away by that. In Yale, I was first a temporary adjunct professor and after a year the question of this arrangement being made permanent came up, and I asked actually John whether she would act as my referee, and that was because I thought we understood each other by then. It was a very rash move, because she wrote a very devastating letter about the subject. On our second occasion some years later, it so happened that a paper of mine was sent to her for review, and she turned it down but the editor of the Journal of Atmospheric Science, Dick Reed, had heard about these personal differences which we had, so he sent it to a third reviewer after that, who duly accepted it. But both these episodes show that the judgments in the scientific world are not always made on

scientific grounds. To come back to scientific work, I thought having recommended that Woods Hole should go into air-sea interaction, I should put my foot where my mouth was and I tried to learn something about it. And I was very intrigued, which nobody seems to be very interested there, in the phenomenon of the ocean-mix layer. And I've seen in innumerable XBT diagrams -- then, they were not XBT's, they were just BT's -- and this curious isothermal layer near the surface. And I wondered what that was all about, and I tried to develop a theory about it. And the first paper which was written by Klaus Roofs(?) and me together, this subject was based on the assumption that it was due to the absorption of solar radiation at some depths and cooling of the surface, which would produce convective stirring, which is certainly a contributory factor. It later became obvious that wind stirring plays also a role and that led to a second paper which was written together with Stuart Turner. Actually, Stuart Turner wasn't that much interested in the ocean. He had been a student of mine at Sydney University a long time before and he was basically an experimentalist and he tried to reproduce this mixed layer experimentally while I had this theory how it can be produced by wind stirring and then penetrating solar radiation. And then as you go in the same office, (inaudible) well why don't we instead of each writing paper on his subject, why don't we make it two papers and double authorship, one Turner and Kraus, and one Kraus and Turner. That's how the theory was established. Perhaps I should say at this stage that the basic ideas which were developed in these papers really appear to have been first mentioned or first discussed by Keith Bourne(?). Keith Bourne worked in Australia, was an Englishman. He was very bright and he had a tragic bicycle accident which left him with brain injuries and concluded practically his scientific career. And he came to Woods Hole as a visitor, and he had treated (inaudible) the subject on the atmospheric side. He and I, our ideas worked along the same line but I certainly was influenced by him. He published his theory of the atmospheric convective bond area well before Turner and my paper on the ocean boundary layer appeared. So I think he should be given the basic credit for this development.

Newton: While you were at Woods Hole, did you participate in seagoing expeditions, and could you say something about the importance of those for the advancement of oceanography or meteorology?

Kraus: A bit, yes. Not too much. During my first year, when I was a Rossby fellow, Rocky Miller invited me to come along on a cruise with the Old Atlantis(?) in the Mediterranean which was rather enjoyable. It was rather nice to get in winter away from Woods Hole. It's part of the well-known phenomenon of the seasonal migration of oceanographers.

Newton: Between warm climes and...

Kraus: Between summer and winter, yes. Tropical oceans and northern. Later on, I carried chief scientist, myself, and a couple of air-sea boundary layer experiments. First of all, we tried to do something in Woods Hole, in summer school, (inaudible) around there. And we had great difficulties with getting permission to get our instruments installed in time, because Woods Hole omitted to ask the relevant authority, Massachusetts authority, for permission to erect a tower on the sea surface in Buzzards Bay and we missed a deadline

for it, so we tried to get permission to do it later by going directly to the governor, who could've given that permission but this would've implied -- this was Governor Peabody -- that would've implied that he had to make a decision which is normally the prerogative of the upper chamber in Woods Hole, Senate in Massachusetts. I don't know what it was. And he tried to abolish this Senate, make it a one-chamber House. And so he didn't want to complicate this own position by overriding them or doing something, so he said he couldn't do it, but he referred us or the representative of the institution to our local member, who should be able to fix that sort of thing for us. And when we tried to get to our local member we found that the local member was in jail. (laughter)

Newton: So that's what determines the course of science.

Kraus: Well, at that stage, we gave it up because time had come for the summer course. But some knowledgeable character in Woods Hole said, "You were a fool. You just bothered the wrong side, the wrong way. All you had to do is to tie a swim bladder to your tower and call it a buoy. Then it is no longer under the jurisdiction of Massachusetts but of the Coast Guard. They would've given it to you straight away." So that was one of first experiences of organizing something of the type. We run another experiment and quite a long one which costed a lot of money, and down in Aruba and the Dutch Antilles, where there are persistently very strong winds which makes it a very suitable area. And there again, I had very bad luck and we installed all our instruments with considerable difficulties and erected a sort of floating buoy which was something of a predecessor to FLIP(?) which was invented by Dan Clark(?) in Woods Hole. When we came back for a rest in Aruba itself over the weekend, hoped to start operations and observations the following weeks, and the wireless operator of the (inaudible) thought it was a good idea to use this opportunity to borrow this expedition's Zodiac without any permission from either the captain or myself. He took the Zodiac out, and it didn't work very well. He took it out to a beach, there were strong winds. It didn't work, and it (inaudible) and we fiddled around with a screwdriver and got it going again on the beach, and took off. But within fifty yards from the beach, the motor died out completely. Actually, it was out of gas, but he didn't realize it. At that stage, he could've easily swam back but he didn't and the long and the short of the story was that we searched for him for three days in very strong offshore winds, stopped the whole work. After three or four days we gave up. It was not only us who searched. The Dutch Navy searched and the Coast Guard sent a plane. We never saw him, and he was finally picked up by pure chance by a passing tanker after he had been four or five days at sea. He was in the Zodiac without any food and without any sun protection. He only had a singlet, and without any brains. He had no brains whatsoever, but he had an incredible will to live, because he clung to the Zodiac even when it was overturned by a wave, and he was finally fished out but that was practically the end of the time which I had on the ship, so we didn't get many observations on that time. But what I would like to say on this occasion is that there is a considerable difference between oceanographers and meteorologists, insofar that oceanographers are accustomed to get their own data, and even today most of them do or many of them do. Meteorologists are more accustomed to get -- certainly synoptic meteorologists are necessarily accustomed to get their data served on a platter. And it makes for a very different outlook and produces a different type of scientists. In Miami

later where I was associated, where the chairman of there was a chairman of a department which included most meteorologists and oceanographers, it was obvious that the oceanographers are usually much more ornery and much more difficult to deal with, because they are more independent than meteorologists are. That doesn't say of course that you don't depend both in meteorology and oceanography on data per se. It doesn't matter whether you get them yourself or somebody else gets them. I believe one gets a certain inspiration from looking at actual conditions which can not be reproduced, for scientists, by looking at one's (inaudible).

Newton: Well, if you go on a cruise or on an aircraft flight or so on, taking your data, you also guide where and how the data are taken. That's another aspect of it.

Kraus: But synoptic data, in this sense, is when you see a chart you get an impression of flow.

END OF TAPE 2, SIDE A

Newton: The interview with Eric Kraus. Eric, was the gentleman scientist atmosphere still there at Woods Hole when you arrived?

Kraus: In a way, yes.

Newton: You mentioned the sailing on the Atlantis and we might mention here that Atlantis One was a sailing ship. It was a ketch, I believe the world's largest ketch, and suitable for these cruises to the Mediterranean that you mentioned.

Kraus: I didn't know that it was the world's largest, and most of the time she was under power. But the story of the Atlantis is another story. She really belongs to the story of the (inaudible). Its beginnings were well before my time there and shouldn't be discussed here. I do remember when the Atlantis left Woods Hole, she was sold to the Argentineans and we were all assembled in luncheon, in a cafeteria, where we could see her sailing out. And the operations, head of operations, Woods Hole, asked us all to stand up and be silent for one minute as the old ship sailed away. But you asked me, it was still a gentlemen's institutions. Yes and no. There were some pretty wealthy people in Woods Hole who did science in Woods Hole really because they liked to do it. But apart from that, the director before Phi(?), Columbus Islin, was himself a rather independently wealthy man. And I think he had general difficulty in understanding why people should be paid. It just didn't occur to him. And he was a really very likeable man, but economic conditions were beyond his ken a bit. And on the whole, the payment of the permanent scientists in Woods Hole was not very high. It seemed tight, and what was offered to me seemed high to me when I came from Australia, but it was not very good from an American point of view. Mine was probably better because I was a newcomer. It was people who had been there for a long time who just had been paid poorly in the past, and were not brought up sufficiently for us. So there was a good deal of dissatisfaction from that point of view in Woods Hole, yes.

Newton: Well, another aspect of gentlemen science is you do what you like. Do you have anything to say about that side of it?

Kraus: Well, yes.

Newton: And that's versus being tightly constrained by some project, and so on.

Kraus: What you are constrained in this country, is by the funding agencies. And the pleasure of science in this country, certainly for a university scientist, is in fact that he can do what he likes. It's very rarely that any administrator will interfere with it seriously, provided he has the funding to do it. So that is the main constraint which exists. And in some way it works quite well, and it may possibly encourage competent mediocrity. Clearly, the peer reviewing system prevents anybody being funded who is obviously incompetent or not well prepared, or has poor ideas. It's just possible that a genius would also find it very difficult because their reviewers wouldn't understand what it's all about.

Newton: You next took up a position at the University of Miami, I believe in 1966 or thereabouts. Tell us the circumstances of that.

Kraus: Well, like my transfer from Australia to Woods Hole, it was a gradual transfer from Woods Hole to Miami. And while I was in Woods Hole, we had a visit from Fred Singer, who just had become a dean in Miami and was in charge of creating a new school of the sciences. That had been cooked up by Werner Baum who was vice-president at that stage in the university and Singer together. And Singer in Woods Hole invited me to come down with a visiting professor. So my first visit to Miami was -- I went as a visiting professor for a few months. The School of Earth Sciences, which didn't last for very long, was divided into a number of institutions, oceanography, meteorology, and I forgot what the others were. None of the others got off. They took off, actually. There was one on planetary science, and one had some biological -- molecular evolution, yes, was another one that existed too. While I was in Miami, I was offered a permanent professorship in the so-called Institute of Atmospheric Science. The problem with the whole idea of the school was that there was an existing relatively large oceanographic institute or marine institute, which was incorporated into the school. It was far bigger than the whole rest of the school together, and it was run by Wharton Smith, who didn't much like this idea and who felt something was put over by him, that he was incorporated into the school. And he and Singer didn't get at all work together, so there was very considerable difficulties. And within a few months, within a year of my being there, the whole idea broke down mainly because Smith and Singer couldn't get on together. And as a result, Singer, this school was abolished. The institutes were made again separate, those which existed, and I was asked to become the director of the Institute of Atmospheric Science and Singer for some time stayed on as a professor in this institute. He was an interesting colleague to have, because he brought some very interesting visitors there. He left after relatively short time. You want me to talk further? For some years, we existed as an independent institute. Clearly, you can't run a research institute within a university without having a Doctor's program. We only had graduate students and you just don't get good graduate students if you can only give a Master's degree. And

I tried to --

Newton: Excuse me. You did not at that time grant a Ph.D. in Meteorology. Where did these people go, then, characteristically?

Kraus: Yes, we were told they could go to physics. They could get a Ph.D. in physics, which wasn't very satisfactory because obviously this is a requirement for a physics Ph.D., very different from a Ph.D. in meteorology or atmospheric science.

Newton: Did many of them drift to other universities in meteorology?

Kraus: We didn't have that many. We didn't have any undergraduate program and we didn't have that many good students at that stage. I tried to get a doctor's program approved. And that led around to better position from the university senate, who felt Doctor's programs cost money, which is not true in the case of atmospheric science, because all our graduate students later or even before were supported by grant funds, so it wouldn't have cost the university anything. But it's difficult to for an English professor to understand that. And we had difficulties on that reason, so it seemed to be best to join the Marine Institute. Actually, I was told, why don't you become either a sub-branch of the school of physics or the department of physics, or combine with the Marine Institute which had an approved Doctor's program, admittedly, mostly in biology.

Newton: That's Rosenstiel?

Kraus: That was not yet the Rosenstiel school, but it was a predecessor of the Rosenstiel school. And I thought in any case it's a good idea to educate meteorologists and oceanographers together. So we became a division in a new school. It was not as glorious a science school, the School of Marine and Atmospheric Sciences, because I insisted it's not a marine school. It has to be marine and atmospheric sciences. And that gave us the flexibility to grant Ph.D.'s.

Newton: You mentioned earlier that there was some difficulty between Wharton Smith and Fred Singer about the wedding, and there was a separation of forces earlier. And now the forces are coming together again. Could you comment on that?

Kraus: Well, earlier it really was a very little dog wagged by a very large tail, namely the Marine Institute. And by that time, when the forces got together again, the Institute of Atmospheric Sciences had grown to some extent, and oceanography and meteorology are rather obvious partners. Unlike Singer, I think I got on rather well with this Wharton Smith. He was autocratic, but easy to get on with. We didn't have a similar personality clash, and Wharton Smith was quite happy to be the dean of our school, of which he was the dean, but he was very unhappy towards part of an empire which was run by somebody else. So there really were difficulties on the (inaudible). Smith was a good administrator in the sense that he let things develop as they wanted to. The senior, various heads of division had lunch. That was really as far as our obligation went, had lunch with him once a week, and the difficulty was to recover during the afternoon from this luncheon. These luncheons were usually rather alcoholic. Wharton believed that

alcohol is an excellent universal scientific lubricant. And one of his institutions which still exists there is, with great difficulty because there was institutional opposition to that from various governmental bodies. He organized a school of Marine Sciences, or Marine and Atmospheric Sciences to have a licensed bar. He believed that it is a very good place for scientists after work to get together and have a drink together. That's how ideas are born. No personal difficulties between him and me.

Newton: I think there's a lot of substance to that view, obviously. Can you tell me something about the institutional transition to the Rosenstiel School and eventually to CIMAS. Well, you'll tell us later what CIMAS is.

Kraus: Well, the School of Marine and Atmospheric Sciences was transformed into the Rosenstiel School of Marine and Atmospheric Sciences after Mr. Rosenstiel gave it several million dollars. I can't remember the exact quantity involved. The money was used for buildings, mainly, and also in the Rosenstiel Lecture which is handed out once every year or so. Perhaps I should tell about an occasion several years later after Wharton Smith had retired, and one of the persons who got a Rosenstiel prize. The prize was awarded every year and won a gold medal and five thousand dollars, which in the 1960's and early 70's was a fair bit of money. And it was given in rotation to a biologist, a geologist, and a physicist or geophysicist, and a physical oceanographer, meteorologist. When the turn came around for the dynamic sciences, I was on the committee and I recommended and an award was given to a Kitagorovsky in Russia. Kitagoroski was able to accept the prize but the condition of the prize was that you had to give two lectures, and he couldn't do that because the Russians wouldn't let him out at that stage.

Newton: For what reason? Is that known?

Kraus: Well, if you want to, I'll tell you something about Kitagorovsky which might have a bearing. Kitagorovsky's a Jew. Don't think he made much use of the fact that he was a Jew, but he was a pretty unorthodox character. I ran into him first at an oceanographic congress in 1967 in Moscow, when he invited a number of us, Turner, and me, and Nick Fofanov(?) from Woods Hole to a beer garden on the outskirts of Moscow. And after, when we wanted to go back, we had to go back because there was a reception in the American Embassy, and when we tried to get back there was no taxi available for love or money. We hadn't a clue how we could get back to our reception. That didn't faze Kitagorovsky for a moment. He had solved the problem by summoning an ambulance. They stuffed the three of us in an ambulance, into the ambulance, explained it to the ambulance driver who was tickled pink, refused any payment, and we arrived in style at the American Embassy in an ambulance. Well, a chap who operates in this way, I wonder that he lasted as long as he did in Russia! Finally, he left Russia, but after he got the prize from us for several years, he couldn't come to give the lecture, and he just wrote me and said "Give me the money and let me in peace." But we couldn't do that. He finally emigrated, first to Finland and then to this country, and he came to Miami to give his lecture. I was sitting, I had to introduce him and I was sitting in the front row and next to me, a couple of seats away, there was a character, rather funny-looking character who I'd never seen before. There was a newspaper on his lap. He was rather unshaven.

In the middle of the lecture he jumped up and threw a cream pie in Kitagorovsky's face in the auditorium and ran out as fast as he could. And the whole thing went so fast that nobody tried to catch him in time. Kitagorovsky took it in pretty good spirits. He sort of licked the stuff off his nose and said, "Tastes pretty good." The audience thought it was a pretty good show.

Newton: But you never found it whether it was a rival oceanographer.

Kraus: It was not a rival oceanographer. It was a representative of the Jewish Defense League who protested against a Russian being given this prize, without realizing -- obviously they hadn't done their homework, because they didn't realize that Kitagorovsky was both a defector and a Jew! But they didn't know this. If you want another occasion of a similar nature which happened to us, we had to visit on a Russian ship, and we had a seminar in Fort Lauderdale, at Nova University and in the middle of that the police came in and ordered everybody to go out because of a bomb threat, a Cuban bomb threat. So we duly all traipsed out except the lecturer on the blackboard which was Klaus Ruth. And he said, "Just a moment! Just let me finish this sentence!"-- writing furiously on his blackboard. Well, that really hadn't much to do with the name Rosenstiel, but I would like to say that the Rosenstiel Prize went to a number of rather distinguished scientists, and some of these lectures were very interesting.

In Miami at that time, it was the late 1970's, 1977, had a rule that everybody had to retire completely at the age of 65, and I became 65 in 1977. So I wondered what else I would do. I certainly didn't want to retire, and I had a long-term connection with Boulder. I had spent every second summer in Boulder, had a collection with NCAR. And I wanted to come out here, and I approached the director of the environmental research laboratories in Boulder, at a lower institution, whether I could join them out here. There are a number of reasons why I didn't want to get to NCAR. I had been a trustee and I think it would've been wrong trying to get a job, and I'm not sure whether I would've been offered a job at that stage. But I thought I could get myself a job at CIRES, which was a cooperative institute between the University of Colorado and NOAA. The director of the environmental research at that stage was Bill Hess, and he told me, well, you can do that if you like to but why don't you stay where you are? We tried for a long time to have a similar institute in Miami, and why don't you start an institute like that in Miami and we will give you partial support for it. So I decided why not, and for that reason I stayed on in Miami. And the university allowed me to stay on as a professor without tenure, because I had this other job.

Newton: So that's how the Cooperative Institute for Marine Research...

Kraus: CIMAS standards for Cooperative Institute for Marine and Atmospheric Science. It was a joint venture, really, between the School of Marine and Atmospheric Sciences and the local NOAA laboratories. The basic idea, as it was explained to me then, it's no longer exactly the way as it operates now, is to have these institutes at places with an active university department and at the same time a number of NOAA, major NOAA laboratories. The original idea was that Miami, Boulder, Seattle are particularly suitable

for that purpose.

Newton: And what were the other institutes at Miami that cooperated with the --

Kraus: There were three major NOAA institutes in Miami. Atmospheric ocean, the Atlantic -- I'm sorry, but AOML, I'm not quite sure what AOML stands for.

Newton: (inaudible) Marine Laboratory, I believe.

Kraus: I don't know. I only know it as AOML, after I was a part of it. I can't remember all it stands for. They were major, major partners. There's the Large Fisheries institute and my successor actually was former director this fishery institute, Bill Fox. And at that stage there was an independent hurricane research institute which later became part of AOML, so there were these three different groups.

Newton: Would you like to say something about your personal scientific work during this time?

Kraus: I don't know how much there is to be said. The Kraus-Turner paper appeared during this time, but it really was prepared and written in Woods Hole. Around an experiment, I actually was in Miami when I ran the Aruba experiment, which I mentioned before. During this period, when I was in Miami, I also wrote a book, which takes considerable time when you do it alone, and I edited another book which was the result of my being the director of a NATO advanced research institute in Urbino(?) in Italy. And that book turned out to have considerable success. We deliberately wrote it to give graduate students a useful indication of the state of the art for the following three years. In fact, now, fifteen years later some of the articles are still rather widely quoted.

Newton: The art in this case being oceanic modeling? Or was that the other one?

Kraus: It was modeling of the upper, or modeling and prediction of the upper ocean. Yes, I did run another NATO institute a few years later, but that didn't have the same (inaudible) in France. But it didn't have the same degree of influence as the Urbino institute really had. It was a good institute. Among papers which I wrote then, there was a paper which compared qualitatively the circulation of the ice age to present. The circulation pointed out that because of the divergence of the moist isobars, the top surface...

Newton: Moist adiobats.

Kraus: Moist adiobats. If the surface temperate differs by two degrees over the equatorial ocean, then the saturated air parcels rising upwards may arrive at the two hundred millibar level with a temperature of something like five or six degrees. I can't remember the exact numbers. But in any case, a small difference in the ocean surface can make a relatively large difference in the upper equatorial troposphere. As a result you probably had during the Ice Age equator a smaller equator-pole temperature difference than you have now although at the surface the temperature was very much larger. That may have caused a reduction of traveling perturbations at that stage and reduced heat flow. Something

which can be modeled has been modeled since then but this was a purely physical speculation, this paper. Another paper which was fairly widely quoted, I believe, was one written together with Emiliani and Shoemaker which involved the meteorological consequences of an impact of an asteroid or large body, extraterrestrial body, on the Earth. And it showed that such an impact probably occurred about 65 million years ago and as it probably occurred in the ocean it would have produced at some stage a very large increase in the greenhouse effect and therefore warming rather than cooling. Cooling occurred first because of the amount of dust which would've been in the atmosphere, so you probably would have had a series of very large climactic changes in very rapid succession, which were described in this paper. I would like to say that it was Alvarez's work on iridium deposition which partially stimulated this work.

Newton: Was that work in the earlier work that you mentioned about the ice age? Were those two of the things that brought you to your current interest in matters of uncertainty and chaos?

Kraus: Not really, Chester. If anything, I was always interested in paleoclimatology but if anything I tried to find determined answers in climatology. I tried to find determinate reasons why things developed as they were, so that was not the reason. And probably my interest in this area started, certainly was influenced by, my association with Ed Lorenz. And Ed Lorenz was interested in this subject in the early 60's. I very much liked his papers then. We talked a fair bit, so Ed is not an easy man to talk to informally. We wrote a paper together and this question of predictability came up in this context. Incidentally, this paper was written at NCAR. We used the NCAR computer to produce the model which was described in this paper.

Newton: Among your experiences as a scientist and doing your research work, can you tell me some of the things that most exhilarated you along the way?

Kraus: It's difficult to say. As you know, particularly in the fields of meteorology and oceanography, essentially applied sciences, you add a little bit here and there. You don't make any basically new discoveries. I'm not aware of any basically new discovery. Maybe the equatorial undercurrent was one such discovery when it was first made (inaudible) others in the Pacific. What excited me when I was a student of Jac Bjerknes in Bergen, he asked me to analyze from the polar year 1935 (inaudible) data, and gradually in these isobaric charts I saw polar vortex emerge. I don't believe for a moment that I was the first to discover the polar vortex, but it was extremely exciting to see it for the first time on an isobaric chart. Later, perhaps the most --

Newton: Excuse me, when you say polar vortex, what you're talking about is a vortex embedded in the polar airstream. Is that correct?

Kraus: I mean a circumpolar vortex in the upper atmosphere.

Newton: Oh, right.

Kraus: That's what I meant. A circumpolar vortex in the upper atmosphere which is clearly due

to the change in temperature below. It's just a thermal wind effect. Probably the most exhilarating in some ways, the most exhilarating period in my life since my first two years in the Royal Air Force when we started the meteorological reconnaissance flights. And it was an exciting period. Other periods, yes, it was interesting to be involved in the early phases of the Snowy Mountains Authority, which was a huge engineering scheme involving many different disciplines. And to be involved in that (inaudible) and to be involved also in the observational aspects of it was interesting and exciting, while it has lasted. If you ask me what you asked me before, I believe, what papers of mine were most widely quoted, I think there were these climate papers in the 1950's, long-term changes in the rainfall, but probably my most widely quoted paper was the paper with Stuart Turner on the ocean-mix layer. Curiously enough, that paper was almost ignored for three or four years, but then it was very widely quoted and it gave rise to innumerable different variants and derivations. I think a lot of graduate students the world over lived on this paper.

Newton: This is a rather broad question, but during your scientific career, can you say something about how you ordinarily learned about important developments through the agency of journals, telephone calls, personal contacts, and so on?

Kraus: I'm not sure. You probably learn a lot by glancing through journals, not necessarily reading them in great detail. There are too many journals and too many articles for any one person to go through. But in some cases, you get your ideas directly from the data. As a matter of fact, I remember now that you asked me Vilhelm Bjerknes telling me once that it's bad for a scientist to read too much. They shouldn't read too much. It is more important to think yourself than to look too much at the literature. And there's probably something in that. So you get your ideas for a variety of processes, each of these being important.

Newton: What journals did you read, or have you read regularly?

Kraus: I can't tell you to which journals I subscribe. For a long period, I always had been a member for a long time of both the British and the American, the Royal Meteorological Society and the American Meteorological Society. I've given up my membership in the Royal Meteorological Society some time ago, so I haven't read the quarterly journal for a fairly long time now. And during the last few years I restricted my reading of journals of the American Meteorological Society largely to the Journal of Physical Oceanography which I glance through fairly regularly. And if there's anything somewhere else, I rely on being told somehow hearing of it from other sources rather than finding it myself. It's not a very efficient and new way of finding out, but I find there's just too much to digest by itself. For the same reason, I argued in Miami when I was a professor there that language examination should not be part of a graduate curriculum. I prefer to have students who are well educated and speak other languages. They shouldn't learn it as a graduate student because it's unlikely that they'll have the time in their profession to read many foreign journals. As it is, they probably don't read the English journals.

Newton: I understand that, but how much do they miss in the process by not reading? You

yourself are able to read in many languages, a number of languages. I always felt that I somehow missed something by not going to the foreign journals regularly and at least seeing what was in them. Do people miss a whole lot by not being multilingual?

Kraus: I don't think they're missing anything by not going into them on a regular basis. Something important will be brought to your notice somehow, will be repeated. But you miss the opportunity to read the occasional article, the occasional reprint perhaps, which is sent to you which you heard about and which you want to read in the original. That can be useful, but it is a relatively rare event. It is not something which comes to you regularly. And the other important advantages of a foreign language is to be able to (inaudible) your colleague at a cocktail party. It's quite an important advantage!

Newton: Oh, yes. Have you started on any grant review boards or any committee which influenced the way funds are given out in research?

Kraus: Well, we have all have served innumerable times as reviewers for a proposal to NSF and other granting agencies. I did serve on the review board for NASA, and from two review boards actually, one in connection with the scatterometer, and work that affects it. When I was a chairman at the School of Marine and Atmospheric Sciences, all the chairmen sat together at luncheon with Wharton Smith once a week and we had to look through all the proposals which we are meant to approve them, and it was interesting. It was quite a rule that a proposal that involved one or several million, breezed through this discussion, through these meetings, as practically without any discussion. It was immediately approved, but some proposal which may have involved some five thousand or ten thousand dollars for a biologist may have evoked half an hour pretty fruitless discussion. The time given to the review of these issues was inversely proportional to the amount of money involved.

Newton: So you were putting into operation Parkinson's Rule to that effect. (inaudible) toolshed.

Kraus: I think I put into operation a rule which seems to apply (inaudible) in the political world, and administration of this country.

Newton: Have you served on any advisory bodies? For example, government panels or others?

Kraus: Yes, I served on one of the government panels and I was for a long time a university representative and for six years a trustee on the UCAR board

Newton: Can you tell me something about your experiences as a trustee, or particularly the views you held as a trustee of UCAR?

Kraus: I became a trustee in the wake of doing a period which involved a large number of major changes in the administration of UCAR and it was at the end of Walter Orr Roberts' tenure as chairman of the corporation and there was a large change in the composition of the board at that time. This was due to a certain amount of dissatisfaction with the

university, or the majority of the university community about the way NCAR or UCAR was being run. To some extent this divergence of opinion or difference is perhaps inevitable, because there's a natural conflict of interest between NCAR and the university. There's also a confluence of interest, but there's a conflict of interest as regards the allocation of funds from the National Science Foundation goes. And there was a feeling among many university people that more support should have gone directly to the university and perhaps less to NCAR. It's a question of a prize being cut up, or a pie. It was final, and was finalized at the time. I certainly believe as a trustee and it was not always a popular opinion to hold, that scientists at NCAR should operate in a very pleasant environment, they should be very carefully selected, and it's a privileged position in some ways and they should also get the maximum remuneration which can be paid. But at the same time, I also felt that this should be accompanied by a lack of tenure, that there should be a large throughflow of people in the NCAR environment.

END OF TAPE 2, SIDE 2

Interview of Eric Kraus

TAPE 3, SIDE 1

Newton: This is side one of tape three of the interview with Eric Kraus. Go ahead, Eric.

Kraus: I did feel that NCAR scientists should be well paid but I didn't think they should have any tenure at this place. Many scientists at NCAR thought at this time that they were over-reviewed, and they had too many review processes. And I didn't have a great deal of sympathy with this point of view, because coming from a university where people had to apply for support for their research, to NSF every year or every second year, this could tend to be reviewed at least as often as the people of NCAR were, so I didn't see why they should have any particular privileges in this regard.

Newton: But you did have a strong feeling that, as you mentioned, the flowthrough of people was necessary to keep an institution alive, and you must have been concerned about scientists becoming stultified by having too much security in their positions, sitting in the same chair too long.

Kraus: It's not the question of security. It's a point of the same people being together for too-long time. That stops them from having to say to each other anything, and they are unlikely to produce new ideas on these circumstances. And it's a problem of any institution which is entirely devoted to research. At a university, at least you get graduate students coming through asking (inaudible) questions which professors have to answer, like it or not. But if you're at an institution there's a risk of people becoming old together, not a desirable development.

Newton: Do you feel that NCAR is at all special in the respect that there is a strong flow of visitors here through NCAR and this includes graduate students from various universities?

Kraus: That is true and all the NCAR's relatively large, so I'm not sure how much people in different branches of NCAR talk with each other. But the risk of getting old together is absolutely deadly in a small research institution.

Newton: You served as chairman of the department at Miami and later, or perhaps currently, as the director of CIMAS. Could you give us your opinions about the role a chairman should play at institutions like that?

Kraus: Well, obviously as an academic institution where people -- but an academic institution in this country where people get their own funding, the power of a chairman is very restricted. There's practically no power. And his influence is strongest at the time when people are being appointed. And I think it's the job of a good chairman, that's his real job, or a research director, to get out of his way to identify good people, to invite them and to bring them to the notice of his colleagues. And that is the most important job which a chairman of a university can do.

Newton: Would you tell me, who are some of the people you did bring to Miami?

Kraus: Well of course, I didn't bring anybody. They all came under their own free will. But I think I did some fairly strong selling in some cases. And as a result, several people may have come to Miami who wouldn't have even thought about it otherwise. I would mention Klaus Ruth(?), and Walter Doing(?), Roget Lamitte(?), and Reinhard Bleck, perhaps, in this connection. Marvin Geller is another one.

Newton: Could you tell us more about the many famous scientific figures you have run up against, not just respective to meteorology and oceanography, but you have encountered well-known people in many fields.

Kraus: Well, I'll try to do, but what I have to say will be mainly anecdotal. Before the war, I was in England and I met Lindemann, who became Churchill's main scientific advisor, who even then struck me as a rather autocratic gentleman. I think he had very right-wing sympathies. He was very firm in expressing them. Vilhelm Bjerknes, the father of Jac Bjerknes was a delightful gentleman. I went skiing with him when he was over eighty. He was an extremely gentle sort of soul, and I was talking about Jac Bjerknes before, so I don't want to repeat it. He had great physical insight. He had a horror of lecturing. He was a very shy man, essentially, who was very strongly aware of his own shortcomings. So he was well-known. Amongst the more colorful figures which I met, there was Bergeron and Rossby, who were both rather exciting types. Bergeron, I met him -- I can't remember who it was. He had just run away with the wife of a German meteorologist but he was an exciting chap to be with. I remember how excited, he liked in Bergen to go to the weather bureau, which was at that stage presided over by Sverre Petterssen, which was on a different floor in the same building. And Bergeron liked to go up there and rummage around in the (inaudible). It was a great triumph in the 30's, of finding a news station, of sending news from somewhere. There was a cartoon (inaudible) there, with Bergeron going and yelling "Timbuktu!" as one of the places we very first reported. When I came to England, I met a number of figures who became well-known. The operations researcher at coastal command was Woddington. Became a rather close friend of mine. He was always known as Wod. He didn't even like people to know his first name, and Wod was a well-known biologist. He wrote other books on art. His first book which he wrote was on the scientific attitude. It came out as a Penguin book. And then he wrote, among many other books which he wrote, a book on science and appearances, which is an odd book, which is a rather interesting book. Wod even introduced me to some of his colleagues in Cambridge. I met C. P. Snow, the novelist, started off course off as a scientist, and I remember we once traveled together to Cambridge. First time I visited Cambridge I was an Air Force officer, and it was quite impressive to be invited in Cambridge to sit at (inaudible) and I didn't know (inaudible) be invited to sit on the high table. It's a very privileged society (inaudible).

Newton: They were in robes and all that?

Kraus: Yes, they were. I was in uniform. Then I remember when we were alone briefly, C. P. Snow asking me, "Well, what do you think about Woddington?" I refused to answer.

The trouble with Woddington is that he's just too clever. People don't trust him because he's obviously so much more clever than they are. And Woddington, as I told you, was very well known. Never got the Nobel Prize. And one of the first people I knew even before I met Woddington, one of the first people I met in London just before I even joined the Air Force was yidney Chapman, who invited me several times to his house and when I got married he invited my wife and me and I think my wife even thinks today that she was corrupted by Katherine Chapman, who explained to her how important it is to treat these important men, namely scientists, very carefully, that they have such an important role in the world to play, and she believed that about her husband and treated him accordingly. But Heather told me she wishes she hadn't been browbeaten by Katherine at a very impressionable age and Woddington introduced me to Burnell(?), the crystallographer who possibly was one of the most influential English scientists at that time. His influence was mainly due to his very large, very broad sort of background. He knew something about everything. Didn't publish as much, but he was a well-known crystallographer. When we first visited Burnell for tea, we were received by a young man about eleven or twelve years old, stark naked without any clothes on. He was Burnell's son, and my wife was twenty-four or so, and while we waited for his parents he entertained us by telling us all about his mother's miscarriages in great detail. And Blackett(?) was one of the most charismatic, best-looking of these English bunch of scientists. He was also somewhat less radical in his political opinion. And Burnell and Haldane were both avowed, card-carrying members of the Communist Party. And I believe Burnell remained so all his life. And so he was involved in several of the Churchill-Roosevelt meetings and apparently kept considerable influence at the meeting (inaudible) had together in Newfoundland.

Newton: Blackett, were those people purged somehow by...

Kraus: Not in England.

Newton: Not in England.

Kraus: No, not in England, they were not. I don't think Blackett was in the nature -- Burnell was not a traitor like Philby. He was open about Communism. (inaudible)

Newton: But they didn't suffer him in terms of their employment, or his (inaudible)?

Kraus: I don't think you could as a professor, once you have tenure. Blackett was not a Communist. He was a former Navy officer but he was a very strong socialist and became a pillar of the Labour Party and a Lord and he was elevated to the House of Lords when the Labour Party came to power after Churchill's downfall. I can't know whom else you want me to talk about. The most impressive meteorologists whom I've met, certainly the best, although my best was those who were best-known probably, were Ed Lorenz and Jules Chapman(?).

Newton: Jules Chumley(?).

Kraus: Jules Chumley, I said (inaudible). I'm sorry. No, Jules and Ed. Jules was by far the most

stimulating to talk to. Ed was a closer friend of mine. Jules was a friend who visited me, visited us several times in Miami. And Ed was difficult to talk with. He was probably the most original scientist in our field, and when you tried to talk with him after a few sentences he got a curious vague look into his eyes and you felt you must bore him terribly and the conversation then came to an end. We got on very well climbing mountains together but we didn't talk very much about science together. He wasn't getting to do that. I spoke about Bjerknes. I don't think any of the others I knew were quite in the category. Ed Lorenz, for his originality, and Jules Chumley, partly for his charm, made him exotic. (inaudible) the most impression oceanographer I've met in this country was probably Henry Stommel. I briefly met Swerer(?), both in Bergen and this country, I never knew him really well, so (inaudible) talk about him.

Newton: Can you tell me something about your plans for the future?

Kraus: Well, I'm seventy-six years old. I don't know how many plans we should have. We're going to move to southern Colorado, live in a house in the mountains. And I'd like to write two books while I'm down there. My book on atmosphere-ocean interaction, which appeared in the Oxford series on meteorology, Clarendon(?) Press, has been out of print for a long time and we want to write another book which follows it up which will be written together with Joost Businger, and I have a contract to publish this in the academic press. But my main goal or my main ambition is to write a book on uncertainty, on uncertainty not only in meteorology and fluid dynamics, but in the widest sense of you taking my example from a very wide field, and concluding with the fact that ultimately anything really new or creative can only be based on complete uncertainty. If it would have been predictable, it wouldn't have been really new. So the fact that the world changes involves essential uncertainty. It would be very dull if it wouldn't do so. Perhaps I should end with this note. Thank you.

Newton: Thank you very much, Eric. This ends the taped interview with Eric Kraus.

END OF INTERVIEW