American Meteorological Society
University Corporation for Atmospheric Research

TAPE RECORDED INTERVIEW PROJECT

Interview of Dave Fultz
November, 1992-January, 1993

Interviewer: Paul Frenzen

Frenzen: This is a taped interview with Professor Dave Fultz at the University of Chicago, on November 11, 1992. The interviewer is Paul Frenzen.

Dave, I see by your curriculum vitae that you were born on August 12, 1921, in Chicago, Illinois, and your father was Harry Fultz and your mother was Ora Fultz.

Fultz: That's right.

Frenzen: What did your father do? What was his profession?

Fultz: He was really an educator. At the time, he was a teacher of mechanical arts at the Laboratory High School here at the University. They lived over on Woodlawn and 60th in an apartment building that's still there.

Frenzen: Was he a graduate of the University?

Fultz: Yes. And also of Armour Institute, predecessor of Illinois Institute of Technology.

Frenzen: Oh, yes, I remember the U.S. Institutes up the road there.

I know that you spent some time overseas when you were very young.

Fultz: Two or three times, actually. I don't know the exact chronology here, but when Mother and I went over, followed my father to Tiranë...

Frenzen: Albania. What was he doing in Albania?

Fultz: He was director of the Albanian Vocational School, *Skolla Teknika Tirane*. It was only a couple of years old. He was the second or third director and the longest-serving one, eventually.

Frenzen: But you were very young.

Fultz: One or two at most. My mother became very ill with malaria and typhoid fever, both. My sister was born in Rome, probably 1923. My mother was in the hospital for a fair time, I don't know how long. We came back to the United States, and except for at least one visit to Paris, where we met my father, she did not go back.

Frenzen: High school was here in the United States?

Fultz: Well, that's a little complicated, too.

Frenzen: You told me you went to Proviso, out in Maywood, here in the western suburbs, my neighborhood.

Fultz: What happened was that the family came from a Southern Indiana town, Salem, which is still literally in almost the same form it was in the thirties. We were living in Denver, where two of my aunts also were, and that's part of the reason Mother went there. But every summer we came back to Salem and at least one of them, we went across to Paris and met my father. I can remember a fair amount about that trip--I must have been six or seven. I can remember the looks of the Luxembourg Gardens, at least, and the model boats the children used to sail in the pond there.

But we made an arrangement that I was to go over to Tiranë in 1932, when I was eleven. Aside from being met by people at all major stops, my uncle in Chicago, and the previous director of the school, Bradley Kelly, in New York, and more or less going along with Robert Ripley on the boat, we landed at Naples and the vice-director of the school, Charles Hollingshead, met me there.

Frenzen: You were traveling alone as an eleven-year old boy?

Fultz: So we went to Bari and took a boat to Durazzo, or Durrës as the Albanians call it. Finally got to Albania, to Tiranë, where the school by then was a quite large enterprise with something over 500 students.

Frenzen: This would have been in 1932.

Fultz: 1932. At the beginning, it didn't cover a very large range of subjects because they started out with students who barely had elementary educations.

Frenzen: Had you entered the elementary school?

Fultz: Yes.

Frenzen: And what grade would that have been?

Fultz: It wasn't obvious. They put me into various things, including Hollingshead's English and natural science, I don't really remember what other things. There must have been some math. We were there for a year, and that had a big effect.

Frenzen: Was there anyone there who led you to an increased interest in science that early?

Fultz: Not at the time, at least not especially. One of the things I remember most vividly about it were the workdays out on the two farms that the school had in the valley there, one sort of up on the slope of the hills--it was called the Hill Farm--and then one in the valley that was a real working farm. In fact, both were, from which the school in fact got a lot of its food, sold milk, sold ice.

Frenzen: And the students worked on the farm?

Fultz: Yes. Not only those who were in agricultural specialties, but the whole school essentially worked on various things.

Frenzen: That's very interesting. Then you came back to the United States the next year, you were there for about a year?

Fultz: What happened was that the Italians were kind of taking over, and that produced a big shakeup in the school system. The Red Cross had not intended to help support the school as long as they did, until 1933, but they essentially were asked to leave. At that time, we decided to leave because the political conditions were getting worse.

Frenzen: So you came back to the United States in what...?

Fultz: 1933. I don't really remember what month it was, but it would have been August or September, probably. On the way back, we took a motor trip, it was very impressive, with Dr. Hackett, who was a malaria researcher for the Rockefeller Foundation. We drove with him, taking mosquito samples and so on through Northern Italy, Southern France, Carcassonne, Barcelona...

Frenzen: Your first scientific expedition.

Fultz: At the end of that, we went back up to Paris and visited a couple of the military cemeteries below St. Mihiel and so on. Then by boat back to Salem, Indiana, where my mother was by then. But most of elementary school I was in Denver, Colorado--Stevens School was a particularly well-known one nationally at the time. I don't remember who the founder of that was.

Frenzen: Did they have a strong program in science?

Fultz: I wasn't aware of it.

Frenzen: Where did you first become aware of a potential career in science?

Fultz: I'd have to think about that.

Frenzen: Stevens School took you through primary grades, or was it sort of a middle school?

Fultz: It was probably up to fifth or sixth grades--in 1932, I would have been eleven, so it was probably sixth grade or something like that when I left. The Albanian Vocational School was much broader than the typical curriculum would be of the day. In fact, I must have done quite a bit of carpentry in the wood shop and a fair amount of typesetting in the print shop. They had a complete printing press.

Frenzen: That's interesting. I just read a critique of education of engineers today who learn computer-aided design and little else, and was thinking of the number of students that I had in the last few years and trying to put them into field projects--who've never seen a drill press. It's difficult. So in a way, your technical education began in this way.

Fultz: With a spade out on the Hill Farm.

Frenzen: So you were in the Stevens School until about sixth grade.

Fultz: Right. And then I went to the Tiranë School for one year.

Frenzen: The school was still running then?

Fultz: Yes. In fact, it ran in a fractured sort of way for the next decade under Albanian directors, but without the Red Cross connection.

Frenzen: Your father was still there, then.

Fultz: No. We came back in 1933 and the Albanians took over then, running the school. So my father and I both came home together. We went to Salem, Indiana, and that year I went to school in Salem, first to the eighth grade for the early half-year, and then they booted me up to the high school. I don't know precisely why.

Frenzen: Probably because you were multi-lingual.

Fultz: No, because I didn't learn much in the way of languages there. In one year, I didn't learn much Albanian.

Frenzen: Did the school teach in English?

Fultz: The school taught in English. That was done to a large extent deliberately and all the boys had to learn English. It was done because particularly when they started in 1921-22, there were no books in Albanian on science or anything like it, and no technical terms in the language at all for mechanical or scientific ideas.

Frenzen: So you started high school in Salem, Indiana?

Fultz: Right. So that would be 1933-34. My father was traveling for the Red Cross then. In 1934, we moved up to Maywood and I went to Proviso, which I think is now Proviso East.

Frenzen: Maywood is a western suburb of Chicago, for the record.

Fultz: I went there for two years and one summer, I took a summer course at Hide Park High School, where we are now, and the following year, we moved to the South Side, and my last year of high school, I went to Hide Park High School (1936-37).

Frenzen: Which is the public high school situated near the University of Chicago.

Where did science begin then? You told me a story once, about your grandfather

Fultz: The earliest scientific idea I can recall was in a conversation with my maternal grandfather during one of our visits in the summer. We were sitting in the evening on the swing on the porch looking southward out where there was lots of sheet lightning going on, and he was telling me about the conservation of mass. I don't remember what the context was, but I can still recall it.

Frenzen: He didn't call it heat lightning, as we did in those days.

Fultz: Probably we did too.

Frenzen: Can't see it anymore because of the city lights.

Fultz: Salem is a fairly small town, still is, so you can still see that sort of thing there, even now. It was about 3,000, and I doubt if it's more than 4,000-5,000 even now.

Frenzen: Back at Hyde Park High School, did you know early on that you were headed for the University of Chicago?

Fultz: No, not particularly. We lived at 73rd and Dorchester and I, most of the time, walked to Hyde Park High School, which was feasible then in that neighborhood.

Frenzen: Your father was a college graduate. In fact, was he a graduate of the University of Chicago?

Fultz: Yes. And of Armour. My mother was a graduate of Bradley.

Frenzen: So there was a strong college tradition in the family?

Fultz: --both families. My father had nine brothers and sisters and they all went to college, which was unusual for the five girls in those days. I believe my mother and both of her sisters went to college.

Frenzen: So when was the decision made on the University of Chicago? Was it just a neighborhood decision?

Fultz: It was made because I took a scholarship exam and succeeded which meant that I could go to the University very cheaply, living at home.

Frenzen: I remember when I did the same thing, it was $100 a quarter to go to college.

Fultz: Since I had a scholarship, it was even less than that for most of the time I was there. I had a small account book that I kept through those years, totaled all my out-of-pocket expenses for the whole university period and the total was about $630. The books, everything else.

Frenzen: Had the Hutchins plan started, the great survey courses? I was about three years behind you, I would say, from your birthdate.

Fultz: It was right in the middle.

Frenzen: So you were taking the big survey courses, Bio Sci, Phy Sci...?

Fultz: Right, with the comprehensive exams at the end of the year.

Frenzen: With the orals.

Fultz: And orals.

Frenzen: Thirteen books in biology. Well, did those courses lead you to believe that science was the way?

Fultz: I think it was probably earlier, in high school, including at Salem where there was a very effective teacher whose name I don't remember. A lady who taught what amounted to botany, and we went on lots of field trips in the spring with her. Then at

Proviso--

Frenzen: Did you start collections or anything at that time independent of your courses?

Fultz: Not at that time. But I remember the Latin teacher there, they were still teaching Latin in high school at that time. Then at Proviso, some of the names escape me, but the teachers I can really remember.

Frenzen: In what fields?

Fultz: A teacher of ancient history, one of mechanical drawing, which I took quite a bit. In fact, I had some of that in Albania, at the elementary level. And a geometry teacher, a lady who, not too much later, was president of the mathematical teachers' society. She was quite impressive.

Frenzen: Back in the days of Hutchins' programs at the University of Chicago, I was impressed by the fact that our lectures were given by the major professors in their field. Was that true in your day?

Fultz: Yes.

Frenzen: I especially remember the biological science curriculum for some reason.

Fultz: Well, I had Anton Carlson too.

Frenzen: I was thinking of Buchsbaum. There was an anthropologist, Fay Cooper-Cole, and--

But that exposure at that age was very impressive to me and perhaps to you. The war intervened in my case, World War II, but the physical science course in those days, do you remember some of the people? Today, the kids call it "Rocks and Stars" because it was geology--

Fultz: They've got several variants now. Then, it was fairly set. There was one quarter that was geology, and...

Frenzen: Atmospheric and oceans--

Fultz: Not much atmosphere-oceans, that I recall. It was mainly glacial geology, drumlins and things like that. I can't pull up the names of the people.

Frenzen: When did you decide to remain at Chicago in graduate school? When did you graduate from the University of Chicago?

Fultz: 1941.

Frenzen: I see here that in that winter perhaps you worked with the Weather Bureau in Chicago? Emergency assistant, U.S. Weather Bureau.

Fultz: That was the next winter, I'm pretty sure.

Frenzen: After you started graduate school? There's a typo here...so that was after you entered graduate school.

Fultz: It was an unorthodox graduate school because what happened was that I graduated in chemistry, having switched from economics in first or second year.

Frenzen: Those were the days when you had to name a major in the college.

Fultz: Right. And what happened was--I don't recall the circumstances by which I got into it, but by the third year, I got to the primary civilian pilot training program, which the University ran at an airport out south with Piper Cubs. Also, I was in that the next year in the secondary program, flying a Waco biplane. Both years, the aviation was run by a very impressive guy named Walter Brownell.

Frenzen: They had a ground crew here on campus, how did they work that?

Fultz: I don't really quite remember, but there was a ground school component that would have been here, I'm pretty sure. Of course there were just these small feeder airports, no commercial operations at all at them, just private flying.

Frenzen: In those days, there was only Midway for commercial, here in Chicago. We had all these small airports, I remember them. But this was then your third year at the University of Chicago, and that would have been what calendar year, 1940-41?

Fultz: It would have been 1939-40.

Frenzen: There wasn't any obvious preparation for World War II that early here? The general population was actively neutral. The war had broken out in Europe.

Fultz: The 3rd of September, 1939. I can still remember. I was down in Indiana at the time.

Frenzen: But there was no military component to this civilian pilot training.

Fultz: Not at the time. It was run by the CAA, which was a civilian agency. But it was certainly a part of gearing up for eventual military action.

Frenzen: And the ground school included some atmospheric science, some meteorology? Was there a meteorology department here in Chicago then at all, was it organized?

Fultz: No. That started up in 1940.

Frenzen: As part of the physics department?

Fultz: Right. As an institute of meteorology in the physics department.

Frenzen: And who were the people involved?

Fultz: Well, it expanded pretty rapidly, but the original people were Horace Byers, Victor Starr and Harry Wexler--this is in the first course, and I believe, Oliver Wulf was here then, he certainly was by the second course, which was the one I took.

Frenzen: These were the first Army Air Force training for forecasters? The department began with the military program.

Fultz: The bulk of the cadets were Army, but a fair fraction were Navy and a small number were civilians, namely under CAA scholarships. So that explains how I was one of those.

Frenzen: I see. And then the Navy took over Bartlett Gym...

Fultz: The cadets, they took over International House.

Frenzen: I remember the offices were in one of these...

Fultz: Well, they were all around.

Frenzen: I went in to sign up in my day too, to the same project at MIT.

They recruited the likes of Horace Byers, Victor Starr and Harry Wexler, or did they take them out of--were they here in the physics department already?

Fultz: No, no. They were recruited for this program. The only one I can recall--there were two who were teaching who were previously here. One was Michael Ference, who was in the physics department already, had been for some years. He wrote a very much-used physics introductory text with Harvey Lemon...and there were two others. One was Bill Reid in the math department, and the other was Henry Leopard in the geography department.

Otherwise, all the instructors were recruited specifically for the program. They received some sort of appointments in the physics department in the beginning.

Frenzen: Dave, when did you actually graduate with your B.S. from the University of Chicago?

Fultz: That was June, 1941.

Frenzen: You entered the then Institute of Meteorology that same summer?

Fultz: That's right.

Frenzen: That was the second class in a series of classes run during World War II by that program. As I recall, my MIT program--the so-called "A" course--lasted nine months.

Fultz: Here it was three quarters, so we finished at the end of the winter quarter of 1942.

Frenzen: I see by your vitae that about that time you took a job with the Weather Bureau.

Fultz: That was arranged for me by Rossby, I think.

Frenzen: Rossby was with the program then. You named everyone but Rossby. I was going to ask when he joined the Institute.

Fultz: We didn't quite finish with the faculty...Rossby of course arrived at the time of the second course, or in the middle of the first course, I don't know the exact time. Others who were on the initial faculty were Mike Ference, who was already in the physics department, Henry Leopard, who was in the geography department, and Bill Reid, who was already in the math department. The others were recruited from outside.

Frenzen: Did Rossby then become the director of the program at Chicago?

Fultz: Yes. And Byers was essentially the executive secretary.

Frenzen: As **Time** magazine once said, "Backstop to a genius." We all knew that quotation.

Rossby went around spewing ideas with his usual charisma, did he start early?

Fultz: Oh, yes. He was right in the middle of it then. It was almost the year of his wave paper in the conference in Toronto.

Frenzen: I remember especially in that there were map discussions in the old department, and it was very interesting to hear people like Rossby talk about the day-to-day weather. Did they run things like that in the--I'm talking about the years immediately after the war when I was a graduate student?

Fultz: Oh yes, they certainly did. I'm a little unclear whether that was done during the war because I don't believe they were really set up for current weather.

Frenzen: There was no spare time.

Fultz: There certainly weren't during the second course. We worked mainly on special cases, historical cases of various kinds.

Frenzen: Did you have any close contacts with Rossby that determined the course of your career?

Fultz: I'd say they weren't particularly close during the second course, but off and on during the war, when I was working at the University and especially right afterward, a good deal of contact...some of the specific suggestions came from him.

Frenzen: We'll come to those. Let's remain as chronological as we can. You finished the course, and then went to work for the Weather Bureau.

Fultz: Yes. For a few months.

Frenzen: In a job arranged by Rossby?

Fultz: Yes. And was that here in Chicago...

One month of it was an extended visit to Jerome Namias' Extended Forecast Unit in Washington.

Frenzen: Who else was there, at that time?

Fultz: At Namias' unit? It was pretty much his regular crew that had been around there for several years. The only one I can think of at the moment is Tom Gray and Herb Dorsey was at that time working in that unit, too. Oh, and Phillip Clapp was one of the principal ones.

Frenzen: What was your assignment in that group?

Fultz: It was essentially to make alone and in connection (with the group) trying to use CAV trajectories. It was already started...

Frenzen: I know you did a well-known report out of Chicago in those days.

Fultz: That was later...

END OF TAPE 1, SIDE 1

Interview with Dave Fultz

TAPE 1, SIDE 2

Frenzen: The Albanian Vocational School that Dave Fultz attended for a year when he was 11-12 years old, directed by his father, was fairly important to his early interest in technical matters.

I read from the "Forward" to a book written by his sister, Joan Fultz-Kontos, entitled **Red Cross, Black Eagle: A Biography of Albania's American School**:

Between 1921 and 1933, the Albanian Vocational School was financed by the American Junior Red Cross, the income from the various commercial activities in which the school engaged and some small grants from the Albanian government. The success of the school, however, owed much to the talents and efforts of its underpaid international faculty. Foremost among these devoted teachers were two Americans, Harry T. Fultz and Charles A. Hollingshead, to whom this book is dedicated. Hollingshead was one of the original faculty, and he continued to serve in this capacity until the school was nationalized by the Albanian regime in 1933. Harry T. Fultz came to the school in 1922 as director...
[Dr. Frenzen continues on the tape]

Returning to the story of your life, Dave, you wanted to add something about the people you knew in high school.

Fultz: Yes. My last year at Hide Park High School, several teachers were very significant influences. One was Beulah Shoesmith, who was the mathematics teacher. She's the one that Shoesmith School is named for. She was also a very astute player of the stock market during the Depression and left the University a couple of million, I think, when she died. So she was one who very strongly influenced my interest in mathematics.

Two others were Mr. Fiedler, who was a chemistry teacher and who spent lots of time discussing the Spanish Civil War out of class hours. Mr. McLane, who was the physics teacher and ran the audiovisual operation of the school; I spent a lot of time running classroom films for him on ancient projectors.

Frenzen: Oddly enough, I did the same thing at Morton High School.

Did you want to add anything about your days as an undergraduate at the University of Chicago?

Fultz: I suppose the most unusual part of it was that I spent all of my summers in Southern

Indiana near Salem, where my uncle was a farm manager and orchard manager. That was quite strenuous compared to city life. Among the courses I took as an undergraduate, I took a rather broad range, including a number in the philosophy department. The two that I remember best were Thomas Vernor Smith, who was well-known at the time as a very active teacher and politician, and Channer M. Perry, who was one of the leading members of that department.

Frenzen: Were these courses in logical aspects of philosophy, or the semantic?

Fultz: Across the range: history of philosophy, ethics.

Frenzen: You told us how your interest in flying took you into ground school and eventually into the Army courses for meteorology, the second course. You became an assistant in the Weather Bureau, and a research assistant at the University of Chicago after you graduated. At some point, you went down to the Institute of Tropical Meteorology at the University of Puerto Rico.

Fultz: When it was founded.

Frenzen: July, 1943. It says here you were there for just a couple of months. Was that a longer assignment?

Fultz: I don't really quite remember. That was set up as a research teaching institute, performing the same function in tropical meteorology that the institute did in general primarily for Army Air Force cadets. I believe the typical session was only three or four months. It wasn't as long as the nine months--

Frenzen: It was added on to the nine months for forecasters who were headed for the tropics. Who were some of the other faculty members down there?

Fultz: ... a New Zealander who really was part of establishing it was Clarence Palmer.

Frenzen: Of the Palmer Index?

Fultz: That's a different Palmer. The other most important faculty member there was Herbert Riehl. He stayed longer than I did, and a number of his early miscellaneous report papers came out of that.

Frenzen: As eventually did his book.

Fultz: Yes, his books on tropical meteorology.

Frenzen: But then you came back to the campus here and worked as a research assistant.

Fultz: What actually happened was, from June 1942 to June 1943, I was working primarily on CAV trajectories, and a good deal of that was after the district office moved out to Midway Airport. I worked alongside the regular forecast shifts to see how successfully those trajectories could be used in one and two-day forecasting.

Frenzen: Projecting air mass movement?

Fultz: Right. That was using primarily 10,000 foot data, and we had a fair amount of data over the oceans so that it was more nearly hemispheric than had been possible before the war. That was at least ten or eleven months and the material for that is what I wrote up as a research assistant in 1943-44; it became Miscellaneous Report 19

Frenzen: Yes, the nascent department had a series of technical reports called "Miscellaneous Reports" numbered so-and-so, and yours was number 19.

Were some of the forecasters you worked with out at Midway familiar names to us? We're in a later generation of them, I think, here.

Fultz: The meteorologist-in-charge originally was Gordon Dunn, but he was away for a good deal of the year, first at the Institute of Tropical Meteorology, working there since he wrote one of the pioneering papers on tropical circulation models. Then, he spent a lot of time essentially consulting in the China-Burma-India theater for a good fraction of the war. I don't recall who became the meteorologist-in-charge after he left.

Frenzen: It wasn't Joe Fulks, who was here for a long time.

Fultz: Joe, I think, came just after the war. The other forecasters, one of them was an old Navy man named Downs. Another was Jacobson...I remember one story that Downs used to tell which, for the time then, was a really remarkable forecast. He made a forecast of a major cold outbreak from Canada, essentially twenty-four hours ahead, which was a very tough call, but led to a cold wave which had an average speed of sixty miles an hour from the Canadian border on down past Iowa. Unfortunately, it caught and froze quite a number of hunters, primarily due to communication problems. If it had been distributed as well as it was made, it would have been enormously effective.

Frenzen: Eventually you did some instructing in those army courses, is that right?

Fultz: Only a minor amount. I gave lectures a couple of times to classes on CAVT's, but that was about it. As a research assistant, I was mainly working up the material from the year out at Midway.

Frenzen: You didn't work with anyone in particular on the faculty at that time? You were really more or less on your own. We're getting close to the modern era, getting to the hydrodynamics laboratory. Although I see here you worked in Asheville?

Fultz: Yes. I had been called up for the draft a couple of times and was rejected both times for a heart murmur. That's why I was here instead of in the Army. But Rossby got me into what amounted to a consultant position in the operation analysis section of the Air Corps, and that was headquartered down in Asheville. So after some breaking in down there, in which for example I worked on some of the Japanese balloon reports, I was sent out to the Twentieth Air Force Base at Guam.

Frenzen: The trans-Pacific balloon? Were you trying to work backwards?

Fultz: I don't really remember what the question was that we worked on, but we had the data that was available. They never did any particular damage...

Frenzen: They killed two picnickers, as I recall. Started some small fires. But the question must have been where they were coming from? The home islands or from ships?

You spent some time visiting Guam and the Marianas?

Fultz: I was with--essentially attached to the Twentieth Air Force forecasting section, who were doing high-level forecasting for the B-29 planes. I worked in Guam, got there about a month or six weeks before the Hiroshima bomb. That occurred while I was there in Guam.

Frenzen: Did you participate in any of the arguments as to who really discovered the jet stream?

Fultz: No, I didn't. It hadn't been discovered yet.

Frenzen: The stories I've heard--the high-altitude forecast group in England, forecasting for the European theater, felt they knew about it all along. Whereas the people in the Pacific said they discovered it because they found some B-29's flying backwards.

Fultz: Yes. I've heard that, and I don't know what the correct story is.

Frenzen: A subject for future research.

Fultz: But it certainly wasn't singled out as a major research topic until after the war by Rossby.

Frenzen: After the war, you came back to Chicago, and by that time, a department had begun as opposed to the Army course within the physics department?

Fultz: It was an institute of meteorology right up to the time it became a department which would mean its status would be comparable to the Oriental Institute except for lack of endowment, relative to academic departments.

Frenzen: Rossby was the first head then of the department?

Fultz: Right.

Frenzen: Early on he interested you in fluid dynamic models, or how did that come about? It must have been about that time, 1946.

Fultz: No, it was built quite a bit earlier. I'm trying to remember if I had any contact with it.

Frenzen: That's right. You told me that someone had built it as a demonstration lab.

Fultz: It was run by the astronomer, John Phillips, who I mentioned, all through the war. It was used as a demonstration lab for the courses, and among other things, Mike Ference went out to visit a couple of major hydraulics labs, particularly the one at the State University of Iowa in Iowa City, which was run by Hunter Rouse, and had a tremendously well-thought-out teaching, in addition to research, lab so they constructed things considerably resembling teaching experiments that were in the works out there. Particularly one of them was a Reynolds tube experiment demonstrating onset of turbulence in a pipe, measuring the corresponding discharges and Reynolds numbers.

Frenzen: There were no rotating systems though.

Fultz: No. I'm not quite sure whether the flume was used in teaching, but it was built right at the beginning, so it probably was for some purpose, maybe demonstrating hydraulic jumps or something like that.

Frenzen: I remember it was used for a lot of sedimentation transport.

Fultz: Right. That was later though. It must have been used for open-channel, to study open-channel flow. There was also a Reynolds pipe experiment set up so that it could measure the pressure gradient.

Frenzen: I think I remember that, alongside the flume.

Fultz: There was a Hele-Shaw apparatus that they used, I think, for demonstrating irrotational flow.

Frenzen: Flow past obstacles and cylinders.

Fultz: Right.

Frenzen: The first rotating experiments in the hydro lab then--

Fultz: The main research experiment was a rectangular glass tank that John Phillips built. That was intended to study thermal convection in a two-dimensional sort of situation. The principal purpose of that was to investigate the type of theoretical situation discussed by J. W. Sandstrom in the 1900's and 1910's, in Sweden. And also [been discussed] by Harold Jeffreys in which Sandstrom essentially pointed out in a qualitative way that in order to have a vigorous thermal convective system, on the average the heat gains must occur at higher pressures than the heat losses. So that if you have fairly concentrated thermal sources, the hot source must be located at a level lower than the cold source.

Frenzen: Was that apparatus inside the constant temperature room...?

Fultz: It had to be in a controlled-atmosphere room which was done in a pretty effective way by maintaining a temperature higher than the one in the room just with electric heaters controlled by a thermostat which could be set to about 1/100th of a degree. I think it was simply a very large mercury thermometer with a traveling contact so that you could set the level by making electrical contact through the mercury.

Frenzen: And this went on during the war years.

Fultz: Right. I don't really know when it was started, but John Phillips built it sometime during the war. And I got into the lab there, and I don't really remember how, but I was working for John Phillips on that convection tank.

Frenzen: I'm eager to learn when the rotation experiments began since they are the main feature of the hydro lab in my memory.

Of course, Rossby did some early experiments of that kind at MIT.

Fultz: No, not at MIT. It was before MIT, in fact. I believe it was about 1927-28 when Rossby was in some sort of advisory capacity to the chief of the Weather Bureau at Central Office.

Frenzen: He found space to build a rotating table?

Fultz: Somewhere down in the basement at 22nd or 24th and M is where it must have been.

Frenzen: Is that the apparatus that's in the photograph you have downstairs on the wall?

Fultz: Right.

Frenzen: Rossby looks very young there.

Did [he] ever publish any of these early results?

Fultz: Nothing particularly extensive about the actual experiments, but some of the background he laid out in two papers in 1926 and 1928.

Frenzen: And then did he suggest that experiments of this kind could be done in the hydro lab in Chicago?

Fultz: In essence, yes. It was after the war when the general circulation project was well underway one or two years. The actual start of it was that Rossby had Victor Starr in his office. He called me in and the two of them suggested that I might try to set up something that could produce essentially turbulent convective mixing in the spherical shell in connection with his theoretical idea that turbulent mixing over the polar cap could be a mechanism for producing absolute vorticity. Flux is toward the average latitude of the jet, so in essence the motivation was to see what would happen in the way of any sort of mean circulations if you have a rotating shell of fluid and produce convective mixing between the pole and the equator.

The way I set it up was to cut a couple of chemical boiling flasks. I think the two sizes were three liters, two liters--something like that.

Frenzen: I, of course, remember that very well. You're holding a copy of your paper which was published in **Tellus**, was it?

Fultz: No, this was a review paper in **Advances in Geophysics**, the Academic Press, in 1961.

Frenzen: It has all these details and it has these illustrations of early historic experiments and hits some of the early hydro lab experiments. You started with a harder experiment, the hemispherical shell; when did someone realize you could flatten it out and do it in a dishpan, some of it?

Fultz: I think we probably realized--the actual order is a little bit hazy. Some of these things I found out later, but fairly early...I think the first one I knew about was Exner's experiment in the twenties, which Oliver Wulf drew our attention to. Then I probably found out about the ten after the dishpan experiments were already running.

Frenzen: Someone built an early dishpan who was in the lab when I joined in 1951 which actually ran on rubber casters.

Fultz: That was the first one. That was built by Ferguson Hall who intended to look into the hurricane question by rotating the pan with two layers in it and heating it with a Bunsen burner at the center.

Frenzen: That's more or less what Nakagawa was doing...there were experiments going on like that in Tokyo immediately after the war.

Fultz: The order really didn't go one-two-three, so we'll have to jump around a bit by types of experiments. The hemispherical shell experiments that Rossby and Starr suggested were the first from our point of view, and they were successful in the following sense within a month or two of the first one. What we did was rotate an assembly of two chemical boiling flasks that had been cut off on the equator with a heater arranged in the neck of the larger one, and with no particular provision for cooling so the working fluid actually warmed up fairly rapidly in the course of the experiment. But in general, there was a fairly substantial cold source associated with evaporation at the opening surface on the equator of the fluid shell, and with conduction through the glass both in the inner and the outer shell. Now that was a rather unrealistic sort of thermal distribution, but what turned up almost immediately was that though the intensity was sufficient to produce rather small-scale turbulent flows, eddies of the order of just a few centimeters in size became immediately obvious when you put ink in.

This zonal motion was discovered as soon as we had successive photographs of tracer, which in the first instance was simply injected from a hypodermic needle rotating with the shell. There were systematic motions such that the equatorial low latitudes were lagging behind the absolute clockwise rotation of the shell. We happened to rotate it clockwise as seen from a borea.

Frenzen: So you had easterlies.

Fultz: There were easterlies in low latitudes and substantially greater westerlies toward the pole. With those magnitudes, when measured in units of the speed in inertial coordinates at the equator of the shell, just of the order of magnitude that applies to characteristic zonal flows in the troposphere. In the particular example that's in this review article, the average easterlies were about one-and-a-half percent and the average westerlies somewhere near fifty or sixty. Degrees latitude were about three-and-a-half percent and those are just in the ballpark of a randomly picked zonal flow in the mid-troposphere.

Frenzen: So that was a promising result.

Fultz: That was in fact the result that I took as one that suggested it was likely to be very fruitful to follow it up. As it became clearer and clearer as we found out more about what had gone before in the way of experiment, attempts to do this sort of experiment. It finally turned out that they went back almost 150 years.

Frenzen: I have to ask, because that's one of the things the project is interested in: the early reception within the departments, within the meteorology community of these experiments--were they immediately accepted as successful, or were they viewed as an oddity or perhaps as a waste of time by anyone?

Fultz: Well, the spherical shell ones, it's a little hard to say. I would say they were probably accepted as interesting but cautiously.

Frenzen: And this was all within the general circulation project.

Fultz: That's right.

Frenzen: Did you seek independent funding, or did anyone seek independent funding?

Fultz: Not at that point. In fact, before we got started on the shell experiments, the convection tank of John Phillips was supported by a very small grant from the Weather Bureau. As I recall, I don't know that I was paid anything when I was working on those.

Frenzen: If Ferguson Hall began the first dishpan experiments at Chicago, when did he join the hydro lab?

Fultz: He was not a member of the hydro lab. He was sort of an administrative assistant in the departmental office if I recall, and devised the first dishpan experiments, as I said, as a hurricane model, just a side interest of his own.

Frenzen: But not in the hydro lab itself.

Fultz: That's right.

Frenzen: In the radiosonde lab.

Fultz: Right.

[This is a continuation of the interview with Dave Fultz. Almost to the end of Tape 1, Side

2.]

Frenzen: If Ferguson Hall began the first dishpan experiments at Chicago, when did he join the Hydro Lab?

Fultz: He was not a member of the Hydro Lab. He was sort of an administrative assistant in the departmental office, if I recall, and devised the first dishpan experiments, as I said, as a hurricane model, just a side interest of his own.

Frenzen: But not in the Hydro Lab itself.

Fultz: That's right.

Frenzen: In the radiosonde lab.

Fultz: Right. Now, the sequence of events was that, for reasons that became clear much later, he couldn't get any very intense vortex out of the way he was heating with the Bunsen burner in the center of the standard dishpan with a layer of water in it. But he had some aluminum powder tracer on the surface of the pan, and showed me on one occasion, the sorts of turbulent flow fields that he was getting. I was interested in the sort of scale that they were showing of eddies that seemed to be the order of magnitude of a few centimeters in characteristic size. Shortly thereafter, Ferguson left the department to go, I think, to the Weather Bureau in Washington, but I'm not certain of that. He left the apparatus behind. We moved it over to the Hydro Lab, planning to try it at some point and I don't recall exactly when we did that, but what we did was set it up essentially the way he had it, running on casters and move the Bunsen burner out under the rim so that we had the case of a heated periphery and relative cooling at the center corresponding, if it would turn out to be an atmospheric model, to a flattened-out hemisphere of air. And the thing that struck me that led us to expand those fairly substantially during the year I was in England was that we really couldn't tell too much about flow patterns, but you could tell that some of the most obvious jet-like flow structures that were proceeding either toward the center or toward the outside with the pan rotating counter-clockwise were turning relative to the radial line in the opposite sense that you would expect from the Coriolis force, because they were bending to the left going in and to the left coming out. That was about all we could see about them, and not everything was doing it, but some of the most visible streams were, and so when we had it set up in such a way as to be able to take a streak photograph (we must have done it probably with a rotoscope, although it could have been by rotating the camera)--

Frenzen: The rotating prism technique. I was wondering when that was introduced.

Fultz: I believe that was first used on the spherical-shell obstacle experiments, but we put it onto the dishpan almost immediately. Some of the earliest streak photographs taken in that way of rim-heated dishpans were effectively highly vertically unstable. So there was substantial normal convection in the atmospheric sense occurring, and this produced eddies on scales on the order of two to three centimeters as the dominant thing showing.

Frenzen: But still not very realistic as a hemispheric model.

Fultz: Right.

END OF TAPE 1, SIDE 2

Interview with Dave Fultz

TAPE 2, SIDE 1

Frenzen: We are continuing the interview with Dave Fultz at the University of Chicago on November 19, 1992.

Let's say a few words about the personnel of the Hydro Lab, which by the middle forties was a going concern. In 1948, I believe, Walt Bohan joined as a research assistant.

Fultz: Yes, he was the first and very soon after that, George Owens joined and I think in the autumn of 1949, the first graduate student worked there, Bob Long.

Frenzen: I remember them all, of course, and Walt Bohan is well known. When he did leave the lab, I remember he went to Cook Electric, John Bellamy's company up in the northern suburbs, and a few years after that, he established his own firm and became very well-known in satellite photo interpretation and making movies out of the early satellite photographs. Today, of course, he's in forensic meteorology. There are too many people in the satellite photo-reduction business. I believe I've lost track of George Owens. I don't know what happened to him, if he stayed in meteorology or not.

Fultz: I'm not even sure of that. Somehow I don't think so.

Frenzen: Bob Long, of course, went on, was quite successful, became a professor of fluid dynamics at Johns Hopkins. I used to see him at turbulence meetings now and then, but not in recent years. I gather he must have retired by now.

Fultz: I think that's very likely.

Frenzen: You and I of course will never retire.

Then, along about 1950, you were awarded a Guggenheim, which took you away from the lab from August, 1950. When I joined the lab shortly after I got my Master's degree in June, 1951, you were still away, but you came back in August of that same year.

When Bob Long joined, they must have started the spherical-obstacle experiments because that's what he was doing at some time in there.

Fultz: Yes, that's right. In fact, the initial idea for the circular obstacle experiments, which were the first, was his.

Frenzen: It was a cylindrical obstacle supported between the two shells of a rotating hemisphere.

Fultz: Right. What led up to that and what he started initially working on were the polar vortex experiments, which were two-layer experiments that followed on from the original heating experiments.

Frenzen: Where they heated the equator.

Fultz: The general idea in those was to investigate the stability of an anti-cyclonic circulation at the pole--it was one of the ideas that Rossby had. Rotating a controlled obstacle instead of the two-layer thing was Bob Long's idea.

Frenzen: I remember that the first ones were eccentric--they were actually supported, they were off-center cylinders supported at the pole.

Fultz: At mid-latitudes, supported on a--

Frenzen: But that was later. Weren't there some earlier ones that actually were a whole obstacle. Cross-latitudes: I think what happened is, didn't the obstacle kind of move away...as the experiments progressed, or was that a later trial? There were some--because you couldn't have jumped from a polar vortex to a mid-latitude obstacle.

Fultz: Well, I think that's what happened. I think what you described sounds to me like the two-layer experiments where there was a fan-type thing on the inner shaft that was run at a different speed than the spherical shells themselves so as to produce an anticyclone.

Frenzen: With a dense liquid?

Fultz: Right. A rather small difference of 1% or 2%.

Frenzen: I think I only saw photographs of it, and I may have assumed that it was a rigid device mounted at the pole. However, the obstacle experiments were the thing that I started working on very early. There was difficulty in getting usable photographs of streamlines, so we spent a lot of time working on different tracer materials, all of which were summarized in one of the Hydro Lab's major annual reports in the mid-fifties. But that's much of the activity while you were away, except at that time Walt Bohan was working on the dishpan and was building cylinders which were heated at the rim.

Fultz: I don't recall--well, we could have been building cylinders that early.

Frenzen: But we abandoned the aluminum dishpan and there were some of them around when I joined in 1951. I remember I was helping him make glass cylinders at that point.

Fultz: I think those were mainly for the rotating ______________ experiments, not for the dishpan.

Frenzen: Then that is several years later.

Fultz: Because the second type of dishpan we really kept using with various modifications. We had one, for example, with an interior cylindrical aluminum wall cemented to the pan.

Frenzen: Polar cold source.

Fultz: Right.

Frenzen: Sink.

Fultz: And a number of other modifications. We kept using those really through a fair amount of the fifties. The big three-dimensional experiment on the three-wave convection that Herbert Riehl analyzed with me was in that type of pan still.

Frenzen: Those were in later years.

Well, tell us about the Guggenheim. I've never heard much about that.

Fultz: As a little bit to start with, a lot of the original dishpan discoveries were in 1950 before I left on Guggenheim, and then some of the major ones were made while I was away when Bob Long and Walt Bohan were running the experiments. I found the actual notes that mention both Exner's experiment and Rossby's experiment as part of the motivation for doing those, together with what I had seen in Ferguson Hall's experiments. Rossby's experiments, I find, date to the spring of 1927. I had a note on that.

Frenzen: In the basement of the Weather Bureau.

Fultz: Right. The central office of the Weather Bureau. But we moved Ferguson Hall's dishpan from the radiosonde lab over to Rosenwald, and the first significant trial of it I did in the evening of the second of March, 1950, with just a Bunsen burner at the rim, and my note doesn't say how fast it was rotating. But what turned up was the same sort of thing that the spherical heating experiments showed--namely that there seemed to be, in spite of the rather small scale of the individual eddies, definite zonal drift, which was about 2% easterly at about mid-radius, and 1-3% westerly out at the rim. So that was a very striking thing to do; it excited Rossby immediately. He came over for an evening demonstration about five days later.

Frenzen: The problem was really you were not in the right regime of rotation rate and/or heating rate, to see the fully developed jet and wave pattern.

Fultz: Right. And it was probably too unstable vertically.

Frenzen: Not enough radial heat.

Fultz: The radius of deformation would have been pretty small with very slight stratification. And it was in that same week that in attempting to trace things, we poured some ink into a heated dishpan and ended up with Taylor inkwalls, which eventually got connected with Taylor's slow-motion rotating experiments, which I had started reading. The first note I find on that is January, 1950.

Frenzen: These are rotation-dominated.

Fultz: Right. Taylor's experiments were all uniform density, driven mechanically in one way or another.

Frenzen: Concentric cylinders.

Fultz: No, this is not his famous double-cylinder experiment. Those followed just about the same time, 1919, early 1920s. But these were the experiments, for example, in which he rotated a small rectangular tank and translated a small sphere across the axis on a track and showed that when you move it very slowly, what I later estimated would have been Rossby numbers of about 1%, that the flow around the sphere was essentially barotropic and two-dimensional, and carried the cylinder with it.

Frenzen: I've seen photos of that.

Fultz: That was the crucial connection during that Guggenheim year I finally realized was connected with the thermal wind equation and quasi-geostrophic balance in the atmosphere. We started using Rossby numbers. Bob Long was using Rossby numbers before we knew what they were.

Frenzen: I remember that you, it was later, I guess, that you christened another number, the Taylor number. Isn't that true?

Fultz: I think that was Chandrasekar who did that.

Frenzen: I remember a letter to Taylor asking if we could call it that. Maybe it was a copy of a letter to Chandrasekar. He asked that we wait until he was dead. I couldn't have made that up.

Fultz: I don't really recall that. We didn't get involved in the double-cylinder business until quite a bit later when Chandrasekar suggested that we put one together.

Frenzen: Back in 1950, you saw the Taylor walls.

Fultz: As I say, it took us about a year to realize what we were really seeing.

Frenzen: Are we at the Guggenheim departure yet?

Fultz: --that spring, dishpans really got started and Rossby got very excited about them, and I have lots of notes of discussions with Palmén and Starr and Rossby, who were all there during that time about various aspects of that.

Frenzen: Palmén used to come for the summer--

Fultz: He was there during the academic year at least some of the time. That particular year he was around both in the winter and the spring.

Frenzen: As you go on down your notes there...

Fultz: Well, the important discovery that Bob Long and Walt made while I was away was that there was a general transition between regular symmetric flows and high Rossby number conditions and the wavy irregular Rossby regime flows in the open dishpan for much lower Rossby numbers. I don't recall whether we looked for that at my suggestion or at Bob's, or both. But I had the idea in August, when I was discussing things with Shepherd and Eady and Scorer and John Mason about what was going on in the experiment, Eady suggested looking at Richardson numbers, since that was the parameter that arises in his baroclinic instability problem.

Frenzen: This then was in the first month of your Guggenheim at Cambridge?

Fultz: In London. I passed through London and went to visit Imperial College. After our discussion, I estimated Richardson numbers at about twenty for one of the dishpan experiments where we had some data and that was in the ballpark for the mid-latitude in the atmosphere, too, troposphere. My note says it's important to look at the dishpan experiments at very low rotation rates in order to see whether they go to a radially-symmetric flow which was later the Hadley flow, when it was discovered to be present.

Frenzen: Well, that should have put Walt and Bob on to that path. You must have done a lot of reading; didn't your paper on the non-dimensional analysis come out of the Guggenheim?

Fultz: Yes. We were living on the grounds of a Carmelite convent in the caretakers' house upstairs in Waterbeach, which is a village outside of Cambridge. There was no heat in the upstairs except a fireplace, so I was typing on that paper in the bedroom and would have to go into the living room to warm my hands every once in awhile.

Frenzen: Wear gloves without fingers and a hood like Erasmus.

Fultz: That really was the paper in which it became clear to me what the relation was between Taylor's slow motion experiments and quasi-geostrophic flow in the atmosphere. Taylor's experiments were simply the case of zero thermal winds.

Frenzen: There were some exciting people at Cambridge in those days, I imagine. G. I. Taylor, was he very active?

Fultz: Yes, he was still very active for about ten or fifteen years, in fact. He for many years was a research professor in the Royal Society at Cambridge, and was given space in the Cavendish lab by Bragg, who was the Cavendish professor, so he could do what he wanted--he didn't have to teach. But he had research students, maybe not in the formal way, but he'd talk to them. And in particular, two who came just after the war were Allen Townsend and George Batchelor, both who of whom stayed on after taking their degrees.

Frenzen: Well-known name in turbulence. Townsend did a lot of turbulence work, too.

Fultz: He did, and some very skilled measurements on thermal convective turbulence.

Frenzen: And turbulent shear flow. He did his book on--

Fultz: Most of it is on turbulent shear flow, but he did some experiments, I think it was the second year I was there that he was doing those on the flow above a heated plate.

Frenzen: Who did you interact most with at Cambridge, as a visitor?

Fultz: It's a little hard to say. I attended several lectures, including one class that Batchelor was giving on general fluid mechanics. But I suppose I'd see G. I. every few days, when he'd come into the lab. Otherwise, I suspect most of the interaction was with Batchelor, and to a lesser extent with Townsend.

Frenzen: You probably had an opportunity to continue your historical studies at Cambridge with their library facilities.

Fultz: Yes, I did a fair amount there in finishing up the 1951 compendium of meteorology paper, and in fact, I found quite a number of references on trips to several different centers on the continent that I took in winter and spring at Frankfurt and Hamburg and Freiburg.

Frenzen: Who were some of the people you met at these places?

Fultz: I deliberately went to visit Berg at Cologne, and Mügge at Frankfurt, and Paul Racthjen at Hamburg. Racthjen in fact was the only one who himself was interested in actually doing experiments, and later he in fact built a rather large type of dishpan at Hamburg, and did a fair number of experiments there. The man at Freiburg was Görtler, who was a theorist but had triggered some homogeneous rotating fluid experiments with small oscillatory excitations that--

Frenzen: Isn't there a Görtler wave or a Görtler vortex?

Fultz: He worked on curved flows and so the type of instability that you get in the boundary layer on a concave surface is called Taylor-Görtler instability, because it's basically the same instability that occurs in the Taylor double cylinder classical experiments.

Before I left Cambridge to travel around in the winter, I had come in contact with a research student, Raymond Hide, who was planning to do a thesis with Keith Runcorn, who was in the Department of Geodesy and Geophysics, and was interested in the Earth's magnetic field. They, in fact, were already setting up in October, 1950, a convection apparatus between two concentric cylinders, planning to produce convection in the annulus between by having a heated bath on the outside and a cool bath on the inside.

Frenzen: This was in the Cavendish lab?

Fultz: No, it was set up at the Observatory, which is outside Cambridge, set up in what they called the Pendulum Room, for what reason I don't know. But we do have a picture of it down in the corridor which I took on that trip. By January, before I left--

We'd better go back to the historical paper because the important thing I'd picked up in doing that was the existence of papers by Vetten, in the 1850s and 1880s, who had done rotating disc experiments in air and in fact it produced Hadley circulations according to his figures, so that immediately became part of our object in surveying the dishpan experiments Bob Long was doing. By March--

Frenzen: Of course, in those days you were communicating by air letter, not fax or e-mail, so things were a little slowed down. But I imagine--

Fultz: By December, in fact, Bob had already run off an air test and established that you get a Hadley circulation very easily there and very shortly later he had done the same thing with water experiments. In other words, establishing it at especially low-range rotations with the typical heat sources we were using can get a Hadley-symmetrical circulation. That was also connected with our gradually beginning to understand what the significance of the Rossby number was.

Frenzen: The illustrations of these old Vetten papers--I remember them, they're reproduced in--

Fultz: In the monograph, and also in the **Advances in Geophysics** survey.

Frenzen: Ray Hide, of course, came out to the Hydro Lab and spent some time in Chicago. Was that just a short visit?

Fultz: Well, that was quite a bit later. It was a visit of at least a year. He came to visit the Hydro Lab a bit, but he was mainly working with Chandra at Yerkes.

Frenzen: Yerkes Observatory, about fifty miles north of Chicago.
What were Ray Hide's experiments like down there in the Pendulum Room?

Fultz: It turned out almost immediately that they were much more narrowly meteorological than convection in the earth's core, because he found what later turned out to be under low Rossby number conditions, a series of wave motions which were extremely regular compared to anything that occurs in open dishpans, and had regular regions of occurrence, so that in one region you could have a wave number one, then wave number two, then wave number three, then wave number four, five and so on in addition to symmetrical Hadley experiments. And he found rather rapidly that these waves occurred on a flow that concentrated in a well-defined jet at the top surface, and aluminum powder would immediately collect in the jet so that you could tell the nature of the flow very easily. Then the combination of finding that the general relative flows in the annulus were running pretty much proportional to the radial temperature difference which makes them correspond in essence to the thermal wind that's imposed across middle latitudes. A combination of all those things convinced me by March that I'd decided we'd abandon some of the things we'd intended to do and set up an annulus piece of apparatus right away.

Frenzen: The annulus gives you both the geometrical boundary and a regular thermal boundary in that you have both a heat and cold source.

Fultz: Right. By this time, of course, we were providing cold sources at the base in the open-center dishpans. But the results are not as regular as they are in a pair of concentric cylinders, and the essence of the matter is that the presence of the extra boundary essentially cleans up the spectrum of responses by the fluid to the heating field, so instead of getting broad-spectrum merely turbulent-like results, you have almost single wave number responses over a considerable part.

Frenzen: On the other hand, the open center of a surface cold source is more realistic for the atmosphere.

Fultz: Yes, but a much more difficult problem to solve than any single wave number problem, plus a much more difficult thing to make experimental measurements in, as shown by the work we did very much later on a three wave annulus experiment in which we were able to make three-dimensional temperature and indirect velocity determinations in the interior of the fluid by using a three-dimensional set of temperature measurements that were obtained by moving a single thermo-couple detector to a whole series of meridian point positions, taking a trace as the waves there move past in the steady-wave train so that the time variation at the particular R-Z position could then be converted into the longitude variation, and the whole three-dimensional temperature field deduced.

Frenzen: Because the whole pattern was that steady.

Fultz: That was something that just couldn't be done in the irregular flows.

Frenzen: Didn't Hide see the vacillation cycles, so-called, between wave numbers?

Fultz: Yes, that turned up within the first two or three months. I have notes I took on one experiment that was a clear vacillation case. Both steady waves and this very regular pulsation of an otherwise steady wave train were new phenomena that were not clearly present in the low Rossby number open-center experiments. In fact, were not observed until very much later in the open-center cases. One case, by Hide, forced some internal heating experiments in the seventies, and in our case by Tom Spence's open-center experiments, which turned up with the same sort of steady and vacillating-wave responses as occur in annuli when internal heating associated with photographic light were applied.

Frenzen: I've always been fascinated with the relationship between the vacillation cycle or possible relationship and the index cycle and the atmosphere.

Fultz: That's an unsettled question, still. It's not even clear to what extent there are distinct differences in kind among the possible vacillation states. There are some that are obviously qualitatively different. As in our annulus experiments, there were considerable regions where there was a type of vacillation involving wave number change from three to two to symmetric flow to three to two to symmetric and so on.

That's obviously different than a single wave number going through amplitude variations.

During the same fall, Bob Long was starting up his spherical-obstacle experiments, rotating a circular obstacle in mid-latitudes, and by early November, he had shown that in the case where the relative flow past the obstacle is westerly in sense, that there were some large-scale wave motions excited, and he had identified those with the Rossby-Haurwitz modes.

Frenzen: This was the fall of 1950.

Fultz: Yes, so that's a barotropic case that was, I think, the first one to establish an experimental instance of a significant meteorological-theoretical case.

Frenzen: You mean the direct connection between theory and laboratory experiment.

Fultz: Right.

Frenzen: That became his Ph.D dissertation.

Fultz: Yes, right.

Frenzen: I helped him with the photos and so forth when I joined about six months later.

Fultz: Another thing happened during the same fall that's connected with the convection experiments. That was a rotating flame experiment that has an interesting history because it was directly suggested by what was turned up in my historical surveys. Namely, by this time I had read James Thomson's Bakerian Lecture in 1892. There, he had summarized Hadley's idea of what drove the trade winds which essentially it was produced in some manner by the fact that the zone of maximum heating by the sun was in relative motion toward the west, and he specifically suggested that somebody put that to an experimental test...

Frenzen: A rotating hot spot under the sun as the earth rotated...

Fultz: Nobody had done this in the 200 years between 1686 and 1892, and in addition, Thomson, in another place in the same lecture, specifically suggested what we had begun to do as dishpan experiments, as something worth doing. And again, nobody had followed it up. But by December, Bob Long had verified that moving a Bunsen burner under the rim of a stationary dishpan did produce a systematic vortex motion which in fact was anti-flame at smaller radii at the top surface.

Frenzen: The fluid tended to rotate in the opposite direction to the motion of the heat source.

Fultz: In the upper layers. Something else must be going on down at the bottom of the pan. But we never had very accurate instruments that--the sort of intriguing thing about that is that it's not very probable that that's a significant mechanism in the atmosphere, but if it were, it would correspond correctly to the upper westerlies in the upper troposphere of middle latitudes. Those are anti-flame compared to the--
Another place I visited in the spring of 1951 was Göttingen, which I intended to go to in order to meet Ludwig Prandtl. When I got there, he was away on vacation, but I spent a number of days at the Institute, which was originally the *Aerodynamische Versuchsanstalt*. At the time, a good deal of it had been dismantled after the war and Prandtl had been forbidden to work on aerodynamics anymore, which was one reason he was doing a certain amount of meteorological work at the time.

Frenzen: How old was he then?

Fultz: He would have been early seventies, I think. But I had a number of discussions with people at the Institute, one of whom was working on double cylinder experiments. I looked at a lot of instruments [and] had a number of discussions with Ernst Kleinschmitt, Jr., who was the meteorologist on board at Prandtl's Institute, and with Horst Merbt, who later went to Stockholm. He was in the Army Weather Service of the German Army during the war.

The thing of most interest to me was that Prandtl had a rotating room, about ten feet across, which they had built in the twenties. And had run some dishpan experiments, but not obtained any very definite patterns, so they had given them up, and one of the things we did while I was there was [to] take a pan about a yard across and arrange some heating for it--I forget how we did that. But we were able to verify the sort of Rossby regime flows that we were getting in the lab at Chicago.

Frenzen: In this arrangement, the observers sat in the rotating room. I remember stories about it, confining his head so that he couldn't move it otherwise he would get sick.

Fultz: About the only things they used it for were demonstrating Coriolis forces for visitors. The story was that if a visitor was not very welcome, they'd take him in, throw a ball, and in trying to follow it, he would disturb his balance sufficiently to get sick.

END OF TAPE 2, SIDE 1

Interview with Dave Fultz

TAPE 2, SIDE 2

Frenzen: Did you then finally get a chance to talk to Prandtl?

Fultz: Yes, on two to three occasions, about the dishpan experiments and a number of things that he was interested in.

I always think of him with small-scale motions...

One thing he was interested in that is still interesting but we never followed it up--it would be quite difficult to solve this problem--how, when you swing a glass of water in the proper way, you can generate within a couple of seconds a very intense vortex.

Frenzen: You had something like that here at a Christmas party once.

Fultz: As far as I'm concerned, nobody has ever come up with a plausible mechanism for that. It also goes back to a possibly apocryphal story about Laplace, who said, shown that somebody was emoting about how everything was predictable. But Prandtl thought it required wave breaking at the surface in order to fold over and get circulation into the interior of the fluid, a long discontinuity [of] surfaces. But that definitely is not necessary.

Frenzen: No, but the _________________ you had here, it was a toy, wasn't it, a Christmas toy? It was a tornado, I remember it, and it was sealed. Problem for the future.

Your travels took you elsewhere in Europe as well.

Fultz: Quite a number of places during that spring and summer. I went to Freiburg to talk to Gőrtler; I think I mentioned that. And then back through Frankfurt where I spent quite a bit of time with ____________________, who was then mayor. It was before he moved over to this country. And in May, I went back down to France, through Paris, where Paul Queney was back from his vacation, and on to Grenoble, where there was a very impressive hydraulic laboratory called the Etabl. Neyrpie. I forget the name of the man who started it--Paul Danel, or something like that. But that's a commercial laboratory that's practically the equivalent of a fluid mechanics university. They had a number of research projects going on in conjunction with the University of Grenoble. John Macnown from State University of Iowa was there for a year working on harbor oscillations and some very interesting stratified fluid experiments were being done by a man named Gariel. Essentially on mountain wave-type questions in a tank or a flume. This sort of thing Bob Long later did at Johns Hopkins. Then I went to the University of Toulouse, where there was again a well-known hydraulics laboratory. They had some very interesting discussions, although they were not particularly meteorological, with the professor L. Escande, and his assistant, Nougaro. Then, at the end, I was back in Cambridge and getting in touch with Hide again, at which time my notes show that he had a very good Hadley symmetric case going, which was earlier a question as to whether he could get it. And then both the family and I traveled to Stockholm early in June where we had a long series of discussions with a lot of people at Rossby's institute--general circulation questions with Rossby. Arnt Eliassen was there, Earl Droessler, Dan Rex, Ernst Kleinschmtt, Jr., and quite a number of others including Tor Bergeron. That was primarily about dishpan, and Taylor-type experiments.

Then, about the twelfth of June, the Rossbys and the Palméns took us and the Rexes on his standard trip to Visby in Gotland.

Frenzen: Any significance to this--?

Fultz: --all sorts of castles. Marty's question was, "Has anybody ruined this place?"

Frenzen: Marty, your daughter, who was how old?

Fultz: 1951--she would have been four or five.

We got back about the middle of June and still sitting around talking with a lot of people. Among others, Gustav Arrhenius was involved and was talking a great deal of some of the first ocean coring results which he was working on at the time. He's the grandson of Svante Arrhenius, and his father was the one who got Rossby into meteorology, apparently. At least that's what Rossby said.

So we were bouncing a lot of general circulation-jet mechanism questions around with people at Rossby's institute, and I got a chance to look at Alfvén's institute, which was just getting started. Then with some people, N. Herlofson and B. Lehnert, with some big magnets and experiments in mercury and other such things, including--I'm pretty certain they had at least one small vacuum chamber to do experiments like the terrella experiments that Birkeland did in the 1890s.

Frenzen: For the Northern Lights.

Fultz: Yes.

Frenzen: The experiments, they didn't really lead to what we finally did for over-stability, but the techniques--the idea for that experiment didn't come from there.

Fultz: No.

Frenzen: But later on. Did we learn from Alfvén, didn't you tell us they worked these experiments with heated mercury which potentially were quite dangerous, were toxic?

Fultz: They weren't heating it at the time.

Frenzen: But every shelf space in the lab was covered with particles of zinc, or did I hear that from somebody else?

Fultz: That would be somebody else, I think.

Frenzen: And if they got sick, they went skiing, drank a lot of milk and went skiing? From mercury vapor.

Fultz: No, I hadn't heard that.

Frenzen: Of course, Alfvén was given a Nobel prize for some of that work.

Fultz: Right. At that time, it was just when he was writing his first theoretical papers on Alfvén waves in plasmas.

Frenzen: And all of this was in the spring of 1951?

Fultz: June. So it's getting along towards summer. Then in July, I was back in Cambridge where Hide was getting a lot of results so I spent a fair amount of time with him. A couple of long discussions with G. I. Taylor about some liquid gyrostat experiments, which was what he was doing at the time.

Frenzen: With regard to Hide and his apparatus, I remember from the photographs, it was fairly narrow and tall...did he modify the aspect ratio of the cylinder.

Fultz: Not too much later. All the Cambridge experiments that he did were done with cylinder combination that was--in which the outer cylinder was about--

Frenzen: 20 centimeters in diameter?

Fultz: Something like that. The radius ratio was about 40%. It in fact was much higher in depth to gap ratio than we generally used later, but it was practically identical with the spherical tube TB1A that we built right after I got back.

Frenzen: The heated hemisphere.

Fultz: No, the annulus, the small annulus experiments. Those were started up precisely as a follow-on to Hide's, and we'll probably come back to it quite a bit later because it goes on for years. But essentially the very clear-cut wave states that I showed were there were just begging for somebody to look at them in great detail, which we tried to do eventually. The main thing we did in the first few years that went well beyond what Hide was able to do early on was to establish the existence of rather sharply defined wave number transition curves, and a symmetry transition outer envelope curve for changes of state that were sufficiently slow.

Frenzen: These were given in the reports.

Fultz: Right. That was first done in the couple of years right after I got back.

Frenzen: We're practically at the point where you came back from Europe.

Fultz: Well, not quite. In the middle of August, I went back to the Continent to visit Delft, where there's a very large hydraulics lab, again somewhat like the one in Grenoble. But partly doing commercial work for dams and rivers all over the world. That was also the place where Berger was. I visited his institute but he was away the time I was there. Then I went down to Lille, where there was a small institute; the professor was doing primarily aerodynamics, things in turbulence, things like that. The professor was Kamtc de Feriet. And after that, I went up to Brussels where for three or four days, there was one of the big meetings of the Union of Geodesy and Geophysics International Union. That was the first time I'd been to a big meeting like that. Quite interesting. Lots of people there. J. Bjerknes, Rossby...

Frenzen: Did you give a paper at that meeting?

Fultz: No.

Frenzen: About that time, you returned--

Fultz: I went to Cambridge for a few days and then down to London and Southampton, where we boarded the **Mauritania** for New York.

Frenzen: Then back to Chicago, nonstop?

Fultz: Yes, by railroad probably.

Frenzen: We've got you back in the Hydro Lab. We'll end this session at this point and resume at a later date.

***

Frenzen: Resuming taping on the 27th of November, the day after Thanksgiving, at the

University of Chicago.

It's the fall of 1951, and you rejoined the Hydro Lab, having left Bob Long in charge during your Guggenheim in Europe, and by then, I had joined the lab. George Owens was there and Walt Bohan was there. I believe at that time we were supported by the Air Force Cambridge Research Center.

Fultz: Right, and that support was developing to be very considerable for the time. We had a large number of contacts with people in Boston, Starr's project especially, Ed Lorenz, Bill Widger and the project scientist, who was Bob White, later became chief of the Weather Bureau--

Frenzen: Yes, I remember Bob coming out on a visit probably that winter.

Fultz: His first visit was in the middle of September, 1951; in fact, that was when we started the guest book for the lab. I'd seen such books in the labs I'd visited in Europe, and he was the first man to sign.

Frenzen: The work going on at that time, the spherical heating experiments were winding down...

Fultz: But still continuing to a certain extent.

Frenzen: And Bob Long's obstacle experiments were in full swing then, because I was helping him on that.

Fultz: The dishpan experiments must have been going primarily with George Owens running them because in fact either during the year I was away, or the year of 1951-52, we did the preliminary experiments with streak photographs on a Rossby regime-type flow in the dishpan, and those results were being analyzed by measuring horizontal velocities on a grid in the pan, and were being worked up by Starr's project, in fact.

Frenzen: At MIT?

Fultz: Right. So the big questions were, to what extent would we find large-scale eddy fluxes of the general type that Starr was beginning to measure with pretty good precision from atmospheric data. Needless to say, those results were quite promising at the time.

Activities that following year would have [included] a continuation of the irregular Rossby-regime dishpan experiments, urgent construction of an annulus apparatus almost identical with Hide's.

Frenzen: This was one in which you could control the temperature both in the rim and in the core.

Fultz: Right, with liquid baths on both sides. Because I'd come to the conclusion that was one of the most promising types of experiment to pursue. Quite a number of new types of experiments came along that year in more or less accidental ways that later developed over quite a period of time into substantial experiments. For example, in November, 1951, we ran a demonstration for Willett, S. Solot, Riehl and Horace Byers, showing them Taylor ink flow in a cylinder and we left it running and found later that it was filled with ink Bénard cells, which we identified almost immediately.

Frenzen: Convection cells driven by evaporation cooling from above.

Fultz: That later became one of our standard demonstrations and some significant experiment series.

Frenzen: I remember there were a lot of other things going on as well at that time.

Fultz: Yes, we were doing some tests on the hot spot vortex to try to find conditions that would produce good ones. It was about that time that Yoshi Nakagawa arrived as a postdoc--he'd been doing some similar experiments in Japan.

Frenzen: On model typhoons.

Fultz: Right. And the same month, January, we ran the first moving flame test [Narrator's note: *This was not the first moving flame test. That was run by Bob Long in December, 1950. See page 34.*] to follow up James Thomson and Halley's idea of whether there'd be a significant rectified current connected with a moving heat source.

Frenzen: Trying to explain the trade winds.

Fultz: Right. And at the end of that month, we were running a test on a two-layer rotating Bénard cell system, driven by evaporation to see whether we could distinguish the pressure differences through displacements of the interface in the cells, and that turned out not to work well enough to be very successful, but in the course of spinning up the two-layer system, we found that we could by frictional secondary flows produce a strong easterly jet in the upper layer and a ring interface, corresponds to one of the discontinuity cases that Helmholtz discussed in the 1880s. And contrary-wise, when you spin down from a high rotation, the lower liquid would collect into a dome and you'd end with a stage or go through an intermediate stage where a circular jet in the upper flow would sit over the maximum slopes on the outer rim of the dome. In the first tests within a couple of days we found that the westerly jet situation and the other one in fact were baroclinically unstable to first very short

Helmholtz-Kelvin-type waves on the interface, and then to what was identified in the meteorological literature already as baroclinic instability associated with the names of Eady and Charney.

Frenzen: I remember seeing those domes and for the first time realizing essentially what was going on inside the dishpan because of the presence of the waves at the edge of the polar dome so closely related to the waves in the jet aloft. It's a very effective demonstration, still today.

Yoshi, of course, by that time--or shortly after then--became interested in the cellular Benard case. Either he already had the basis of the theory when he arrived or he worked it up shortly after he joined us. I'm not sure about that.

Fultz: Yes, I think that's partly right. That gradually developed into a fairly significant experiment series that you worked on, too.

Frenzen: Yes, I worked with Yoshi on it and we published a paper, although I thought it was earlier; it appears to have been not until 1955 in **Tellus**.

There were still some obstacle experiments going on at that time, I remember, even though Bob Long had gone on to Johns Hopkins. We had the 90 degree barrier experiments, and we had a paper on the easterly flow across those barriers that [had] peculiar behavior. And we put together some movies, I remember, about that time, of the obstacle experiments.

Fultz: Right. And also on the Taylor two-dimensional flows. It was at that time that the first really clear suggestion for the barotropic cases arose that there was a very strong duality between the beta effects on the sphere and the depth effects in almost any geometry ___________________ _____________________. The crucial observation there was when von Arx at Woods Hole tried to get a western boundary current like the Gulfstream by rotating a paraboloid at the equilibrium rate. And starting a wind system of the sort applied in the sub-tropical gyres of the oceans, and he completely failed to get it. What he found was that he had to rotate at a higher than equilibrium rate and Carl Rossby happened to be passing through Woods Hole at that time. He identified what was happening at the equilibrium rate. The columns were essentially remaining axi-parallel and of uniform length, so that as seen in rotating coordinates, the beta effect was being exactly compensated by the decrease of perpendicular depth as you move from the center to the rim.

Frenzen: So in the end, instead of having to try to put in a latitude-driven effect, a latitude dependence of Coriolis, the beta effect, with that parabola, it was found you could do it with a flat pan...

Fultz: Flat disc, right.

Frenzen: --and run it at a speed such that the change in depth away from the pole to the equator played the role of change of Coriolis parameter with latitude.

Fultz: And that works beautifully as long as you're dealing with barotropic uniform density cases. It's by no means so direct when you have stratification in horizontal temperature gradients.

Frenzen: Von Arx visited us, I remember him.

Fultz: A couple of times. That was later.

Frenzen: William von Arx, from MIT and Woods Hole.

The dishpan results were so spectacular they started becoming fairly well-known about that time.

Fultz: Yes, and there was an important conference organized by GRD at Boston on large-scale processes. It included practically all the people in the country working on general circulation questions of that sort.

Frenzen: The spring of 1952.

Fultz: Right. Yale Mintz, Ed Lorenz, Victor Starr, George Benton, Bob Long, H. L. Kuo, J. Bjerknes, I've forgotten several others. And we made a fairly big splash at that because I had two or three movies that we'd already done. In particular, a movie on the moving flame results, which made quite a splash at the conference.

Frenzen: The newspapers picked up some stories then, too. In fact, one of them even said at that time they expected weather stations to have dishpan experiments to aid their forecasts, which was a bit far-fetched.

Fultz: Well, I can say we did not expect that...

By May, 1952, we had put together an annulus-type system in the dishpan just by inserting a cold source cylinder, and ? already confirmed that we could get jet waves of the type Hide had gotten in his much-smaller annulus; that we could get a symmetrical flow and in fact, essentially all except the vacillation cases that Hide had seen. Those came later.

Frenzen: But this is in a different aspect ratio.

Fultz: Right. It's almost one-to-one in gap over depth. Hide's were quite deep, five or six times as deep as wide. The Hide depth-to-gap ratio, the higher that is within the limits that have been checked, so far, the more clearly expressed you see the jets at the top surface and the steady wave states.

By October, 1952, we were running a number of dishpan-annulus tests. In particular, a three-wave case that on one visit from Ed Lorenz, he made a crucial suggestion that we do surveys of the three-dimensional temperature field by picking a steady-wave case and moving to a large grid of points with a single junction in the meridian plane. And then from the time variation at each of those points, reconstruct the longitude variation.

Frenzen: The period was so steady, you were able to do that over a long period of time, to get all the necessary measurements.

Fultz: So that was the origin of what we did in the first major three-dimensional set of measurements that Herb Riehl analyzed over a period of two to three years later. It took a long time to get satisfactory data on it.

Frenzen: It's like trying to measure the global atmosphere with a single radiosonde; the radiosonde allowed to take only one measurement at a time before the next radiosonde is launched.

Fultz: Along in this same time, to impress people, we were running Taylor wall experiments in a large spherical flask that gives beautiful series of patterns that, as far as I know, have never really been followed up, even semi-quantitatively. You simply put columns of dye crystals in to the neck of the flask, start up rotation from rest, secondary flow to boundary layer on the sphere carries color up to the equator. It then penetrates inward, and finally leaves the equator in a series of Taylor walls that depend on the details of how you started it.

Frenzen: Seen just at the surface, I remember comparisons with the banded structure of the planets such as Jupiter and Saturn.

Fultz: It reminds one a great deal of that. Nobody's connected it very much. The patterns are striking enough, however, that it made the cover of **Scientific American** in connection with an article by Victor Starr on the general circulation. He did not explain how it connected to the general circulation.

Frenzen: But the editor wanted a colorful cover.

Fultz: That's right.

Frenzen: There was even a suggestion that Taylor walls, that early--I've seen it since, and I knew it came out of our lab and probably from you--that the Jupiter Red Spot could be evidence of a barrier below, reflected aloft in Taylor walls. Now you're looking dubious.

Fultz: I don't really recall what happened. The person who made that suggestion in most detail was Ray Hide.

Frenzen: It's been repeated since.

Fultz: Then we must have been working through 1953 pretty seriously on rotating Benard cases including mercury, which we did because the theory that Chandrasekar had worked up for his book on hydrodynamic stability predicted that a liquid with a Prandtl number as low as that of mercury should set in with oscillating cells at the onset of thermal instability.

Frenzen: Instead of steady state.

Fultz: Instead of a steady state cellular distribution. And as far as I know, that's the first case in which such an oscillatory unstable onset has been established. There were some others later.

Frenzen: Suggested first by Eddington, the idea that if the pendulum's restoring force would be a function of--

Fultz: I don't recall whether Eddington had a specific problem in mind.

Frenzen: No, but the principle of the oscillating onset--

Fultz: And the terminology was--

Frenzen: Over-stability, instead of instability.

Yes, I remember those experiments. They were rather chancy, heating mercury in the confined spaces in the lab. We had it all sealed--I guess we had liquid nitrogen traps, too, somehow. Through rotating glands: I remember the mechanical problems.

Fultz: They were considerable.

Frenzen: And about then, I guess Chandrasekar became interested in their work. I remember visits to the lab.

Fultz: Yes, and we did at least a couple of other series of experiments over a number of years with he and Russ Donnelly that arose either from suggestions from what we had done or from questions that he thought ought to be looked at experimentally.

Frenzen: Donnelly, Central Shop?

Fultz: No, no, you're thinking of O'Donnell. But Russ Donnelly was a helium, low temperature physicist who was doing Taylor double-cylinder experiments in liquid helium below the lambda point when I first ran into him.

Frenzen: Chandrasekar's interest, of course, was the interiors of stars and the theory which he had presented in his book led to experiments in magnetic fields. One of which you told me was done in Ryerson?

Fultz: The only one that we did there. That was a case where we did a simple Bénard single-layer experiment with mercury in a field of medium-size magnet in order to test whether the onset of convection would be as over-stable oscillating cells. That was the first confirmation. Then we did some more extensive experiments for the rotating case.

Frenzen: Of course, Yoshi Nakagawa, who was involved in all of this, and because of his interest in both the theoretical and experimental aspects of the Bénard cellular motions--he started working more closely with Chandrasekar using the old cyclotron magnet that was--as I recall it's now in the building that's now the University Press, wasn't it, some unusual--?

Fultz: No, it was shoehorned into what used to be the old buildings and grounds.

Frenzen: Right, but it's right in this same complex. I remember you had to get to it through the back alley.

Fultz: Which is now the lawn in front of Creran.

Frenzen: Yoshi then left the lab--

Fultz: And then went with Chandra's project to do the combined case of magnetic field using the cyclotron plus the rotation.

Frenzen: And after a few years there, Nakagawa went with the High Altitude Observatory in Boulder, and was there for quite a number of years, and the last I heard, went back to Japan as a professor in a new university. Not very far from Tokyo.

That puts us now in about the year of 1955.

Fultz: Yes. By this time, we were working very extensively on a small, tall annulus that had practically the geometry of Hide's original annulus assembly. And had started running systematic spectra by holding rotation-fixed and bringing the bath temperature difference very slowly up from zero. And that established for the first time that there were quite sharp transition loci where the steady wave states would change from one wave number to another. That were really quite reproducible as long as the change of drive and temperature difference was sufficiently slow. By the time we had discovered that there was both a set of wave number transition curves and an outer curve separating symmetrical states that had both an upper branch and a lower branch that occurred for that particular geometry with water at the temperatures we were using at a little less than 2/10 of a degree Centigrade, after the radial temperature difference, we found that in order to locate that lower symmetry transition accurately, you could not change the bath differences more rapidly than about 1/100 of a degree in five, ten, fifteen minutes. If you went faster than that, you would get a failure to go to the symmetrical flow.

Frenzen: I remember in principle that we were doing the same sort of thing in the few months before Yoshi left, with the Bénard rotating cellular convection experiments. We would slowly increase the vertical temperature gradient, and you could see the onset of regular convection. Then you could see it break down into what was probably a regular convection, and then finally some form of irregular motion, precursor to turbulence.

END OF TAPE 2, SIDE 2

Interview with Dave Fultz

TAPE 3, SIDE 1

Frenzen: We are continuing recording on the 27th of November.

Fultz: About that time, in April, 1955, we had run into many cases of vacillation with the tall annulus, the type that Ray Hide had seen very early.

Frenzen: Where the wave pattern oscillates between two states.

Fultz: In particular, the case I have a note on here, one where modulation between the wave passage period and the cycle period was getting up into the hundreds of revolutions. Very long period responses.

Somewhat earlier, in fact in 1954 for the Rome meeting of the UGGI, we put together a dishpan movie over a range of Rossby numbers for both the annulus cases and for one open center sequence plus the sequence on the moving flame, which is still the best overall survey that exists on film. That was shown by Professor Byers at the UGGI meeting.

Frenzen: Was that a review paper? I mean, a movie like that runs fifteen minutes. They usually give you five minutes for a paper.

Fultz: No, it ran 20-25 minutes.

Frenzen: So it must have been a basic review paper.

Fultz: But I think the international effect was substantial.

Frenzen: There were hydro labs springing up all over the place.

Fultz: _______________ heavily involved really with the Starr general circulation group, and in October, 1955, there was a major general circulation project at the Princeton Institute for Advanced Study that had all the usual suspects--J. Bjerknes, Holmboe, Norm Phillips, Charney, Starr, ______________, Benton, Mintz, Arnt Eliassen, Ed Lorenz and von Neumann--discussing a range of things, including the experimental ones. There, as on a number of other occasions, some of the computer people discussed the prospect of doing numerical integration work on one or more of the experimental cases. Thirty years later, unfortunately, there are only one or two that have been actually carried out and none on some of the most interesting cases. It would be very hard to convince the computer people to get out--

Frenzen: The computer modeler does not want to be held too close to something that can be checked experimentally, perhaps?

Norm Phllips was around in those days, wasn't he?

Fultz: In fact, this conference was held just before he published his epochal general circulation two-layer model paper in 1956, which showed for the first time that a numerical model was capable of producing quite realistic baroclinic disturbances and fluxes out of an initial state of rest, after going through a purely zonal state of motion.

Frenzen: I remember that Joyce Cork, who later became Joyce Weil, and Bob Kaylor joined the lab at that time. Probably a year or so earlier, it may have been in 1954 or so. Wasn't a lot of this work in those days and previous summarized in the meteorological monograph? You must have started working on it about then.

Fultz: Yes, we must have been working on it very seriously by 1955 and 1956. It contains really a survey both of dishpan type and annulus experiments, some of which extend all the way back to 1952 and up as far as 1957, I believe.

Frenzen: And it was published in 1959. That's **Meteorological Monograph volume 4, no. 21**.

Chandrasekar suggested some other work. (For the record, that's S. Chandrasekar, noted astrophysicist-astronomer, and Nobel Prize winner.)

Fultz: Yes. He and Russ Donnelly discussed with me in the middle of February, 1957, the possibility of building a Taylor double-cylinder apparatus for a radius ratio of .50. A lot of double-cylinder work has been done since Taylor's classical paper in 1923, which was preceded by also classical work of Couette and Mallock in the 1890s.

Frenzen: These were in thinner shells.

Fultz: Practically all were much thinner shells. Chandra was doing theory more or less for all radius ratios as part of the material for his book, and he wanted to get some experimental values for that particular radius ratio. It took a while to build it, but we did get eventually to the first really good critical point in August, 1957. That really was fairly short. The point there is that if you rotate the inner cylinder and hold the outer one stationary, which was the case we did first, the motion is really laminar and steady at times when it ought to be unstable according to Rayleigh's inviscid criterion until a very sharply defined critical point which was first calculated by Taylor in 1923 for the cases he considered. It was succeeded then by a regime in which there are for quite some time steady secondary helical cells, which set in at a definite wave length and whose properties have been studied actually by now by hundreds of people.

Frenzen: Did this work go on--you went overseas again about that time. Did that go on in your absence?

Fultz: Yes, very shortly it was continued by Russ Donnelly and probably one of his students.

Frenzen: And the papers were published in 1961? No, 1960.

I had left the lab by then, but I remember that Allen Faller came here for an extended period while you went to--

Fultz: Yes, he took over the lab for the year that I was away.

Frenzen: Allen Faller is at Maryland or at Johns Hopkins?

Fultz: He may be retired by now, but he was at the University of Maryland.

Frenzen: Didn't Bob Kaylor go out to join him there?

Fultz: Yes, he did.

Frenzen: This was an NSF senior postdoc, 1957-58. And you went back to Cambridge?

Fultz: Right. For the autumn and then followed by a fair amount of traveling around back and forth.

Frenzen: A lot of this historical work on modeling, did you do a lot of the work that eventually was published in the--

Fultz: Yes, a fair amount of it was done that year.

Frenzen: **Advances in Geophysics.**

Fultz: That's right.

Frenzen: Published in 1961, volume 7, **Advances in Geophysics**.

Your first trip overseas was very productive. What did you find out, and who did you meet on this one?

Fultz: Well, just as many things if not more, and a number of the same people and a lot of new ones. In the first place, Hide's annulus experiments were still going on when I got there in the autumn of 1957 by a student named Arthur Smith. Along lines like those we were carrying out, but not really duplicating them. There were some interesting experiments going on at Imperial College by a student of Dick Scorer's, named Betsy Woodward, which consisted of essentially measuring the self-similar expansion of a buoyant plume, intending to model some features of cumulus.

Frenzen: Were these heated?

Fultz: No, they were done upside-down by pouring a slightly heavy solution from a half-sphere into a big brick tank.

Frenzen: I remember seeing some of those.

Fultz: But then fairly early in some discussions I had in October with George Batchelor we were discussing the over-stable oscillating cells in mercury, and he suggested there had to be some stabilizing mechanism to explain the oscillation plus an energy transfer mechanism to explain the self-sustaining nature of the operation.

Frenzen: This is G. K. Batchelor at Cambridge.

Fultz: In the course of that discussion, he asked whether anybody had ever done the type of elastoid-inertia experiments that were calculated theoretically by Kelvin and by V. Bjerknes in the **Physikalische Hydrodyanmik,** and I didn't know that anybody ever had, so that was the germ of the idea for taking a small rotating circular cylinder and generating standing oscillations n it by simply oscillating a disk on the axis. Rather early on that, I was able to determine that we could tell resonant conditions in which the disk oscillation was equal in period to one of the free modes very accurately, in fact more accurately than 1%. So I proceeded to determine oscillation frequencies as a function of the depth-to-radius ratio in a cylinder, and of the mode that was involved; we were able to do three of the vertical modes and two of the fundamental for the radial mode and either two or three of the higher harmonics for that. So that was a very useful set of experiments that we extended somewhat when I came back home and also used in the National Committee for Fluid Mechanics films as two of the sequences. As George commented, Rayleigh should have done them, namely the experiments.

Frenzen: Could he have timed them accurately?

These experiments were summarized in the **Transactions of the New York Academy**, 1962 paper.

Fultz: Aside from the elastoid-inertia experiments, I started in Cambridge, the most important thing I did was hook up with Ian Proudman and his research student, Derek Moore, who were just starting to build a concentric sphere apparatus with a radius ratio of 1/2. They intended to verify some earlier expectations that Proudman had for that situation when you rotate the inner sphere at a very slightly different rate than the outer sphere. It corresponds not to the instability in the Taylor double-cylinder but more to the secondary flows that occur when you have rotating disk combinations with Stewartson or Ekman layers in them that were running at different rates. Later on that year, I did quite a large number of experiments with Derek that turned up a staggering array of different types of free shear layers and instabilities we can come to later.

Aside from that, I did a fair amount of traveling again to various places: in February to see Stewartson in Bristol, who was an aerodynamicist who had done a good deal of the theory on shear layers of the type that Proudman was working on. I went up to Newcastle to see Raymond Hide and Runcorn. That was in March, and by that time, Derek and I were getting very good pictures of the concentric shear layers. Then in March, the family and I traveled to Europe, Frankfurt again to see Mügge. Talked to Flohn who was interested in the experiments comparable to an elevated heat source like the Tibetan Plateau, and we never got back to doing that. Other people have since. Went to Gőttingen where Ernst Kleinschmitt was still present and a man named Tinman was doing high-precision Taylor double cylinder experiments like those that we were doing with Russ Donnelly and Chandra. Then on to Stockholm, where there was a large group—Kirk Bryan, Fritz Defant, Pierre Welander, Arnold Arons— interested in oceanographic and a number of other types of rotating experiments. In particular, Arons and Bryan were working on the source sink sector experiments that stem from the paper that Arons and Stommel wrote just a little bit earlier.

Frenzen: Is this an ocean-basin model idea?

Fultz: Yes, it's sort of an ocean-basin western boundary current idea. If you have a triangular sector rotating and put a source at the apex, it'll proceed to the rim along the western boundary--

Frenzen: Western intensification.

Fultz: --and fill the sector first zonally from the west, and then from the south...it's a basis for the reasoning that suggested to Stommel that there ought to be a counter current under the Gulfstream filling up the North Atlantic Ocean from the south. Not only in the main part of the basin, but underneath the Gulfstream.

Frenzen: Well, Arons of course tried to model the whole thing. Has anyone else gone on with ocean-basin models or globe-ocean models?

Fultz: Only sporadically. Nobody's doing it really systematically that I'm aware of.

Then, in Oslo, there was an interesting group: Arnt Eliassen, E. Hoiland, Enoch Palm, and Fjørtoft, plus Solberg of the original Norwegian group. They described to me and in fact pulled out a little hand-rotated cylinder that the Bjerknes's had used for elastoid inertia demonstrations, particularly the type where you put in a sphere and it oscillates up and down when you disturb it. That was a sequence that we used in the fluid mechanics film. And, at the time, they had some ideas of starting an eventual lab there. That suggestion came up lots of times in that particular decade, but almost none of them in fact have succeeded.

Frenzen: Were there any undergraduates there? It seems to me--

Fultz: I wasn't quite aware of the administrative structure. Part of it was part of the University of Oslo, and there were undergraduates there. But part of it was an institute of geophysics, or something like that, which did have graduate students, sort of jointly.

Frenzen: The reason I ask that is that so many schools, it seems to me there's a period where they started hydro labs for the purpose of demonstration to students, rather than a research institute.

Fultz: Well, not too many succeeded even in that. At least I'm not aware of it if they did.

I spent six or seven weeks in Oslo, and in fact, along with discussions primarily with Eliassen and Palm, who were both theoretically interested, did most of the work on the elastoid-inertia oscillation paper which was published in 1959 in the **Journal of Meteorology**. Then, after leaving Oslo, we traveled several places. First, down to Hamburg where Raethjen was in the process of beginning to build a laboratory for quite large rotating disks for dishpan-type experiments. And for a few days to Aachen, where Schultz-Grunow was doing Taylor double-cylinder experiments of a quite innovative kind. He was, in fact, the first to show that when you get up in the range of very high instability such that there's very intense small-scale turbulence in the gap, you still have detectable cellular structure present, but it jumps to double the wavelength, roughly. That still has not been adequately followed up.

In addition, there was a young fellow named Zierep there who was working on Benard-type experiments, which we were, of course, interested in. Particularly, he was doing the theory for a circularly-symmetric case where the cells set in as circular rings. That was something that was done experimentally later by Lothar Koschmieder.

Frenzen: Is that in a shear flow?

Fultz: No, just in ordinary Benard situation. I forget what prevents it from going in to hexagons...

Then we visited Darmstadt where the younger Koschmieder was still professor. He was the one who did the seabreeze studies at Danzig, and had done a lot of work on the atmospheric boundary layer. On to Freiburg, where Gőrtler was the rotation man, and doing curve flow stability questions. And a young man named Wrage, who, as I recall, was doing stability of the vertical heated wall. A short trip to Munich, where I both met Ernst Schmidt, who was the grand old man of engineering thermodynamics, and a young fellow named Tippelskirch, who was at one of the Max Planck Institutes doing cellular convection.

Frenzen: He's the man who did cellular convection in molten sulfur.

Fultz: Yes, that's right. I think he'd already done it by that time, though.

Then we went through Paris and talked to Queney and through London, where a research student named Shaw was doing obstacles in stratified flows, the sort of thing that Bob Long did and was doing at Johns Hopkins. And finally back for a fairly short time at Cambridge in July and August, 1958. That interval, in July and August, was when Derek and I did most of the concentric sphere experiments and measurements on them.

Frenzen: Is that what led you to design a concentric sphere for the Hydro Lab when you got back? Weren't these big spheres made?

Fultz: No, that was much earlier. The great ones were really fabricated in 1951 or 1952 and they resulted in the intention to do the original concentric sphere thermal turbulence experiments at a much bigger scale.

Frenzen: I remember their delivery and so forth. They handled like jewels.

So at the end of the summer, the end of August, 1958, you were back in the Hydro Lab at the University of Chicago.

Fultz: Yes, that's right, and quite a number of things were in process then at the same time, partly on work that was done by Al Faller while I was away. In particular, he did some very careful three-dimensional temperature measurements that were really 2-D temperature measurements in symmetric cases, including measurements in the wall boundary layers. That had not been done before and these were the first I'm aware of that were accurate enough to resolve those. And that suggested that we ought to do that much more extensively, so in particular, one of the rotating Hadley symmetric cases was analyzed by Vaisanen, who was at that moment a postdoc with Herb Riehl.

Riehl and I had just finished the three-wave papers and we were discussing whether to tackle the job of trying to do a comparable set of measurements on a periodic five-wave vacillation experiment, which we eventually did but at the cost of a very long period of work. But the Hadley symmetric fields were very interesting in the boundary layer structures, and we intended to do quite a series of them to try to determine the actual steam function for the meridional flow. In particular, for an annulus case, the strongest boundary layer of the cold cylinder had a viscously driven tall eddy next to the boundary layer itself that produced an upward flow of cool fluid, opposite to the elementary assumption that cool fluid sinks. We eventually did that much later, both for a rotating symmetrical case in an annulus, and for a non-rotating symmetrical case, which had its own set of eddies in a completely different boundary layer structure in meridional cell structures from the rotating one. That came later, however.

Frenzen: Thinking for a moment about Al Faller, when he went back to Maryland, didn't he build a much larger--

Fultz: Yes, he did.

Frenzen: --and one of his purposes, at least, was to be able to look at the boundary layer, the deeper boundary layer in a larger pan.

Fultz: That might well have been. He of course had a very large pan at Woods Hole in which he did some of his original dishpan experiments.

Frenzen: Was that on von Arx's apparatus essentially?

Fultz: I don't recall. Probably it was...it was a flat one. So he built a very substantial one at Maryland.

Frenzen: Because he was doing some interesting boundary layer work that I became aware of later on.

Fultz: Right. That was slightly different. That was barotropic flow generated by sources and sinks, and I forget whether he started that at Woods Hole. I think perhaps he may have, but in any case when you force it intensely enough, you can force the boundary layer flow to go unstable, to sort of spiral cellular modes in unstable flows.

Frenzen: But meanwhile, back at the Hydro Lab...

Fultz: In addition to the symmetrical dishpan experiments, we were running the small, tall annulus all the time for various aspects of the spectral transitions, and turning up--well, in fact, part of the work had been done while I was away for a paper by Bob

Kaylor and I in the **Rossby Memorial Volume**, in which we did a series of comparison experiments in a dishpan annulus with inserted conical bases of various slopes. The idea there was to try to see what would happen to a baroclinically-driven heat source, cold source annulus if you added on the sort of grass-depth variation that in the barotropic case would give you an equivalent beta effect. And what we were naively looking for was that if we put a sufficiently high slope up toward the center, which gives a positive equivalent beta, that we ought to get phase velocities more rapidly toward the west according to usual Rossby frequency equations.

Unfortunately, what happened was they went more rapidly toward the east, and so we had to look for something more subtle and eventually found it, in fact, but not in a way that could be exploited too much beyond what we were able to do. Namely, it turned out that if we reversed the slope, we could detect it, in effect, of a negative beta. But the effect that was detectable was essentially a group velocity phenomenon in which essentially spontaneously (so it amounted to a different type of secondary instability) you would see a change of trough shape and of local wavelength between two successive troughs propagating around relative to the wave train and relative to the pan. Both propagating through and propagating relative to the pan. Those turned out to be crudely consistent with what would be predicted by a group velocity equation for a set of Rossby waves, for the equivalent betas determined by the slopes of the cones. But it took a good deal of work to get what essentially just produced a qualitative result and couldn't be generalized very easily to anything more realistic.

Frenzen: Along about here or within a year or so of this time, other graduate students began to join the lab. T. S. Murthy must have come in the early sixties.

Fultz: Yes. Bob Kaylor was already there.

Frenzen: Bob Kaylor might have left shortly--

Fultz: Not too long after that he went to Maryland.

Frenzen: He went to Maryland with Al Faller.

Fultz: Jack Kaiser came and he did a major fraction of the work that was eventually done on the annulus spectrum and wave number transitions. And properties of both the steady waves and vacillating waves that were done there. We essentially did sort of broad survey types of experiments for the most part in the tall annulus because the things went rapidly and you could change easily from one type of experiment to another. Then we reserved the dishpan sort of sizes, roughly 40 cm. outer radius, for more elaborate experiments like the big three wave experiment that we did with Riehl. Or like the very detailed symmetrical experiments that Al Faller did.

Simultaneously with the convection experiments, we had started Taylor double cylinder experiments at the radius ratio of 1/2 for Chandra. Those were continued by Russ Donnelly while I was away and may have even been worked on by one of his graduate students...

At some point, and I'm not quite sure of the order here, Chandra again suggested that we build a narrow gap apparatus, which we eventually did with a radius ratio of .95, and we did a fair number of experiments just with that of the type that Taylor had done, I believe, at a somewhat smaller gap than any that he used. But essentially, the first transitions agreed almost perfectly with Taylor's theory which agreed almost perfectly with Chandra's over the range they had common parameters.

One result of that was that we discovered a very effective, what amounts to a parameterization for the critical point at very large magnitude negative values of the parameter mu, which is the ratio of the rotation rate of the inner cylinder to the rotation rate of the outer cylinder. Because that's a case where the unstable region is confined to a very small radius near the inner cylinder, and finally becomes almost independent of the gap to the outer cylinder because all the rest of the fluid region is stable in the Rayleigh and Taylor and Chandra sense.

Frenzen: It's all within a layer.

Fultz: It's all confined to a single layer, maybe two with one. You can detect some ink tracing outside the main cells, but we measure wavelengths quite accurately in there and could tell that most of it was occurring essentially inside the first zero of the zonal velocity.

Very shortly after that, Chandra asked us to rig the double cylinder apparatus so that we could run an axial flow simultaneously with the rotation of the inner and outer cylinders. That took a little time to do but we succeeded and Russ and I measured a fair number of first critical points for the onset of cellular motion there.

Frenzen: Did you introduce sources and sinks?

Fultz: No. We just arranged a manifold to pump in at the bottom and then out at the top.

Frenzen: That's what I meant. Source and sink.

Fultz: OK, right. But not in the working volume. It was arranged so as to minimize as much as possible any variation across the inlet apertures.

Frenzen: They weren't points.

Fultz: Though this had been a situation that had been considered theoretically, I think before no experimental measurements had been done on it, and that was the basis for a paper in 1960 that Chandra sent to the **Proceedings of the National Academy of Sciences**.

Frenzen: Spiral flow between rotating cylinders.

Fultz: I don't recall how much use was made of that apparatus thereafter, but at least some was by Harvey Snyder, who was a graduate student of Russ Donnelly's.

Frenzen: And that was over in Rosenwald, no, I'm sorry--in the Physics Building--

Fultz: No, it was in Rosenwald. It was set up in Rosenwald and he came over to do it. The liquid helium stuff was in the Research Institute.

Frenzen: I might just insert for the record that about that time, in 1960, you were named full professor here at the University of Chicago.

Fultz: About this time, in late 1959, I had a fairly nice coincidence in a visit from G. I. Taylor himself, which he seemed to enjoy a great deal. Later on in October, after considerable struggle on our part with financial matters in the last three or four years, when we were gradually losing Air Force support, the National Science Board passed a three-year proposal for us for $215,000, which was very generous of them at the time.

Frenzen: The National Academy?

Fultz: No, the National Science Board. It was apparently big enough that it had to go up to the National Science Foundation board. These days it wouldn't.

END OF TAPE 3, SIDE 1

Interview with Dave Fultz

TAPE 3, SIDE 2

Frenzen: In 1960, a couple of papers by yourself and R. J. Donnelly on the double cylinder work originally suggested by Chandrasekar.

Fultz: We continued those in fact for a fair time after that. We didn't write any more papers out of the lab. The main thing that was pursued and a couple of graduate students of Donnelly's worked it out in the lab on thesis topics involving the same apparatus.

Frenzen: Do you remember their names?

Fultz: One was Brian Springet, and the other was Klaus Schwartz.

Frenzen: In the Hydro Lab itself, the annulus...

Fultz: Probably the most extensive experiments continued to be the convection experiments first both in the small annulus on spherical two, in which we were continuing to fill in more details of the wave number change spectra and amplifying that, I don't recall exactly when it was done, but we did spectra both for proceeding upward toward higher thermal gradients at a fixed rotation and proceeding downward from high values to low values. Plus we did one major wave spectrum for the opposite to meteorological case, where you have a hot source at the axis and a cold source on the outside.

Frenzen: When you say "we," who was working with you on that at that time?

Fultz: It was Bob Kaylor doing a great deal of it, and then after 1960, Jack Kaiser took over a major portion of that work.

Frenzen: It's a little confusing when you say the experiments with the cylinders were done on spherical two. Now I know that spherical two is the basic rotation apparatus, which was originally built for the experiment with spheres.

Fultz: Yes. Mike Fain always said that he preferred not to use those names, but they were too much set in concrete to get it changed.

Frenzen: When was the big hydraulic rotating table put into operation?

Fultz: That was built really quite early, in the mid-fifties. It was originally installed in the basement of Rosenwald in a pit.

Frenzen: I remember the plumbing.

Fultz: We mainly did--well, there was a fair problem with the drive. The hydraulic drive never did work properly, it tended to hunt, and vary rotation too much, so eventually we built a new gearbox and bought a very large Graham synchronous transmission, which turned out to essentially solve the problem because it was stable enough if it was operating in a middle range to essentially follow the line frequency variations. So we got rotation stability that was as good as the frequency of the power lines, and that in this area at the time was about 1 part in 4,000.

Frenzen: It's interesting that these experiments have to be that precise in their rotation rate.

Fultz: In fact, some of them ought to be better than that. We never got to the point of designing a system that would do better. But Edison was quite coy about telling us how the controlled frequency would do when we set up and actually measured it, then they were willing to talk.

Frenzen: That's Commonwealth Edison, the power company. I gather that what they really do is attempt to keep the frequency accurate enough to keep the clocks accurate. When they fall behind, they speed up.

Fultz: That's right. But also they really have to maintain uniform phase over the whole grid or they'll be working part as generators and part as motors.

Frenzen: Well, we solved that problem.

The annulus spectral work Jack Kaiser, that went on to be his dissertation work, is that right?

Fultz: That's right. The principal results of that were presented in the series of papers that he wrote drawing on his thesis plus some later work in 1969 through 1971. What he did there was among other things--this comes much later, but it's probably best to discuss it here--was first, measure very high precision transition spectra such that in fact we were pretty certain we were able to distinguish humps and cusps on the symmetry transition curve. Quite high precision wave number transition curves inside, and then both in the symmetric regime and the wave regime, detailed three-dimensional temperature profiles which were sufficiently good even on these very small sizes to resolve the thermal boundary layer. And so a number of his papers were discussions of the properties both of the lower symmetric regime and the longitude average temperature field for baroclinic waves. In addition, those got accurate enough that in 1971, he and I took a number of the examples that involved disturbances from a single height hypodermic needle thermocouple probe, and showed by some extrapolation experiments that putting in a single hypodermic of 8/10 millimeter diameter, even in the Hadley symmetric regime, was in certain areas having the effect of making a global alteration both in the stratification and in the meridional circulation strength.

Frenzen: It's a little bit bigger than Ed Lorenz' fluttering butterfly...on scale those are major disturbances...

Fultz: That alerted us to the fact that when you get down to trying to do, say, a few highly selected experiments with great precision, then you have to start worrying about a number of things like that, which don't show up enough to be obvious when the measurements are rougher.

Frenzen: Where is Jack Kaiser now?

Fultz: I'm pretty certain he's still with the Naval Research Lab. Last time I talked to him, which was sometime ago, he was involved with some oceanographic field experiments. But he's probably done a number of things since he moved there.

Frenzen: Presumably the open core dishpan experiments were continued at that time, too.

Fultz: We didn't do too many of them for awhile until almost 1970. We concentrated mainly on dishpan size annulus experiments with a very much cleaned-up apparatus that was no longer an aluminum dishpan from the hardware store, but was built with quite accurate dimensions and provided with various size internal cylinders that could serve as cold sources. The principal things that we did with that were, while Bob Kaylor was still with the lab before he went to the University of Maryland in 1963, we did experiments for the **Rossby Memorial Volume** with conical bases, which I think we mentioned in the previous reel [of tape]. We had agreed with Herbert Riehl to try to find a vacillating experiment that would be sufficiently periodic, both in space and in time, to try to do the sort of three-dimensional measurements that we had done on the steady three-wave case. And that turned out to be quite a job. We eventually settled on a five-wave experiment that was really quite good, but turned out to have a problem that we never quite solved because of the fact that the Type-3 dishpans that we built very carefully unfortunately were not provided with an outer controlled water source, but had a heat source provided by resistance, wire wound underneath insulation.

Frenzen: Out at the outer rim, at the bottom.

Fultz: Along the whole...so it essentially was an annulus-type situation, but with essentially a heat flux condition at the outside. And what we found when we finally did the major one week run on the vacillation case was that there was enough sensitivity to the still-occurring drifts a little bit cooler at night and a little bit warmer during the day that the dishpan was reacting to it and so we were seeing on repeating, a certain selection of points that we were coming in with temperature values that were 1 or 2/10 of a degree different than on a preceding part of the run.

Frenzen: Did it change the patterns as well?

Fultz: Not detectably. While this would have been pretty negligible, if we had found even that big an amount in the thermal boundary layers--especially on the two inner and outer walls--it was a bit of a worry on the interior where the general gradients, primarily the radial gradients, are about an order of magnitude down from what they are in the wall boundary layer. That means that since the plan was to do the same thing we had done before, namely to measure velocity fields at the top--which we succeeded in doing quite well--then with the three-dimensional temperature field to integrate hydrostatically downward to get hydrostatic pressure fields in the interior and then reverse and essentially by a balance equation, recover the interior velocity field. And this analysis work was done by Russell Elsberry as part of his PhD thesis working for Riehl out at Colorado State University. But there were some residual problems we weren't really able to settle because of the differences.

Frenzen: After Herb Riehl went out to Colorado, it's interesting that he took his contacts from the Hydro Lab and the data source with him and produced another graduate student out at Colorado.

Fultz: That was more or less inevitable because it took us five-six years to get up to the point of doing the one week experiment. And it had another offshoot in that when we measured the streak photographs at the top on I forget how many cycles we used, we averaged at least, I think, three together, three separate cycles--

Frenzen: Three complete vacillation cycles?

Fultz: I think we took ten different times in the cycle period and averaged three at each of those times from separate cycles. What we found was that was some sort of aperiodic variation that was appearing in the flow field at the top was not drifting with the waves or any other speed, but appeared to be standing stationary relative to the pan. So it was moving toward the west relative to the waves. We puzzled a great deal about where that could be coming from. We finally decided to check very carefully whether the base, which had been very carefully machined, was really flat. It was a composite of synthane plastic mainly with an inlaid circular copper mid-radius heat source that was one of the things we found necessary to clear up aperiodicities in the vacillation cycle. And what turned out, I don't remember exactly how big this was. There was an elliptical-shaped deviation from flatness--

Frenzen: You mean a change in depth over--

Fultz: Right, except it was considerably less than the moon of your toe span, whereas I forget now, but I think the flow depth was seven centimeters so this was an exceedingly small percentage change. That suggested that we go back and look at a regular annulus case with both some radial mountain ranges deliberately made very substantial and some that were only a couple of millimeters.

Frenzen: You mean a radial plateau centered on that point--

Fultz: The things we actually used mostly were essentially mountains that--

Frenzen: How many degrees of longitude wide? Ten?

Fultz: Ten, fifteen, twenty. Something like that.

Frenzen: So it was a small sector.

Fultz: --that had a uniform height crest and then variable slopes down to the base at the radial boundary. And Tom Spence pursued that a fair amount when he was first working in the lab and it was published in 1968 in the **Proceedings of the Symposium on Mountain Meteorology** at Colorado State University. But there were some worthwhile results of that. The first thing we found was that it was very difficult to take a steady wave train and get much effect and you had to have quite high mountains before you could see a qualitative difference. But if you were near a vacillating state, the very small ones would do a considerable amount that was visually obvious in looking at the flow patterns. That's a very difficult thing to follow up, however, and it hasn't been followed up yet. The other thing, however, that Tom did was look at an old question that had been raised many times before, namely whether we could get the sort of internal gravity wave modes in the pan that occur in the atmosphere over ranges like the Rockies or the Sierra Nevada. We'd tried this a bit--

Frenzen: Lee waves.

Fultz: Right, mountain lee waves. But it never seemed to develop really well-developed examples. So we put in some very narrow ridges that would be the equivalent of a line fence of nearly negligible width at the various heights, and look at the flows associated both with wave states and symmetrical states. What turned up was that when you crank up the temperature differences in trying to get stronger zonal flows across the mountain, that entails cranking up the vertical static stability at a rate which, in the typical dishpan case, appears to keep you below the criterion for mountain lee waves. So it was almost as hard in this case to get them as we had before. But we did succeed in finding them. We found some local, very short wavelength waves--wavelengths on the order of millimeters that, in fact, definitely went into what was essentially very small-scale internal turbulence for a short distance downstream of the mountain crest in a very limited vertical sheet that had these instabilities in it for a short distance downstream. Then, of course, as soon as ink was dispersed, you couldn't tell what was happening farther on downstream. Except that the other major thing that occurred was that there was also a much larger scale horizontal wave mode superimposed that had wavelengths of five, maybe even ten centimeters compared to a few millimeters for the very short ones. That appeared pretty obviously to be sort of joint lee rotationally-influenced waves.

Frenzen: I think this is one of the first times we've mentioned Tom Spence. Was that his dissertation work?

Fultz: It led into it, but this is what he did in the early part of his graduate career.

Frenzen: He stayed in deep-water oceanography.

Fultz: He came as a graduate student in the autumn of 1965. His thesis work was a far more accurate dishpan experiment that we'd better come back to later, that involved the move from the old lab in Rosenwald to the Hinds Building that was finished in 1965.

Frenzen: The new geophysics building. That was a major move.

Fultz: And major things were discovered in Tom's work sort of accidentally during the move.

Another series of experiments that was carried through during this same period in the sixties was a follow-on to the elastoid inertia experiments that I did in Cambridge during my fellowship year there. We decided to follow that up on a bigger scale, using in fact the large turntable Spherical three, and the first things we did were to look into internal axisymmetric modes of that type generated first in a rather deep paraboloid that we had used before for various experiments.

Frenzen: Searchlight reflector. Some of them were glass searchlights.

Fultz: Most of them were. This particular one is rather deep and I'm not sure whether it was a searchlight or not. But in any case, we found that you could get axisymmetric inertia modes essentially as good as the ones in the small cylinder in Cambridge up to very high radial wave numbers in that particular paraboloid. In addition, T. S. Murthy, who was another graduate student in the lab and was there between 1960 and 1967, worked on these particular oscillation experiments and we discovered that the axisymmetric mode, particularly the one we worked on was one that appeared to have ten nodal surfaces for the radial velocity. So there were ten half-wavelength cells between the axis and the outer rim. He discovered that if you were forcing with sufficient amplitude, the outer cells of the inertia motion would go unstable to azimuthal variations. In other words, the axisymmetric mode was perfectly stable until you reached some critical point in amplitude for the oscillating disk that was producing motion.

Frenzen: It would break down into waves, as seen from afar?

Fultz: It would break down into what looked initially like very short wavelength modes of say, longitudinal wave number thirty or forty. So a very different length scale than the other things going on. Then, in a rather short time, you couldn't tell what was going on, it was relatively quite turbulent out in that region. So we pursued that a fair amount; it was published in 1966 in **Proceedings of the 11th International Congress of Applied Mechanics**.

In addition, as a follow-on to that, we took a somewhat larger paraboloid and sealed it to an outer circular cylinder, which we'd used for some gravity wave experiments, and used that to look into the same modes both for the case where you rotate [at] a rate that produces the same paraboloid for the free surface as for the glass base, and at rates where you rotate faster than that (von Arx's situation), and you produce a steeper paraboloid at the top surface--

Frenzen: So the depth increases from the center outward...

Fultz: Outward. Or you rotate at a sub-equilibrium rate and the depth decreases from the center outward. The upshot of that was there was not so sharply defined an instability as occurred in the other case where the depth was decreasing rapidly outward, but there was definitely an instability in all three cases. Which in the case for depth increasing outward, we eventually identified as essentially a mode conversion to a Rossby wave mode with a wave number of 4, 5 or 6.

Frenzen: Still driven by vertical oscillation...?

Fultz: It was still driven by vertical oscillations, and the question of just how that works mechanically is uncertain. Nobody, as far as I know, has tried to look into it because the--I've tried to interest theorists in trying to do this, because in principle, it's a barotropic case, should be a good deal simpler than many other meteorologically relevant cases, but the usual comment I get on that is that the theory for an oscillatory flow is much worse than for steady flows like plane parallel that have been looked at by hundreds of people as to stability.

That reminds me of another instability that has never been really fully identified, and that developed rather interestingly out of a class experiment that we designed very early in which you take a vertical elliptical cylinder (we constructed one of copper) and start from rest and suddenly start the cylinder in rotation. What happens then for a few seconds is that, relative to the room, you get a pure deformation flow, which is zero-rotational in the room. Relative to the rotating cylinder, you get a constant absolute vorticity elliptical flow and we started out with a class experiment in which we measured streak fields in both of those cases, and had students measure and show that the sum of the pure rigid rotation and the pure deformation flow add up to give you the constant vorticity elliptical flow. Or you could subtract one from the other to get the pure deformation flow.

We did that a fair number of times in the dynamics class that George Platzman taught. We added to it later the same cylinder suspended on the three-wire suspension, which was an experiment that Stokes discussed, but as far as I know, did not actually do. He discussed the theory for it, and showed, for example, that the effective moment of inertia of the flow in the oscillating cylinder is not as large as the oscillation of a solid elliptical cylinder of the same size would be. So we added that oscillation experiment.

Frenzen: That's difficult to picture. Have these been described in the literature at all? Class notes?

Fultz: Not really. Except by inference. What happened in this oscillating experiment was that we measured the effective moment of inertia and compared it with Stokes' calculation. It turned out that it's a little bit more than [what] Stokes calculates because of the effective drag of the oscillating boundary layers. But then for demonstration on those oscillating experiments, we used some ink tracer experiments and on one occasion, we happened to get ink into the whole bottom boundary layer and it turned out that the bottom boundary layer was going unstable to a series of roll cells with sort of millimeter scales because the Stokes layer was only the order of a small number of millimeters thick. These were occurring at low enough Reynolds numbers that it was a question as to how they could be unstable. They were occurring at much lower Reynolds numbers than you get in, for example, the linear oscillatory flow at a bottom underneath a wave, which a number of engineers have looked into.

Well, that looked interesting, so we set that up in a more controlled way and did oscillating experiments with a circular cylinder with controlled amplitudes and controlled oscillation frequencies, and showed that a circular boundary layer also goes unstable at a very much lower Reynolds number than the corresponding linear problem. Those cases, at least pictures from them are mentioned very briefly in a paper in 1967, in **Developments in Mechanics**, which was a proceedings of the Ninth Midwestern Mechanics Conference at Madison, Wisconsin.

Both of those types of instabilities, the ones that occur in the elastoid inertia modes, the ones that occur at the bottom in this oscillating otherwise stationary cylinder case, I don't believe have really been identified.

Another set of experiments that also really grew out of the class experiments eventually turned out to be very interesting work on standing surface gravity waves, both in a rectangular aquarium tank and in circular cylinders. They developed, in the first instance, in the rectangular tank, out of an experiment that G. I. Taylor did after the war, which he was interested in because a particular theory, that was done during the war in connection with the design of the Mulberry Harbor for Operation Overlord, had predicted that the maximum amplitude standing gravity wave would have a 90 degree angle. Whereas results going all the way back to Stokes say that the maximum amplitude progressive surface wave will have a 120 degree angle. So the question is why the difference in angle, and is it really so--this is neglecting surface tension in this theory of gravity waves. So that was part of Taylor's interest in doing these, but he showed that one could do very accurate frequencies on these and in fact, determine the shift in resonant frequency as a function of amplitude of the waves. For the particular depth-to-wavelength ratio that he was using, he showed that finite amplitude wave comes in resonant with the generator and at a lower frequency than the lowest amplitude wave or the theoretical infinitesimal wave. Now he didn't point out, but we noticed before too long that that's an interesting reversal of what occurs with progressive gravity waves, which are higher frequency at finite amplitude than they are at lower infinitesimal amplitude.

END OF TAPE 3, SIDE 2

Interview with Dave Fultz

TAPE 4, SIDE 1

Frenzen: This is tape 4, side 1, recording Dave Fultz at the University of Chicago on December 1, 1992.

We were talking about the standing wave tanks in the fixed rectangular tank.

Fultz: Yes. We organized that tank with a single flap at one end as a wave generator and used it primarily for class experiments in which it's outstandingly successful. It's very easy to get six or eight of the lowest modes and with a little internal tracer, you get a very clear picture of the change in the ratio of vertical motion to horizontal motion as the depth to wavelength ratio changes from mode to mode, and a number of other things are very effective with students. Plus the resonant frequency generally agrees with the inviscid one to a few tenths of a percent. The size was such that there were no significant viscous effects on the frequency.

Frenzen: The fact that this was all being done in a fairly short tank doesn't really have an effect?

Fultz: Well, it's not particularly short. It was about a foot wide, 3-1/2 feet long and the typical depth we used was ten inches to a foot. Something like it. It was fairly long and so the lowest mode, which was the sloshing mode that has only 1/2 wavelength in the tank is a fairly substantially horizontal motion. But what happened next was that one of the people at the Courant Institute in New York, Joe Keller, and a colleague of his, Tadjbakes, published a theoretical paper in which they predicted that this frequency effect, which is in the direction of low frequency and finite amplitude for deep water, will reverse when the water gets shallow enough to be in the direction of higher frequency for finite amplitude. So what we essentially did, Murthy and I, was test this. It really only took us a couple of months, which is unusual, an experiment that comes out of the blue like that. In fact, we were able to show that there is a systematic change in the expansion coefficient that makes the first correction to the infinitesimal frequency or period in the direction of lower frequencies for deep water and higher frequencies below a certain depth-to-wavelength ratio. In fact, we did it accurately enough that I'm fairly certain that we were able to show, from the experimental point of view, reasonably conclusively that the reversal depth-to-wavelength ratio was appreciably different than the theoretically predicted one. Somewhat shallower. Not a great deal, but somewhat shallower. That was a very interesting result that, as far as I know, hasn't been explained in the theory yet. Although a mechanical engineer named Larry Mack down in Texas has worked a good deal on these sorts of problems.

The next thing that Murthy and I did, however, was to take that same problem, which we--I kind of forget the order we did this now--but it's good enough. We take the finite amplitude effect first for a circular cylinder. That turns out to be pretty comparable to a rectangle--namely, for a sufficient depth-to-radius ratio in a circular cylinder, you've got the amplitude effect corresponding to a decrease in frequency, (increase of period), and then as you reduce the depth of the fluid layer in say, the same cylinder, the frequency effect decreases, reverses over to higher frequency at shallower depths as the amplitude increases. Comparable to Keller saying that the circular case would reverse, we found essentially the same difference that the experimental reversal seemed to be a little, though not a great deal, shallower than the other.

Now, I'm a little unclear on just what order these things occurred, but within a fairly short time, or even before, we started doing the axisymmetric gravity modes in the circular cylinder for the rotating case. For those, we didn't look into the amplitude effects. It would have taken far too much work, probably. But we did look at the effect for at least the lowest two modes on the given mode frequency over a period of increasing rotation of the circular cylinder. That's discussed in the New York Academy paper in 1962. What essentially happened was that, for axisymmetric modes, the rotation effect is supposed to be a quadratic that goes proportionally to the square of f. In fact, there's a classical calculation that was carried out by Sverdrup on the tides on the north Siberian shelf, that was the first to lead to that result. It predicts that a long-wave mode will have the square of its frequency increased by f^2.

What we found going into the shallowest layers, that we could get clearly defined frequencies for the modes--that the quantitative rotational effect seemed to be appreciably different than Sverdrup's value, and the value that's still quoted in the texts for this particular long-wave effect of rotation. We concluded that it was about 7/10 of what the classical frequency effect calls for.

Frenzen: What do tidal observations indicate--?

Fultz: It's never been verified for tidal observations as far as I know.

Frenzen: There's so many coastal effects and so forth, it's difficult to see what the--

Fultz: Right. It would be very difficult, and I'm not aware anybody has ever tried to do it for the Siberian shelf at all.

Frenzen: There are a lot of things in oceanography between theory and experiment that involve an arbitrary constant...

Fultz: So we pursued that a fair amount for the New York Academy paper. The principal result was that the higher modes have an even stronger reduction of the amount of this effect, whereas at least in the elementary theory, there ought to be no mode dependence of the rotation effects. For example, at a depth-to-radius ratio of 3/4, the frequency squared ratio is only half of what it classically is supposed to be. Now much later, a graduate student named Mona Conner did a very interesting further set of surface gravity modes in the same circular cylinder with the generating disk set out at the rim instead of at the center, in order to generate the azimuthal modes. And she found--we had always worried about whether you could generate the azimuthal modes very well at all and it turned out that they're just about as easy to generate as the axisymmetrical ones. So, in fact, she eventually determined the frequencies of the lowest, first harmonic, and second harmonic azimuthal mode out to rotations that are almost out at the limit where the free surface reaches the base in our particular cylinder.

Frenzen: What was her name?

Fultz: Mona Conner.

Frenzen: Is there any other case of a girl doing experimental fluid mechanics --

Fultz: I'm not aware of any, and I'm not too sure where she eventually ended up.

Frenzen: She did not do her degree here?

Fultz: She got a Master's degree, but she went somewhere else to go further.

But the interesting things she found, and then some observations that I (and Murthy) made later are not published; I'm confident that this is a type of experiment that really could be followed up with great frequency, very extensively. The difference of the azimuthal modes is that instead of being quadratic in the frequency, they are linear. The mode splits so there are two modes, both of which originate from whatever the non-rotating mode is. One of the modes goes to lower frequency, the other to higher frequency. She was able to verify that split, both for 0-1-0, 0-2-0, and 0-3-0. The horizontal trajectories in those waves turned out to have a property that none of the texts have quoted and nobody has published to my knowledge as yet. Namely, though Lamb already has this splitting difference for a flat bottom circular cylinder; Lamb's terminology, I think, is to call the plus mode the one that goes to higher frequency and the negative mode the one that goes to lower frequency. What happened for all three of the plus modes we looked at is that the horizontal trajectories are inertia circle in sense. That is to say, they rotate clockwise for forward propagation and an anti-clockwise rotation in the Lamb coordinates. But the negative modes, the ones that are lower frequency, are exactly opposite. They have horizontal trajectories; in an anti-inertial circle and propagate anti-clockwise for counter-clockwise rotation. That property has not been sufficiently emphasized in the elementary discussions, or any other discussions.

In addition, the curve for the positive lowest mode, 0-1-0, crosses the curve for the negative mode of the first harmonic, namely 0-2-0.

Frenzen: Does this imply they could coexist?

Fultz: Presumably. And experimentally, we know that all sorts of funny things happen in that region. Mona did some surveying out at higher rotations but in particular, the force sufficiently high forcing amplitude, and we don't know just what the implication of that is, plus near enough to these crossover points plus high enough rotation things in various regions, all hell breaks loose. The first thing that happens is that inertia modes with nodal surfaces in the vertical and with much higher wave numbers in the horizontal, set in spontaneously either at the forcing frequency or at a sub-harmonic of the forcing frequency. So this puts in these special regions, and for the particular frequencies, it essentially generates a very high wave number inertia mode out of the much lower frequency gravity mode. In addition, a lot of what starts going on looks like nothing but turbulence. And this can be present simultaneously with your being able to see in some of the trajectories of a tracer on the top that the inertia modes are still there--they're just very much obscured by all the other things that are simultaneously going on.

Frenzen: Organized motion in a turbulent medium.

Fultz: And finally, to cap the climax, quite frequently all these things going on will start to drive a rather large vortex whose speeds are sufficiently low to be almost quasi-geostrophic, so you have a driving which starts out to produce a single, pure inertia mode that ends up producing subharmonic, much higher wave number presumably still inertia modes plus turbulence plus eventually a quasi-geostrophic--

Frenzen: Zonal flow?

Fultz: Well, it's usually off-center, so it's not purely zonal. But it does have a scale that's comparable to half the radius. It doesn't extend all the way...but that state as nearly as one can tell by qualitative observation, just keeps going, presumably having all of these types of things.

Frenzen: This is all driven by the oscillation?

Fultz: This is all driven by the single oscillating disk, ______________________, right. That's the end of that, that has not been followed up yet...It has not been published except the beginning parts of it in Mona's thesis.

Frenzen: Master's paper.

Fultz: Master's paper, right.

Frenzen: Entering the sixties, you had a generous grant from NSF for three years. How did that continue?

Fultz: It continued rather well solely from NSF for quite some time until at least the middle of the decade. In the first half of the decade, in addition to the regular support, we got a major capital equipment grant from NSF for $200,000, which enable us to design and build 4 to 6 highly capable water source units that had much higher capacity than any we had had before, and were capable of holding water source temperatures steady to within a hundredth of a degree Centigrade.

Frenzen: This was Mike Fain who...?

Fultz: Primarily Mike Fain. We also designed and built a quite precise capacitive wave height measurement device to be used on the gravity wave experiments, and that was done primarily by Tony Kordecki, who was an electronics engineer and came to us from Motorola in 1963. We were very lucky to get him at a reduction in salary from what he was making at Motorola. The third thing that we designed from the capital equipment grant were the Mark IV spherical apparatus tables of which we built three or four.

Frenzen: Again, that word "spherical," although they're simply precision rotating tables or ring bearings or shafts.

Fultz: Yes, they had precision bearings, means of very careful leveling, sufficient access through the central shaft to carry not as much equipment as could be carried up through the ring-bearing gap, but an adequate amount for carrying wires from mercury troughs and things of that nature that were crucial in making measurements while the apparatus was rotating.

Frenzen: Visitors are always impressed by the array of mechanical equipment in the laboratory, but of course it is a highly equipment-intensive activity and sometimes probably it's been difficult to explain that to sponsors.

Fultz: Now the other thing that is crucial for equipment for Tom Spence's experiments is that in about the middle of the decade, the Department of Geophysical Sciences applied for a building grant. I forget the timing on this, but it took several years to go through and fortunately just made it before the program ended. I don't remember what year.

Frenzen: This was an NSF program?

Fultz: Right. In the middle seventies, so a fraction of the cost of this building came through another program from NSF. But the crucial part of it was that we were able to design with the architect, as part of the building, an entirely separate air control system that was designed with separate chillers, separate re-heat coils for six zones, all within the Hydro Lab and a platinum detector-Honeywell bridge system so that each zone could be controlled separately and if necessary, at different temperature levels. In the zones we actually activated and used for convection experiments, the zone in which Tom Spence's experiments were done demonstrated that if you didn't make too much change in what was being operated continuously, the air temperature could be held essentially within the peak-to-peak range of a tenth of a degree Centigrade. We never had anything near that sort of constancy in Rosenwald and a number of the experiments that Tom did in fact would have been impossible in Rosenwald.

Frenzen: So he did his primary dissertation work in this new facility?

Fultz: He got started with preliminary setups on one of the new type turntables in Rosenwald, but didn't get very much work done before we moved. The bulk of his experimental work was done in Hinds.

Frenzen: He was doing dishpan or annulus?

Fultz: Yes, he was doing dishpan. In essence, what he started out to do was go back to the open center pan and see what we could find out now that all sorts of things had been cleaned up.

Frenzen: These are cooled by water on the bottom.

Fultz: That's right in this case and in his case the outer wall was heated by a water bath.

Frenzen: Oh, he got away from the resistance heaters--

Fultz: So there are no resistance heaters at all involved in the apparatus itself. But only in the control. For example, water sources had a good many kilowatts of available heating to use as backoff heat. And then the control in the actual supply hose to the source on the apparatus was controlled by a feedback detector and amplifier system that smoothed off the residual variations that got to the hose.

Frenzen: So just reading the water as it entered the apparatus and compensating for any changes.

Fultz: We don't exactly know how much actual control occurred there, but it was down below a few hundredths of a degree.

Frenzen: Tom was looking at what was in general...in the open center cage to see anything like symmetry and vacillation.

Fultz: Right. So what he set out to do originally was to take this enormously improved dishpan with the measurement capability we had for at least eight thermocouples. They moved around in the pan. And first look at symmetric flow, which we knew we could do, because essentially symmetric flows had been discovered in the original dishpan survey. And then his intention was to raise the rotation rate and try to see what began happening around the transition range to Rossby regime flow. He didn't expect to be able to see anything special. What happened was that he got the pan and the apparatus Spherical six going in a crude way over in Rosenwald, and in fact got far enough to find that one of the cases he looked at was a relatively very steady two-wave vacillating state: a type of thing that had never been seen in an open center pan before. So that was an indication that something strikingly new was likely to come out of this.

Now what happened was that he got only a small number of runs in before we moved all the remaining stuff except the ring bearing to Hinds, and he moved with it. By the time he got set up in Hinds (I don't remember which year it was)--

Frenzen: I see a preliminary report in 1968, so it could have been 1967--

Fultz: 1969 or 1970, something like that. But what happened, there was a lot of stuff to get going before he could run the apparatus in Hinds. One of the things we had done in the big series for the five-wave vacillation experiment was discover that we had lots of trouble from the photographic lights unless we tried to reduce the infrared input from them, which we did by constructing running water filters that essentially took out everything that would be absorbed in 1" or 1-1/2 inches of water from the beam before it headed into the pan. There was still undoubtedly some residual getting through, but much less than would be true because the near infrared absorbtivity of liquid water is staggering in the first millimeter or two.

Frenzen: Oceans are black.

Fultz: So if you take that out, why, you do very well.

When he was ready to go in Hinds with the 61 M1 experiments, he already had the filters out. Everything was comparable to a standard run. I don't remember what he did first, but the symmetrical, no problem. One week he decided to try to go back and look at this two-wave vacillation, and he found he couldn't get it. In fact, everything switched and the only thing he could get in the range of rotations that he had used before or outside it, was irregular Rossby regime flows, like the original survey. So now the question is, what happened? It took us a month to track it down. It turned out that with Standard 4 photoflood lights that we had been using before, that the bare lights were doing something which was switching the system response to these vacillating wave states. And the filters were taking that out and switching back to the irregular flow.

Frenzen: So they were heating the fluid from above in some way?

Fultz: That immediately determined that Tom's major task was to find out as much as he could of what happened in the Rossby regime range with the bare lights. So it's not the order he did it, but the first thing he had to do, or once we'd identified that, we picked some standard conditions and looked at what forty volts on the lights would do and what twenty volts would do and what fifty volts would do, and use that as a quantitative guide as to how we should proceed. Then, the first task was to calorimeter how much would he later pick that the standard light condition was putting into the whole pan. That calorimetry was what would have been impossible, particularly in the old lab. What he did essentially was disable the outer source, put insulation there, run the flush cold source at a temperature below the air temperature, but not too much, get everything to a steady condition and then add the lights. I forget exactly how he did that one, but that one was comparatively simple. What he found was that the forty volts were putting in essentially fifteen watts, total.

Frenzen: As compared to what the other--

Fultz: As compared to whatever the liquid heat source and the cold source were taking out.

Frenzen: When those were running, were they transferring tens of watts?

Fultz: Yes, and it turns out that fifteen watts was approaching half of the total that was coming in and a similar amount going out. However the more critical thing was to get some idea of what the vertical distribution of this heating was. It clearly must somehow be concentrated in the top surface or the top somehow. And it turned out that it was not that concentrated. More was getting down but in order to do that involved a transient calorimetry, sort of comparable to one I described just now, but transient in which you disabled everything and ran the cold source at a temperature below the mean-let everything come to equilibrium with the thermocouple detector at an intermediate radius and various heights. You then turn on the lights, and what happened was essentially that the thermocouple which was perfectly steady, starts up at a certain slope and that slope rapidly decays within tens of seconds. So now you look at the local rate of change of temperature at that location, extrapolated back to the value at zero time when the lights went on. Then you move to another position and you have to wait three, four, five hours because the whole thing doesn't come back to equilibrium from the excess heat that went in maybe two minutes for that many hours. Then you repeat the process in order to get a heating profile...

The profiles were fairly surprising but we finally convinced ourselves that they were reasonably good. The total depth for the series of experiments that Tom ran was five centimeters. The heating rate in milliwatts per gram was, for our standard condition of forty volts on the lights, highly concentrated with more than 90% of it in the uppermost centimeter. It then declined less rapidly from the fourth to the third centimeter. Then it became, as nearly as we could tell, uniform all the way down to the bottom. The conclusion was that there was diabatic input to the pan, which was essentially trying to change the static stability of the upper two centimeters at a very rapid rate and not to change the static stability of the lower three centimeters at all.

Frenzen: I'm surprised it was that deep actually. It was a radiant input.

Fultz: We don't know the separation fractions that are doing these things.

Frenzen: But this was enough to put it into that two-wave vacillating state?

Fultz: It became even more striking as soon as Tom began systematically looking at both rotation rate and temperature difference between the baths, keeping the bare lights fixed. Because what he found was in essence that there was just as sharply defined a wave number change spectrum in this open center pan with the internal heating as we had customarily expected in annulus cases. There was a very sharply defined symmetry transition curve with an upper and lower branch. The wave number transitions we didn't look at as extensively because there wasn't time, but they seemed to be wherever we found them just as sharp as occurred in annuli. We had all possible wave numbers, or most possible numbers, three-waves, two-waves, one-wave--and then vacillating versions of these. So the crucial scientific question that maybe comes out of this, that has not been tackled in any serious way yet, is what about this change in the boundary conditions of the system producing this drastic change in response from essentially a narrow spectrum annulus-type vacillating steady-wave response to a broad spectrum irregular Rossby regime response, which was the only thing we saw in the other case. And we verified in fact by a couple of check runs, that if we ran the water filters with a substantially higher voltage on the lights, we could tip the system response over into irregular even for the water filters.

Frenzen: Overdrying it...

Fultz: We did a vertical profile for the heating for one case there, and what it showed was that the excess in the upper centimeter was reduced to only a factor of two or three times the uniform input in the lower part of the layer.

END OF TAPE 4, SIDE 1

Interview of Dave Fultz

TAPE 4, SIDE 2

Frenzen: This is December 4, 1992, recording at the University of Chicago.

When we were last talking, we were reviewing some of Tom Spence's work just about the time that the Hydro Lab was moving out of the old facilities in Rosenwald to the new building where it is now, in Hinds. That was in the late 1960's.

Fultz: Primarily in 1969.

Frenzen: What was some of Tom's other work?

Fultz: Somewhat earlier, he did some very interesting experiments on the dispersion of an ink pulse in some annulus and also open-center dishpan cases that indicated one would finally analyze them to essentially make an estimate of the time required for an ink pulse to be uniformly distributed over the whole disk working fluid. We found what appeared to be a pretty reasonable non-dimensionalization for that that suggested that a corresponding pulse in the atmosphere would disperse to uniformity with--what we used was a residual root-mean-square difference of concentration of not more than 1%. The time that the most realistic experiment suggested for such a dispersion, comparing it, say, to the troposphere and one hemisphere, was two years. That's a rather interesting order of magnitude.

Frenzen: So this was really global air pollution work sponsored by the Public Health Service.

Fultz: Right.

Frenzen: At a time when some of the other lab support was declining.

Fultz: That's right. NSF was getting overloaded and we had to look around. Finally, we got a fair fraction of support from both the Environmental Science Services Administration [ESSA] for, in fact, some double cylinder work, and from the Public Health Service for this air pollution-type of dispersion experiment.

Frenzen: After that, I know that Tom went on with some open cylinder work as well.

Fultz: Yes, he continued the series with bare lights that he discovered shortly after we moved into the new building. After determining fairly extensively the existence of the wave number change spectrum and of symmetry transition curve on that, he essentially concentrated on two special cases. One, a symmetric one, which was what he originally started out to do. The other, a two-wave vacillating state that looked periodic enough to try to do the same sort of thing we did in the annulus five-wave vacillation case. We weren't equipped to do anything quite as elaborate as the five-wave case, but Tom, with a combination of ink tracing, Baker's thymol blue tracing technique and streak photographs of powder floating on the top surface, was able to get a fairly complete picture of the full two-wave cycle, which had among other interesting features, a very long period of almost 100 revolutions. Which brings it up almost into the seasonal time-scale range. That was sufficient for him to be able to estimate a crude interior temperature field, crude interior velocity fields, and from those to estimate enough of the terms in the sort of zonal eddy energy transfer budget that Norman Phillips introduced in connection with his original numerical general circulation experiment. We couldn't measure everything in that, but we were able to measure enough to deduce the rest by assuming steady state over any integral number of cycles. In the course of analyzing that, we in fact found a way of scaling those quantities to non-dimensional measure that made the zonal eddy energy budget for this two-wave experiment come out just about where you would expect it to be relative to the atmosphere. Somewhat higher in the energy quantities and lower in certain transfer rates because it was a rather simple wave state compared to the broad spectrum you see in the atmosphere/troposphere.

Frenzen: I see a 1977 paper that you wrote with Tom in the **Journal of Atmospheric Science**, experiments on wave transition--

Fultz: That's the main summary that's been published so far on that one.

Frenzen: Jack Kaiser was another one who was around for a few years in the new Hydro Lab in the new building.

Fultz: Yes, he continued the small annulus work in much more favorable circumstances in the new building, and did some very detailed internal temperature fields, especially over a whole range of symmetric cases plus a lot of measurements including boundary layer measurements on some of the wave states.

Frenzen: Jack Kaiser and Tom Spence were the last two PhD's out of the Hydro Lab. I gather that support was disappearing at that time.

Fultz: It was becoming very difficult to put together enough money to operate the laboratory because of the auxiliary people and things that were necessary.

Frenzen: We always had a staff of between five and six.

Fultz: Five, six, something like that.

Frenzen: Without that, the lab couldn't operate. Plus machine shop supports and photography and all of those ancillary costs.

Fultz: In 1972, NSF was in a serious crisis, so they reduced our grant and the other two were withdrawn entirely. I don't know the reason for ESSA but the Public Health Service grant, though we had just gone through a site visit resulting in a favorable report, also changed their plans in connection with the transition to be under the Environmental Protection Agency. So what that did was kill off over half of our support, and that made it impossible to keep the laboratory together.

Frenzen: Tom Spence finished on his own, or with some additional support?

Fultz: He finished with some help from one of the other faculty members, Joe Pedlosky.

Frenzen: But you went on. I see you did some work for the Namias Symposium, that was published in 1986, on air mass residence times. How did that come about?

Fultz: That was something that arose in connection with Judy Curry's thesis for Hsiao Lan Kue. I was on her committee and we had lots of discussions about her problem. In essence, she set out to do a much more complete job of calculating the suitable radiative model to apply to the conversion of maritime polar air into continental polar air, for example, over the Arctic Ocean. That amounted to an attempt to update two papers by Harry Wexler in the thirties which were very simplified models and nobody had tackled the question in between for some reason. The results generally were that Wexler came out with an estimate of 25-30 days to produce a reasonable continental-polar sounding, assuming a start from initially maritime air. Judy found that it was quite a complicated business because you have to deal both with the turbulent processes in bottom parts of the boundary layer and with very high stratifications, which are, I think, not certainly anywhere as well-understood as neutral stratifications.

Frenzen: Did you draw ideas from the model experiments--?

Fultz: A little bit, in connection with the dispersion rates. But the prime question that came up was that Judy came out with the somewhat faster estimate of something like two weeks being sufficient. And the question that occurred to me was--

Frenzen: Other than how can that be?

Fultz: --if the time were that long, how are you going to get the air to stand there in one place long enough to produce anything? In particular, source regions in the Northern Hemisphere would be, say, Siberia plus Northern Canada plus everything north of 70. So the idea I had was to take the general circulation statistics that are now available, compiled by A. Oort and a number of other people for a couple of decades of upper air data, and see if we can't estimate the actual residence time, the actual mass flux function of latitude, and see for example what the actual average time spent by air north of 70 north is. Now that can be done geostrophically. In fact, Hurd Willett, in the early thirties, was doing precisely geostrophic calculations of such mass fluxes, though he didn't express it that way, from surface data. And those turned out in fact for residence times north of the 50 or 60 to be six, seven, eight days, which is just about the ballpark we found from the upper air data for the whole troposphere and low stratosphere. But you can also get directly from the wind statistics without making a geostrophic assumption. So I did it both ways, and as a function of latitude.

So two primary results among a lot of other detail--or three [results] perhaps--were that the residence times for example in the Northern Hemisphere, even for just the low levels, are very short north of 60. Four, five, at most six days, so the question is, If it's really that short, how can you build up a big dome of real continental polar air? And that question, I think, is still open. The other thing that comes out very clearly from these statistics is that the air flux across the equator is high enough that the residence time for a hemisphere is not the one to two years that a lot of the chemical box models have estimated, but is something more like 45 days to 60 days. 45 days if you include the upper trophosphere and the lower stratosphere. That's a consequence simply of the fact that the root mean square of the meridianal component on the equator is not one or two or three centimeters a second, but is more like two meters a second.

Frenzen: A fair amount of air moving back and forth.

Fultz: Right. That also is a question that really has not been resolved.

Frenzen: There's a fleet out there now in connection with TOGA.

Fultz: The third interesting aspect that comes out is that any middle latitude region whatever of the general size of the typical mid-latitutde cyclone system is small enough that the air residence time in it is down in the range of one, two to three days. That means that a great deal of the structure as described in the original Norwegian model must be formed dynamically and cannot--there's not enough time to form it by what originally was thought to be *in situ* diabatic processes.

Frenzen: Well, there's more work to be done there. And here I see you did a little paper on calculator gradient winds. How did that come about?

Fultz: That came out of the dynamics class. In discussing the gradient wind when we first reach it after discussing rotating frames of reference, I discovered in the middle sixties that there's a non-dimensionalization of the gradient wind equation which reduces it to a single equation with only two dimensionless variables, one of which is a dimensionless gradient wind and the other is a dimensionless geostrophic wind.

Frenzen: This simplifies the calculation?

Fultz: It makes the calculation universal and dependent on a single quadratic equation so you can calculate numerical tables, and with the proper choices of sign, the same expression holds for both of the ordinary cyclone-anti-cyclone cases and both of the anomalous cases. Plus it becomes very clear what's happening as you go from ordinary to anomalous cases and you can calculate a numerical table that describes, or will fit any case whatever no matter what the scale.

Frenzen: Details of this are all in the **National Weather Digest** of 1987, volume 12.

Fultz: That's rather sketchy. The important ones are in **Journal of Atmospheric Sciences**, volume 48, 1991.

Frenzen: You've mentioned teaching classes. I know you taught the hydrodynamics courses here for a long time, and you taught some probably very popular and entertaining undergraduate courses using some of the facilities of the Hydro Lab.

Fultz: Yes. Other members of the faculty took over the main geophysical fluid dynamics course sequence fairly early on and I was mainly involved with the initial undergraduate level sort of combined phenomenological-dynamical discussion. There was a sequence of three courses of which one I usually had in the winter and discussed dynamics of the atmosphere and introduced students to maps and analysis of them and theoretical interpretations of a number of such things. The third was a comparable course in the spring which gave the same sort of intermediate level of discussion for oceanic dynamics, oceanography. Sort of a combined descriptive-dynamical course in which we both looked at the most important observed phenomena and at the more elementary models that appeared to explain some fraction of what goes on.

Frenzen: But the courses in which you actually did experiments in the Hydro Lab, like the standing wave and things like that, were they in that sequence?

Fultz: No, those were partly in the hydrodynamics sequence and partly for two or three years, we in fact constructed a specialist experimental course in which those experiments plus a number of others were deliberately done.

Frenzen: You really taught "Hydro Lab."

Fultz: It was broader than that. We did terminal velocities of spheres for a whole set of circumstances, including going back and getting out [Isaac] Newton's data from the Principia which turns out in fact to be quite comparable to modern data when you convert it. It shows how prescient he was--he wrote those results up, which must be some of the first quantitative hydrodynamic experiments.

He wrote those up in such a way that in fact you could deduce what the drag co-efficients were from the data he gives.

Then on the different years the course was given, there were one Benard convection experiment and the standing gravity wave experiment we were talking about earlier plus a Taylor double-cylinder experiment that in a small plexiglass apparatus works very nicely.

Frenzen: I know you also included some Hydro Lab demonstrations in some of the undergraduate courses, some of the introductory courses taught here in the geophysics department.

Fultz: Yes. The introductory ones that I was involved with concerned atmospheric dynamics and oceanography. The atmospheric course was the first place students would run into the question of rotating reference frames. One experiment that I always did for that course was a setup of partly demonstration and partly measurements by the student of inertia circle patterns and the absolute motion patterns of a ball bearing rolling on either a stationary paraboloid or a rotating paraboloid.

Frenzen: And the student has the advantage of seeing this in the rotating system or in the laboratory system by using or not using the rotoscope.

Fultz: That's right.

Frenzen: So it's a very graphic demonstration of effects of rotation. They did some experiments in which they used the actual dishpan experiments as well?

Fultz: We did that once or twice.

Frenzen: Jetstream, long waves in the westerlies?

Fultz: It's really a little bit too complicated for a significant class experiment at that level. You can give them just about as good a feeling for the cases if not better with something like the movies.

Frenzen: I was going to say that. The motion pictures are so detailed and there you take the time to look down in the fluid and see the fronts go by.

Fultz: You can see the range of cases which are not feasible to show in a class.

Frenzen: --shutting down the apparatus and cleaning it and starting over.

You made a film for the National Research Council; what is that series?

Fultz: No, it was for a national committee that grew out of--it was a post-Sputnik attempt to beef up science teaching in high school and colleges--Physical Science Study Committee.

Frenzen: I think we mentioned this film before, briefly, in these films. What did it cover actually?

Fultz: The program that was organized by this committee, the National Committee for Fluid Mechanics Films, eventually did 24 such films, about a half-hour in length each. I did one on rotating fluids. The characteristic procedure of all the individual committees for each film was there was an advisory group that went over outlines prepared by the principal, essentially with a fine-tooth comb. And with some very searching questions. In the case of the rotating fluids film, it took about a year and a half of back-and-forth on that to get the preliminary script. That process is very effective because, depending on what the target audience was, the principal might know almost nothing about what they would already know.

Frenzen: Yes, that's of course a big mistake one makes, to presume something on the audience that they do not know. And if you're trying to make demonstration films for a high school audience or even first year of college--

Fultz: The intended audience was college in the case of these films.

Frenzen: What sort of thing was in the rotating fluids film?

Fultz: Unfortunately we decided with the advisory committee that it just wasn't feasible to do any thermal convection. So we had to do essentially barotropic fluid experiments and I'm not sure that I can remember in what order they were, but the film opened with an ink-pour into a non-rotating spherical flask which was stopped down so you could only see the center of it. Then a repeat of that with the flask rotating, which you couldn't tell until the ink went in. The ink goes immediately into Taylor walls...

Frenzen: Instead of diffusing in a cloud.

Fultz: We did a lengthier sequence of Taylor inkwalls later in the film.

Frenzen: Do any flow past obstacles?

Fultz: Yes, we put in a neutrally buoyant pingpong ball, and towed it on a thread transverse to the axis, showing a difference between a terrifically low Rossby number and a higher one in which case it deflects.

I remember the other one we started out with. We did the case of generating a small vortex ring and aiming it across a diameter. This was in water, so it was an ink ring.

Frenzen: But it was a vortex ring.

Fultz: This was a case that Taylor did in one of his rotating experiment papers in 1919 or 1920. But when you're rotating and the lighting was arranged in this film so you couldn't tell the lights were rotated with the--

Frenzen: --the shadow.

Fultz: You couldn't tell the difference between a rotating tank and a non-rotating tank. And the vortex ring swings violently off to the right for a counter-clockwise rotation.

Frenzen: I used to put my kids on the merry-go-rounds in playgrounds and play catch with them with a tennis ball.

Fultz: Then, we towed the pingpong ball parallel to the axis and showed the difference between the Taylor column in front and the Taylor column behind. We also did the case of a freely buoyant ball rising rapidly when the tank is stationary, and slowing up and rising very much more slowly when the tank is rotating.

Frenzen: To my discredit, I've never seen this film. Are these all available still?

Fultz: As far as we know, they are. The series was distributed by Encylopedia Britannica Films. I'm pretty certain that they're still available.

Frenzen: Increasing awareness of the deficiencies of our introductory education. Something like that could be resumed.

Fultz: But we included several inertia-mode sequences, one of which was one of those we were talking about in which the number of radial half-wavelengths was rather large, done in a parabola. The sequence we included in the film was one that was forced at insufficiently large amplitude to go unstable. So we were able to show a perfectly circular inkband initially oscillating stably with the inertia oscillation, then going into these high wave number azimuthal instabilities.

Then, a very important sequence that we did, probably about next, was the Rossby wave sequence in which we had an annulus with a center cylinder and a conical base around a ring with a rather tall depth-to-radius for a gap ratio, and we put a small mountain maybe of wood 5 degrees or so, whose maximum height was about 1/40th of the total depth.

Frenzen: A ridge.

Fultz: A radial ridge. And this meant we had to generate the motion by rotating at a reasonably high rate until we got rigid rotation, and then following the protocol, determined empirically, of reducing the rotation rate at a sufficient rate to keep a more or less steady relative zonal flow...and produced a five-wave train stationary with respect to the ridge and that could be seen both in aluminum powder on the top surface and in ink injected at mid-depth.

Frenzen: It's funny, from the pure point of view of doing a demonstration of effective rotation of fluids, that's all right. But if you want to do it from the point of view of geophysics, one would have wanted to do have driven a pi connection and not had to induce these artifices.

Fultz: That's right.

Frenzen: Since mountain ridges do not move on the earth.

Fultz: Right.

There's a very nice sequence in it in which we demonstrate a pure Ekman layer at the base of the circular cylinder. By photographing in obliquely with a rotating camera and dropping dye crystals which fall, leaving a set of columns of dye all the way down to the bottom, and then spinning the rotation of the container up. That means the bulk of the fluid is rotating slower so in the new cylinder frame, you have an anti-cyclonic flow which can be seen to be essentially barotropic because the dye trails remain vertical. Then in the Ekman layer there's a rapid spiraling out of the dye that corresponds, essentially to a frictional secondary flow that corresponds almost certainly completely to the original elementary Ekman solution, except it's in circular symmetry.

Then we ended up the film with a rather elaborate cylinder split disk experiment that was very difficult to do. It was something like the concentric sphere experiments I did at Cambridge in 1958, but is much more sensitive. Consequently, they tried to do it at the studio and couldn't, so we had to arrange to do it here.

Frenzen: But the films were not all made in the Hydro Lab.

Fultz: No, a fair number of the sequences were made in the studios of what at that point was called "Education Development Service."

Frenzen: You had to take the apparatus there.

Fultz: We lent them the rotating table which we had to spare. "Spherical 7." The split disk experiment was very striking when we finally got it to work. It consisted of a solid plexiglas cylinder and rigid lid with some access holes on the axes. Then at the bottom, a flush stainless steel disk which was about half radius of the wall of the outer cylinder.

Frenzen: That's why you called it the split disk.

Fultz: Right. The case we ran was one where we ran the outer cylinder at quite high rotation. I don't recall, but it was probably about 1500 rpm. Then we had a differential drive arrangement so that the disk could be driven in relative motion at 1% or 1-1/2% or 1/2% of the basic rotation. Either slower or faster. The more stable one is the one where the disk is slower so that was the only one we really tried to do.

What happens then is the Taylor column over the disk comes to a rotation rate which is probably about halfway between the outer cylinder and the disk itself. That means that the disk is rotating slower than the column above it. That means the relative motion of the column with respect to the disk is cyclonic. That produces an Ekman inflow which then fills the Taylor column above the disk and flows uniformly on the axis toward the top of the cylinder. On the other hand, at the upper lid, which is rotating at the outer cylinder rate, that's faster than the Taylor column. There's an Ekman layer out there which takes fluid coming up the inner column, takes it out to the radius of the disk and then proceeds as a free shear layer down to the outer edge--

Frenzen: --the outer wall of the Taylor cylinder.

Fultz: Outer wall of the Taylor cylinder. In the interior of the fluid cylinder. That goes all the way down to the disk and completes the circuit by going inward.

Frenzen: You trace all of this with ink?

Fultz: Yes. All you have to do is inject a little ink at the top. It spreads out, goes down the free shear layer, comes inward on the disk and fills the inner Taylor column from below. So that made a rather striking sequence and we ended the film by cutting the rotation rate, in which case there are secondary flows all over the place, and the ink column gets first wound around and torn up and eventually goes unstable.

Frenzen: Like the final burst of fireworks on the Fourth of July.

END OF TAPE 4, SIDE 2

Interview with Dave Fultz

TAPE 5, SIDE 1

Frenzen: This is December 15, 1992. We're at the University of Chicago. This is the last tape in the series.

Dave, are you ready to recap the whole thing?

Fultz: That's sort of a tall order.

Frenzen: For example, hydrodynamics model experiments certainly offer some very interesting and intriguing results. What are the advantages in this day of large-scale numerical models? Are there some things that we can still say the models can do better than the large computers?

Fultz: I think so. However, it's a very difficult row to hoe. The thing that the rotating convection experiments are capable of, if you're properly set up, would be to look at very long time scale phenomena, either irregular ones or something like vacillating states. I think it's certainly true that there seem to be qualitative differences among the various types of vacillation states. But understanding precisely what those are and how they operate is very rudimentary; nobody has looked at it seriously.

Frenzen: I suppose one thing that you could do is require a numerical modeler to model the hydrodynamic model, force him to view reality.

Fultz: I've tried to talk them into that often enough. Up until just a few years ago, it really was almost unfeasible because, for example, Gareth Williams, who had done the most extensive numerical experiments, did one of Hide's steady wave cases. Numerical. It took 300 hours of machine time just to get to the steady state. They weren't about to commit that sort of time to--

Frenzen: Of course, even in the last few years, that sort of time requirement has gone down and down. Every year it goes down by a factor of ten. So maybe it is feasible.

Fultz: Now it would be much more feasible than it was fifteen or twenty years ago.

Frenzen: These long--you mentioned the requirements on the experiment--that's a matter of constant temperature in the laboratory and absolutely smooth rotation. All of these physical circumstances that a well set-up laboratory would have to have.

Fultz: For example, one of the test cases that would be an obvious place to start is one of Tom Spence's open center vacillating cases, where the cycle periods are on the order of 100-200 revolutions or days. Among some of the other things that we did, we had a 2 mil platinum wire in for tracing with thymol blue methods. We found that the wire was capable of producing a gradual damping of the vacillation amplitude such as if it was raised to the upper two centimeters the vacillation would damp out after 20-25 cycles. In other words, 2,000-2,500 revolutions.

Frenzen: Of course the scale of the wire on the scale of the model is like a small mountain.

Fultz: Yes, it is rather large, but one of the things that we found was that the electrical pulse--we used a standard six-volt one-second pulse to produce a color band along the wire--

Frenzen: Yes, an electrochemical reaction at the wire that reacts with the otherwise translucent, transparent dye in the fluid to produce a trace from the wire.

Fultz: It's the proton transfer in the dye which has no gravimetric effect whatever so it's a truly--

Frenzen: No change in density.

Fultz: --truly neutral tracer. But the intriguing thing was that we found that one or a few of those electrical pulses would re-excite vacillation. And then if you did nothing further, leaving the wire at the same place, they would damp out again. But in essence what happens is that you have a very high wave number, high frequency relative to the phenomenon pulse, the electrical pulse, which excites a global oscillatory response in the pan at periods very much longer, and that oscillatory response then decays out over 2,000-3,000 revolutions, which means that you have a system that is reacting in a global way to some very slight perturbations and it's an obvious case for trying to work out a whole series of null experiments to try to deduce what the mechanism of action is. A direct numerical calculation that both has the proper scale about the wire and resolves the large-scale flows would presumably have to be some sort of double or triple grid arrangement which is very dense near the wire and much less--

Frenzen: Nested grids.

Fultz: --much less dense away. The number of points involved, if you assume, for example, that the wire has to be resolved to the order of a few tenths of a millimeter all the way around and well away from it, that will give you an order of hundreds of thousands of points just around the wire. Then, whether you can match that to another grid is a numerical question that the direct calculation as a whole, I think, is probably still not feasible numerically.

Frenzen: It certainly is an intriguing situation, isn't it, when the global pattern is essentially meta-stable.

Fultz: Right. Plus just the knowledge that these internal heat distributions, which are not a whole order of magnitude different from the diabatic rates in the atmosphere--

Frenzen: You mean the one associated with the shielded and unshielded overhead lights.

Fultz: Right. Somewhere in between our standard I.R. heating from the bare lights and say the equivalent total heating from filtered lights, there's presumably some sort of distribution which will divide, separate the response from completely irregular Rossby regime on one side to these annulus-type responses on the other.

Frenzen: I realize it's probably a no-no to draw direct comparisons with the atmosphere, but in that case, it seemed to me when you had it a homolog, if not an analog, of a little more heating in the stratosphere, which would totally change the pattern of the troposphere.

Fultz: Except that in the two meridional temperature fields that Tom was able to do, one for the vacillating case and the other for the symmetric case, there was no direct trace of the stratosphere.

Frenzen: I see, no tropopause.

Fultz: No tropopause. In fact, minimum stratification was at the top. Where the maximum stratification tended to was being put in by the lights.

Frenzen: --then heating in the upper troposphere. Once again, this meta-stable state easily driven by small changes in the atmosphere. Very intriguing.

Fultz: By changes so small that they begin to look like the sort of noise they put in to the numerical predictability experiments down at a level that's never going to be resolvable.

Frenzen: The fluttering butterfly.

Well, of course, this points up again the need for precision in the installation itself. As a teaching tool, of course, I believe there are units that are being used from time to time at any rate in other universities.

Fultz: Quite a large number of them indicated interest in that. I'm not aware of any that actually have something operating, even for teaching purposes.

Frenzen: Well, I remember the impression I first got when I saw that there were waves, jet streams, fronts all going on in this device driven only by meridional heating and under the influence of rotation.

Who is doing research today? I know the group at Florida State, Dick Pfeffer and his group. How active are they? Have you been down there lately?

Fultz: I haven't been down there lately. They certainly are turning out a substantial number of papers; one or two of them a year are on the convective experiment, but aside from them, I'm not aware that there's any operation left anywhere.

Frenzen: Before we leave Florida State, I visited there many years ago when he was getting very complex, putting multiple grids of thermocouples--I suppose he's getting the same effects of having the sensor in the fluid.

Fultz: Undoubtedly, and I think they're kind of retreating from putting in quite so much, trying to do it without directly resolving the fields.

Frenzen: Among the other laboratories that were active, Ray Hide was still doing something at the Met Office some years ago.

Fultz: Yes, but I believe that has probably ceased or perhaps changed when John Mason left the director-generalship of the Meteorological Office. Several years ago, Hide apparently moved to Oxford, and as far as I know, there's no continuing operational experimentation at the Met Office.

Frenzen: There was the group at Maryland, Al Faller. Bob Kaylor was with them, but I guess Bob is mostly in numerical work now.

Fultz: Right. They had the same sort of financial troubles we did and had to sort of batten down the operation. Bob, I believe, transferred, actually, to another department, so he's primarily doing programming and numerical work jointly for Al Faller's department and whatever the other one was. Again, every once in awhile, there's an experimental paper out of Woods Hole using von Arx's old apparatus or others but I have the impression that it's just when somebody gets an idea that they want to run simple and fast. They do that and as far as I know, none of them are thermal convection experiments, which--

Frenzen: It does require a peculiar brand of individual. In this letter summary that you showed me, you do address that. It requires of the student sort of a long-term dedication. The student himself has to be what we used to call a "gadgeteer," as much as a student of--

Fultz: That's always a problem with any experimental operation. You get interested in it because you're interested in making things happen with physical objects and a large number of scientists tend to get so immersed in the gadgeteering that they forget about what the ultimate question is going to be.

Frenzen: One physicist once said that the best experimental apparatus is the one that breaks down five minutes after the experiment is over, but that doesn't work in fluid dynamic modeling because of the precision with which you have to work. Perhaps it's more like astronomy.

Other people: you mentioned someone at Arizona, Don Boyd?

Fultz: Yes. They were doing, according to published record, some experiments essentially of stratified flows with obstacles. And also some rotating stratified flows--I forget quite what the point of those were.

Frenzen: The others--they were non-rotating stratified flows.

Fultz: Yes, both. Some were rotating, some non-rotating.

Frenzen: And of course Bob Long at Johns Hopkins was doing some things off and on over the years. Again, not so much in rotating, but a lot of non-rotating things--

Fultz: A major fraction of his work was in stratified flows, internal waves, hydraulic jumps, things like that.

Frenzen: Which makes me think of the big group at EPA, Raleigh, where they've done the wind tunnel work of flow and valleys past power plants and things like that. And they did stratified flow models of flow past over a ridge and past a conical hill; they attempted to reproduce a field experiment. That's about eight or ten years ago.

Fultz: Yes, and I gather just from random things I've seen in the published record that they've managed to survive and they're still doing some work of that kind. The conical hill thing actually was done by a man named Abe in Japan in the late thirties, early forties.

Frenzen: Stratified flow past the conical hill?

Fultz: Right. As a model of Mt. Fuji, which is certainly a conical hill.

Frenzen: With religious overtones.

Suppose there were a facility dedicated to geophysical fluid dynamic models with a long-term budget, perhaps something that would be supported by a number of universities. What sort of work might they undertake? Long period oscillations?

Fultz: Certainly, it's most difficult to do properly. The thermoconvective phenomena are undoubtedly the richest in unexplored problems, and something like followups on Tom Spence's open center experiments would be the obvious first step and could go in many different directions from there. There are plenty of not well-understood barotropic phenomena. For example, one of the things I'm going to do is finish writing up the free shear layer paper on our concentric sphere work that Derek Moore and I did. And in that case, there's not only the statement, sort of free shear layer in combination with Ekman layers on the two spheres but also a variety of instabilities that in part aren't new but could form probably as rich a subject for theoretical investigation as the Taylor double cylinder thing. For example, when the inner sphere is rotating faster, the free shear layer goes unstable and becomes quite wide compared to what it is at the same Rossby number in the opposite case. One of the varieties of instability takes the form of a whole row of line vortices that essentially cuts to half a __________________ in what would otherwise be the return free shear layer from the small sphere equator to the surface of the outer sphere.

There would be a whole suite of possibilities, I think, that ultimately could be investigated as a sequence of instabilities starting with ones that are pretty close to the simplest double cylinder thing to others that are much more enigmatic and certainly not fully understood.

Frenzen: Wouldn't some of the models in the pan again be related to some of the ideas in chaos? Or would they fall in a new area just before chaos, so-called complexity? Symmetry-complexity-chaos.

Fultz: I think certainly both because the old irregular Rossby regime experiments were certainly chaotic and there are all degrees in between those and the sort of very regular laminar periodic flows that occur in the annuli or in Tom Spence's case. Certainly the question of the onset of irregular regime and the open center pan is a basic one. It essentially asks what circumstances determine when you go chaotic out of an annulus type periodic response.

Frenzen: One of the things you mentioned in the letter you showed me--letter summary of the program--was the advantages of joint thesis-sponsor arrangements where the laboratory co-sponsors a student with a member of the theoretical faculty. Can you cite some examples of that; you did something with George Platzman?

Fultz: The two mentioned in the letter are probably the principal ones we ran into. In both cases they involved surface gravity waves on a single layer and what got us into the thesis work that T. S. Murthy started on and that D. B. Rao did--in Rao's case, what he did was re-calculate the normal modes for a rectangular layer and we set up an experiment so that he could verify the period for a couple of the lowest modes, a couple of the higher modes.

Frenzen: Perhaps this structure of joint sponsorship provides a mechanism for students to cooperate with a large central laboratory.

Fultz: In principle, yes.

Frenzen: Dave, I've known you for many years and I'm just curious as to what you're going to do next. You never stop.

Fultz: Well, I expect the main thing will be writing. I've got plenty of backlog.

Frenzen: I know you and your wife have two homes, one here in Hide Park near the University and one out in Indiana near the Indiana dunes. Do you think you'll settle down as Indiana farmer?

Fultz: Well, not likely. We'll continue this dual country-city existence as long as we can.

Frenzen: What about teaching? Will you undertake that course now and then?

Fultz: I did this past year. There was sort of an emergency with one. It's probably not likely unless something comes up.

Frenzen: Always willing to step in.

The preceding was taped on December 15. It's now January 5, 1993, after the Christmas-New Year's hiatus. We're here at the University of Chicago again...and we'll answer some of the bread-and-butter questions that were listed in the suggestions of how this interview should have been conducted.

We covered some of these things lightly and I came back to them, but I don't feel like I ever got a complete answer from you. The interview is in part dedicated to finding out, it appears to me, what makes a scientist become a scientist. So here's a question in the skeleton question list: Were there any secondary school teachers or perhaps other people who had a particularly strong influence on you in science or in other fields? Now this is fairly early. You did mention the famous mathematics teacher at Hide Park, Beulah Shoesmith. I think I just heard someone, I think it was Steve Allen, interviewed on public radio, referring to his memories of Miss Shoesmith. Do you have any specific memories in relation to her or courses she taught or things she said?

Fultz: Nothing very specific, though the one thing I do remember we did for her was build a slide rule by hand out of balsa wood.

Frenzen: Were there science clubs or anything in those days at Hyde Park High?

Fultz: Yes, but I don't remember too much of it because I was only there for the one year, the last year. But Miss Shoesmith certainly had a math club of some kind. Then earlier, I can remember a botany teacher down in Salem, Miss Phillips. And a geometry teacher in Maywood plus an ancient history teacher; the names I'm not quite able to drag out. But then the ones who really influenced me considerably at Hide Park were the chemistry teacher, Mr. Fiedler, and the physics teacher, Mr. MacLane.

Frenzen: Did they suggest that science should be your career?

Fultz: Not directly, no.

Frenzen: You did expect to go to college from a very early age. You weren't the first in your family to go to college.

Fultz: No, the family in fact of the preceding generation, all of them, went to college, which was quite unusual.

Frenzen: For those days.

Fultz: Yes, that would have been the 1890s, 1900s, and so on.

Frenzen: What did you expect your college education would lead to at the outset?

Fultz: At the outset, I didn't have any idea.

Frenzen: But then at what point did you really decide to be a scientist? When did you decide to major in science? Of course, I remember Chicago in those days had a certain degree of freedom that many schools have now in undergraduate years in the college structure.

Fultz: It's a little unclear to me. I entered with a vague idea of majoring in economics, but I think I must have changed ideas sometime in the first two years.

Frenzen: I remember you saying that once before but I guess I thought at the time that this was a paradox that many meteorologists end up going into economics! You may be one of the few who goes the other way, to meteorology.

Fultz: Of the actual accidents that got me into it, I think we went over this where I shifted to chemistry and got a Bachelor's degree there, essentially in 1941. In the course of doing that, I had an NYA job which involved posting the Weather Bureau maps over in Rosenwald...

Frenzen: NYA?

Fultz: National Youth Administration, which was a division of the WPA.

Frenzen: So that helped you finance your early days at school.

Fultz: A little bit.

Frenzen: Well, we know why you chose the University of Chicago. One grows up and lives here...

I remember you mentioning earlier how you worked for the Weather Bureau, but we never really established, I believe, how you really became interested in laboratory models of the atmosphere. Thinking in terms of your thesis work, which was theoretical...

Fultz: And synoptic, really.

Frenzen: Synoptic—constant—absolute--vorticity trajectories.

What were the circumstances? Can you remember the exact events or people? Presumably Rossby would have been involved.

Fultz: Yes, and Byers because the first experimental research project I was involved with was supported by Weather Bureau funds that Byers had acquired. And I suppose that may have been part of the interest; that was essentially a two-dimensional thermoconvection experiment that was designed to arrange line sources of heat, cold, (it never got to more than two) that John Phillips had constructed.

Frenzen: Did you go to work for John Phillips as--?

Fultz: As a student assistant.

Frenzen: --and the project was funded though under Horace Byers.

Fultz: Right. The general interest was in cumulus dynamics. That was a little later, but just about the time that some of the first similarity solutions for point and line sources were constructed in Germany by Schmidt and by Hunter Rouse and his students at the State University of Iowa.

Frenzen: What year would that have been when you really started? After your PhD?

Fultz: It would have been late 1945 and then after I came back from the Air Force, early 1946.

Frenzen: So you started in these non-rotating convection models. When did you switch over to rotating systems? What led to that interest?

Fultz: That would have been late 1946 or very early 1947 when after a session that I had with Rossby and Victor Starr at their suggestion in which both of them asked me if I would be interested in trying to construct a mixing experiment in a spherical shell, suggested essentially by the ideas that Rossby was working on at the time, in connection with the mechanisms for maintaining the atmospheric jet, which a number of people in the department were working on then.

Frenzen: He suggested a laboratory approach.

Fultz: Right, and other people were working on other approaches so that the final staff member paper really had all those aspects in it, including the laboratory.

Frenzen: And that was a miscellaneous report?

Fultz: No, it was a **Bulletin of the AMS** paper in 1947. It's the one that's essentially the beginning of the general circulation jet set of questions.

Frenzen: As you went on, how did you learn about important developments in this field--through journals? Any particular journals? I'm sure your trips to Europe--

Fultz: Not particularly. A couple of things I learned, for example von Arx's work, I learned about from Rossby because he was traveling between Stockholm and Woods Hole and Chicago. There was a good deal of back and forth involving Victor Starr's general circulation project at MIT. We had some people exchanged and I made a number of trips there, and Al Faller was starting to work at Woods Hole so that contact was close enough that I kept up pretty well with the general circulation questions that Victor Starr really posed most clearly and that could be approached to some extent experimentally.

Most of the very early dishpan experiments of different types we set up in conjunction with those discussions with Victor Starr and Ed Lorenz and some of his other students.

Frenzen: Over the years, what journals did you keep up with regularly?

Fultz: There weren't many journals in 1947, but I essentially tried to keep up with the AMS publications, **Journal of Meteorology**, and a few things in the **Bulletin**. With the **Journal of Geophysical Research** of the AGU and also with principal foreign journals: **Tellus**, after Rossby started that--

Frenzen: I remember that we put some early papers in **Tellus**.

Fultz: --**Quarterly Journal of the Royal Meteorological Society** and a couple of the German journals turned out to have some things in them.

Frenzen: Here are some administrative questions on the skeleton list:
Have you ever served on any grant review boards or any committee which influenced the way funds are given out in research?

Fultz: Only one major one which was concerned with a Public Health Service research program. I forget the formal title, but it was a grants review committee.

Frenzen: It had to do with atmospheric problems?

Fultz: Right. With air pollution, that sort of research area that the Public Health Service preceded the EPA in sponsoring, in fact more extensively for the time than the EPA has since.

Frenzen: Did this board meet or work by telephone?

Fultz: It was the first time I had run into this sort of an arrangement because meteorological arrangements I knew about were too small to involve this but it was, I thought, an extremely effective review process that involved twice a year meetings of the full committee at which they reviewed all the proposed research grants for that particular time.

Frenzen: Where did they meet? In Washington? Not Cincinnati.

Fultz: No, that was one of the research centers.

Frenzen: It must be listed in your curriculum vitae, exactly what the name of the board was.

END OF TAPE 5, SIDE 1

Interview with Dave Fultz

TAPE 5, SIDE 2

Fultz: The board was the Air Pollution Research Grants Advisory Committee.

Frenzen: Were you given papers in advance, proposals in advance? How did it work?

Fultz: It was a relatively elaborate procedure which went across a number of fields, so the committee had chemists and meteorologists and physiologists and a number of other types of people on it. That's probably too extensive to go in any large field. What essentially they did was send everyone on the committee a copy of all the proposals to be read, assigned two or three to prepare written reviews. Then at the meeting, the reviewers would give their impressions and assessment. And there would be a general discussion of each one all the way around the committee.

Now I've kind of forgotten whether the rating was by secret ballot--I kind of think it was--but the thing that surprised me was how frequently somebody from completely out of the field of the proposal would make the crucial assessment of it. One I particularly remember was a proposal from a group of chemists to do something with limestone purging for stack effluent treatment, and the chemists on review were relatively favorable, relatively positive review. An old-time engineer who'd been around the field for 30 or 40 years shot the thing down completely because he knew exactly that had been proposed in the Twenties in Germany, checked out and it went nowhere. So that's the sort of thing that mail reviews cannot do and that a narrowly focused committee is not likely to do.

Frenzen: How were you chosen for this review board, do you know?

Fultz: I don't really know, except that we did have a research grant from the Public Health Service for several years there and I suspect what they did was rotate grantees to serve on the committees.

Frenzen: Have you ever served on any government panels or advisory bodies?

Fultz: The other two major ones were the Geophysics panel of the SAB of the Air Force. And the special review that the Department of Transportation ran on the climatic impact assessment program, which was the one that evaluated the problems for supersonic transports and ozone depletion. That latter was a rather short but intense exercise.

Frenzen: The first one you named was an Air Force panel?

Fultz: Yes.

Frenzen: What questions did they address?

Fultz: They didn't have any particular agenda that I could identify but they simply tackled any questions that came up to them and surveyed the adequacy of geophysics activities in the Air Force. We went to a number of centers like Hanscom Field and Wright Field in Dayton and various things, including a trip to Alaska, Point Barrow, and the T-3 ice island.

Frenzen: Did these constitute on-site reviews of programs?

Fultz: In certain cases, yes.

Frenzen: You wrote reports?

Fultz: I don't remember that we did formally as a committee, but I suppose probably our reactions were noted by the chairman and the vice-chairman of the panel and transmitted.

Frenzen: Have you ever served as an editor, or were you on the editorial board of a journal?

Fultz: No.

Frenzen: Have you ever held an office in a professional society?

Fultz: Not any major one. I was on the Awards Committee of the AMS for a couple of years and otherwise just local offices here.

Frenzen: Could you tell something about the main things you've done outside of working hours during your career? Do you have any hobbies?

Fultz: It's pretty hard to put your finger on them. I tend to read extensively and cross quite a number of areas.

Frenzen: You haven't taken up farming in Indiana?

Fultz: Our Indiana place, when it was on my in-laws' farm, had a fairly substantial garden and I used to do a fair amount with that, plowing in the spring and things like that.

Frenzen: Have you written any popular articles?

Fultz: I don't think so.

Frenzen: But you said earlier you might take it up now. What sort of thing did you have in mind?

Fultz: Well, maybe to try to lay out for sort of a general meteorological audience without all the details what some of the possible uses for experimental results might be.

Frenzen: Is there anything we've left out? Is there something of interest in your career we may have missed?

Fultz: I'm really not aware of anything.

Frenzen: Once again, we'll thank you and we'll wrap it up. Thanks again, Dave.

END OF INTERVIEW