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TAPE RECORDED INTERVIEW PROJECT

Interview with Herbert Riehl
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Interviewer: Dr. Joanne Simpson

Simpson: I have the pleasure today to talk to my friend and long term colleague Professor Herbert Riehl on some aspects of the important work that he has done over the years. There is an excellent interview done with him published in the WMO Bulletin, October 1986, and the material here will to some extent supplementary. I would like to start by asking you, Herby, how you got involved in tropical meteorology. But I was interested to read in the WMO interview that you were actually interested in tropical meteorology before that.

Riehl: Well, that's very easy. I came to Chicago under the auspices of Carl-Gustav Rossby, from Seattle where I had spent my first year at a university as the Meteorological Instructor of Geography. This was also arranged by Rossby. I had finished an Air Force course at New York University for weather officers, but the Air Force wouldn't take me. I suppose because of my former nationality, but at that time I had been a United States citizen already for several years. I came to Chicago as an instructor in the Laboratory, the head of the Laboratory being Vincent J. Oliver, in the class on Middle Latitude Meteorology, such as it was known then. There were 100 plus Air Force officers there and six Navy officers. Somehow it seemed to me while I was there that these Navy officers quite apparently would be sent to the South Pacific, and what they could do with the middle latitude cyclone model was not very obvious. So I suggested that one should get hold of whatever information might be available on the tropics and Western Pacific in particular from Pan-American Airlines records and others. Indeed, the person in charge of handling people there, Professor Byers, was willing to let me go to Washington, war or no war, and collect such data. This still amazes me nowadays, that in view of everything else that went on, I was permitted to do this. So I did, and came back with a lot of microfilm and things. And then spent some time with the Navy officers going over these things. So I got some idea what it was like to be in the equatorial zone rather than in the polar front zone.

Then in 1943, somewhere along the line, Professor Rossby got a request from the Air Force for training weather officers in tropical meteorology, since they were sending all their people from the United States via the northern Brazil coast, then on a hop to Ascension Islands, and from there to the Gold Coast of Africa, and from there north into what was to become a major battle ground in the

Mediterranean. So the southwest ferry route was their main objective. The suggestion was that the United States has a dominion or colony in Puerto Rico in the Caribbean. There was a university in Puerto Rico, in Rio Piedras not far from San Juan, so Rossby went down and generally arranged for and Institute of the University of Chicago to be set up there. So, when he came back, Professor Byers was then entrusted with the details of getting it to function. Then he looked for a staff. The first director he picked was a man who had been in the Guadalcanal Operation and had to quit because of malaria, Clarence Palmer; and had Gordon Dunn as a hurricane expert; then John Bellamy as an instrumental expert; and David Fultz as a junior theoretician, etc . Since I had already done this other work with the Navy people, I was sort of pushed through a hole into this thing, and they said you have already done this, so you will go. So I went.

Simpson: When you went down there, did Dr. Cressman go with you at the same time, or was it later?

Riehl: No, that was later.

Simpson: I see. When you were doing the work that began to make you the famous pioneer in tropical meteorology, for example, the Chicago Series of Miscellaneous Reports, one in particular, "The Waves in the Easterlies and the Polar Front in the Tropics", did you interact as a group down there and have heated discussions or were these things individual ideas that you had all by yourself?

Riehl: No, there were various types of interactions and also some individual activity. Sort of a mixture of all that. Different people gave the different courses, and so on. After a while, Air Force officers were giving courses. They came and went.

That was the place we found out something which has become of importance later, that the old method of the change in temperature during the rain didn't work. One used to think that when it rained, the latent heat energy increased, the temperature decreased and the total energy remained the same. But they hadn't found out yet that the temperature ant the dew point decreased together and that meant that energy changed and the low energy had to be imported from somewhere else.

Simpson: Yes, I remember the discovery that you made there about cold-core storms and the cloudy part of the storms being colder and, I guess, having lower O_E than the clear parts.

Riehl: That turned out to be the case after months of soundings. It was very difficult to get any tropical data under wartime restrictions. When you got soundings, you could see the mid-tropospheric minimum of energy of the so-called equivalent potential temperature, another one of Rossby's inventions, and, of course, we closely followed in his footsteps.

Simpson: Did you work at all with Clarence Palmer, or were your ideas and his quite different about how the atmosphere and the tropics worked? Because later on people often used to ask me who is the greatest tropical meteorologist, Riehl or Palmer? Palmer had different ideas about how tropical waves worked. I think one could say in retrospect perhaps, his background was in a different part of the world. Did you have heated discussions at that time with him?

Riehl: Well, really there was no conversation whatsoever. Palmer was the man with the wartime experience and had been out in the field actively, so I pretty much accepted what he said.

Simpson: Almost all of his observational experience, except the times spent in Puerto Rico, was out in the Pacific, however. Is that correct?

Riehl: That's correct. He came from New Zealand, in wartime to Pacific; and, of course, he came quickly to the equatorial trough or convergence zone. We were quite some distance from any such things; actually they never existed in Northern South America, or anywhere in South America. There remains the problem. It tries to exist but it can't, but it could very well exist in the South Pacific. Essentially, what he had to offer was the South Pacific history. He was very good at that. Of course, later he opened the first institute in Hawaii and somehow died an early death.

Simpson: I think one reason for that was that he got discouraged by some of the criticisms that some people made of his work; he took it very seriously. And in order to be a successful scientist with new ideas you must develop a tough-mindedness to criticism, which I think you have always done very well indeed.

When the end of the Institute of Tropical Meteorology came, I guess it was due to the war ending and support from the Military being finished. Then you went back to Chicago and organized a graduate course in Tropical Meteorology which became an historical landmark. What motivated you to do that?

Riehl: Well, it was sort of a logical thing to add to the curriculum in Chicago, and I don't know if I proposed it. I rather suspect that Rossby and Byers proposed that. It couldn't have been introduced without their approval, and the Dean's and so on, so I rather think the suggestion came from them.

Simpson: Did you have writing the book in mind at the time you were organizing the course, or did that come out of having done the course?

Riehl: It came out just about the same time as we organized the course material. Actually, the book material came out of its existence.

Simpson: That course, as you know, as was mentioned in the WMO Bulletin, was a landmark in my life which changed it completely around. It also influenced many other people who were among the first group of students to take that course in the Spring

Quarter of 1947. Do you remember who all was there? I know Noel LaSeur, Werner Baum, and Seymour Hess. It was such an exciting course that I think a lot of the people were greatly influenced. Chuck Jordan was in it later. He was not in the first course, he came later on.

Riehl: The thing is, I think the first course sort of invented itself as it went along. It went from one thing to another, we had to get some kind of order. We had to think about things in the evening before presenting them in class. One of these topics to be sure concerned the trade-wind inversion. We had this previous work from Germany from the 1930's and it somehow didn't make any sense at all. Further, the period coincided with the big arguments of other people about the existence of so called mean meridional circulations, Hadley circulations, or whether it even existed for that matter. So that was one of the things that came up. Actually all of these people have talked about it. Our Chinese colleague Dr. Yeh and myself sat down with the Climatic Atlas and a computer (to calculate the meridional circulation) which hadn't been computed before. Much later, of course, this led to my part in the Dish Pan Rotating-Basin experiments, which became one of the most interesting things that I did, but perhaps we should leave that for a little later.

Simpson: I think that the Dish Pan experiments are important and interesting to talk about. Were you looking at the Dish Pan experiments that you did with Dave Fultz to see if a direct Hadley cell circulation formed in low latitudes?

Riehl: In the Dish Pan, of course, you got only essentially one circulation cell between heat and cold sources. When you put on a coordinate system, latitude and longitude, such as were used more momentum transfer computations, and everything else, then the first result was the indirect circulation cell advocated for the atmospheric middle latitude case. However, if you discarded that and went on to the coordinates following the jet stream, then the signal, what you might call the so-called Hadley cell, emerged and this just shows that you can go to any sort of coordinate system whatsoever and transform them as you like and find mean circulations, eddies, and heaven knows what else, according to your choice. But these things are statistical configurations and have nothing to do with the physics of the situation. That was really one of the things that most attracted me.

Simpson: It is interesting that while you do not have the phase changes of water in the Dish Pan, you still get a Hadley circulation. This is interesting in view of the large role played by clouds in the real atmosphere's Hadley circulation, which you and I investigated together.

Riehl: You get the circulations, but of course it is a cold season baroclinic circulation. What is not in there are the energy transformations at essentially constant surface pressure, that is the "summer" circulations and all that, which are still giving difficulties to forecasters at the present time.

The baroclinic forecasts in the "machine-age" have improved very much over what they used to be. I don't think the same can be said about summer forecasts and also mountain effects. Here now all summer long, we have had pressure variations just like you get in the heart of the tropics. Namely, plus/minus half a millibar, and so the forecasts are not geared to this; they keep churning out with no more than zero skill.

Simpson: One of the very important contributions that you made is that you didn't just restrict yourself to doing laboratory experiments or theories, but you found data from the real atmosphere. Sometimes, I guess you had to go out on aircraft flights yourself and at other times you found sources of data; for example, in the trade-winds and the equatorial trough. How did you find the support and get together the data for those important studies of the tropics that you made after that?

Riehl: This kind of thing at the University of Chicago essentially required resources from a third party. The University wasn't paying for any of that, but I proposed projects to the Office of Naval Research which they very willingly funded in whole, or more than that. And then after a while, I also became a consultant to a new Naval facility that was supposed to translate the results of research into practical application for the Fleet forecasters, etc.

Simpson: That's a very important topic that I would like for you to talk about, but we still have the same problem of how to transfer research results and for research people to work together with forecasters. And I think that what you did there was a very important example of results being transferred successfully. Could you tell us something about that?

Riehl: Essentially the research was focused, in general, on two things. One was the trade-winds and trade inversion and the heat and energy balances of the trades, which is something we all worked on together. And then with the cumulonimbus processes, and this work now has startling consequences so many years after it was first done. We will come to that a little later. The [Navy] Norfolk Applied Facility, however, was started because of demands for better forecasts by Admiral Nimitz and similar persons. They had problems with the tropical weather conditions in the Pacific and their fleets were dispersed, so it was already pretty well set. They were more interested, of course, in severe weather; tropical storm's origin and movement. Later they became interested in other things such as the effects of the vertical structure of the atmosphere on their Polaris missiles they sent up from submarines. And still later, they were interested during the Vietnam war in the outflows from China and how strong they would be and when thy would come for the transit of their ships from the Philippines to Vietnam through the Gulf of Tonkin.

Simpson: Tell me something about the project that was called AROWA.

Riehl: That's what I have been talking about.

Simpson: You did something about jet stream flights, too.

Riehl: This supported all of that. That is to say that the first jet stream very much came into prominence through Rossby and others in the late 1940's and earlier part of the 1950's, or so. They managed to stir up the Navy interest in making flights through Rossby's jet streams, somewhat competitive with plans of the Air Force doing the same thing. We ran a number of flights and some of them showed rather strong lateral shears. We also tried vertical shears by flying at different levels. There were early indications, confirmed later, that jet streams in the wavy basic currents are very finite. You get a limited area of maximum speed in anticyclones. They intensify out of the troughs ahead of the anticyclone and on the other side of the anticyclone, they diminish again and go to nothing. So you get, on the forward side, the so called direct solenoid circulation and on the rear, or east side of the anticyclone, you get the reverse. And that also turned up, interestingly enough, in the Dish Pan experiment.

Simpson: This is very interesting. And I think it is very important to point out that that was probably the first quantitative work on jet streaks, as they are called nowadays, with different balances or imbalances or forces on the entrance end from those at the exit end. It is interesting to note that, as you know yourself, Louis Uccellini and a number of younger scientists in the field today who have worked on the rapid, sudden development of extratropical cyclones and winter snow storms, have based their work very heavily on the studies that you originally did on jet streaks. And I don't think anybody even documented that those existed before.

Riehl: Somehow, once you get going in some subjects, everybody keeps shoving projects to you in that area, and the line of least resistance is to do it. So that was what it was with tropical meteorology. I tried several times to get more into the jet stream business, but also then personnel changes at Chicago made it more desirable to take this tropical line, which was aside from their mainstream interest and acquisitions of personnel, so that was one way to have peace in the faculty.

Simpson: That's a very interesting point. I think it's important to note that even though you left that particular area of research at the time and went mainly to tropical work, that the jet stream work was sufficiently important that years later the young people, such as Louis Uccellini, working in the field had to come back and base their new ideas about cyclogenesis on the work that you did. You said something about faculty changes at the University of Chicago that directed your work more to tropical topics. Was this from causes inside the University, or related to where the Navy wanted to put its support, or some interaction of the two?

Riehl: No, it was from inside the University. Rossby, of course, essentially left there after 1950, or so, and went back to Stockholm. Horace Byers became the Chairman and Sverre Petterssen was a "replacement" for Rossby. So that led to a change in my orientation.

Simpson: Was this about the time that you and Yeh and Noel LaSeur and I got involved in analyzing the data along the trade-wind trajectory in the tropical Pacific?

Riehl: No, that was actually earlier.

Simpson: That's what I thought.

Riehl: This trajectory work on the trades which showed such different results from what had been thought earlier, was really one of the most interesting and permanent results that came out of the Chicago research. We found that the trade-wind inversion is not a material surface, but rather under the influence of the imposed vorticity field. Due to the divergence, the air sinks. Thus air goes through the inversion, which has been shown also by other Pacific data from The Scripps Institution of Oceanography afterward. Then it wasn't really until 1974 with the GATE Experiment that the German ships took such observations as to show more of the turbulence than one had previously pictured. And showed how it must go; they actually showed that it did go that way with these turbulences to incorporate the upper dry air, with higher potential temperature, through the inversion into the lower layer with higher moisture and lower potential temperature. And so the lower moist layer increased, the inversion went up against the air coming down. This was one of the nicest things in my memory.

Simpson: Yes, and that, as you said, is work that certainly still stands today. I remember that you and I had a discussion in which you had the idea that the tops of clouds could cut off and evaporate, and that this may have had an effect on raising the moist layer and deepening it as the air went downstream in the tropics, and then I started working on that with you.

Riehl: You have done quite a lot on that and taken all the photographs which show these intrusions. Time-lapse pictures in great quantity.

Simpson: Yes, and then we were able to show that the clouds actually played a very important part in raising and weakening the inversion and deepening the moist layer.

Riehl: Yes, that was a neat thing!

Simpson: How did it come about that we started thinking about cumulonimbus clouds and their role in vertical transports? I tried to remember the other day whether the hurricane part of it came first or the equatorial trough when we were trying to do the heat and energy balance in the equatorial trough.

Riehl: I didn't quite follow that.

Simpson: Peggy LeMone was asking me questions about some of these same things and I couldn't remember at the time which came first with regard to the hot tower hypothesis that we developed. I could not then recall whether we had the idea with regard to hurricanes first, or whether it was the work in the equatorial trough that you found that a Hadley cell as just a gradual circulation wouldn't work.

Riehl: Exactly, that's how it came. You need something like months for one circuit in the Hadley cell, sinking so slowly and everything, and against that in the end for the mass balance, there had to be a very rapid ascent in the equatorial zone in cumulonimbi. That was expanded at the time to also giving a whole climatology of convective cloud distribution. But in the later literature[1] that has become the paper most often referred by a long measure. And, of course, what we did then was to focus on a very small area, for one cloud I think it was 25 square kilometers where all ascent was concentrated, we were really prompted in good measure by the observations of the normal equivalent potential temperature distribution with the height, which suggested there were two heat sources for the atmosphere, one at the surface and one not so far from the tropopause. Then one found that heating from the ozone layer would not really do this, so you had to postulate that surface energy came undiluted, practically so, almost to the top of the troposphere, and that the heat transfer in the lower atmosphere was cumuli up to about 600 mb and then from the cumulonimbus clouds through sinking down. In this way then you work against the net radiation loss in the middle atmosphere leading to the energy profile that has repeated itself in practically all soundings in the global tropics. So that's how that came about Of course, this has gone through also some kind of questioning but has sort of withstood the test of time. And once we did an order of magnitude calculation suggesting that clouds occupy about a thousandth part of the tropical atmosphere. In a thousand soundings you will find one, which is not very scientific or professional. How do balloons act when they get into these highly convective regions? But at that time we found one at a station, Kapingamarangi (island in the Pacific). So essentially this is precisely the way it goes, and, more recently, this has been apparently confirmed by the radar observations made with very large dishes by Balsley and associates here at CIRES (of NOAA actually). They found what appeared to be "hot" towers in the western Pacific. Their work involved a completely different method of observations, new observations no formerly available. And then, of course, came a completely unexpected thing (maybe not to you, since you are at NASA). On the trip of VOYAGER II, it was pointed out to me by Robert Grossman just quite recently. I didn't know about it, but the pictures from the planet Jupiter confirmed for the equatorial zone that these same towers were in existence there in the same manner. This has been published several years ago, so that's no secret. I also suggested to Grossman that it might be perhaps easier to make a count of convective towers on that kind of a body than on the Earth where so many other things are mixed in. And now comes this most recent great surprise. This is Grossman's story, you should get it from him if you

[1] Riehl, H. and J.S. Malkus, 1958: On the heat balance in the equatorial trough zone. *Geophysica*, **6**, 503-535
This work was redone later, with more extensive and more accurate data, with essentially the same results in the following:
Riehl, H. and J. Simpson, 1979: The heat balance of the equatorial trough zone, revisited. *Contributions to Atmospheric Physics*, **52**, 287-304.

don't know about it already, that this phenomenon also exists on Neptune. From limb photography, and things like that, we can get an idea of the stability properties and all that. He keeps saying (Grossman's story, I'm not going to steal his thunder) that it exactly duplicates what the tropical atmosphere shows. No credits to me in any of this whatsoever. Of course, you have been at NASA, maybe you should suggest that they look for this.

Simpson: That never occurred to me to suggest that they look for this. And I think it is very interesting, particularly since many people, including Vern Suomi, were saying that the winds were much, much stronger on other planets than they are on Earth, because on Earth we have the hydrological cycle, and therefore, hot towers were making it possible to do the circulation that way without tremendously fast overturning in the Hadley cell, so it would be interesting if we can get more information about that on other planets. It looks like the hot tower activity is a more general event in rotating fluid systems, than we had any concept at the time when we were working on it.

Riehl: Of course, you are now in a position at NASA whereby you can probably do something about this more and perhaps still other planets. I think Robert Grossman would only be too happy to discuss it.

Simpson: That's a great idea.

Riehl: He's (Grossman) certainly highly active.

Simpson: He sure is. And he works very closely with us at NASA.

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Interview of Herbert Riehl

TAPE 1, SIDE 2

Simpson: I think these are very exciting observations. Also interesting is the fact that although we had to guess at the amount of transport that went on in the ocean, back in 1956 or so when we were doing that work, when you repeated it in 1978 or 1979 with much better knowledge about the ocean circulation, that you still found the contribution of the hot towers to be about the same.

Riehl: Yes, exactly. Except for something like this, you would never have understanding of the mid-tropospheric minimum of energy.

Simpson: In the Dish Pan Experiments you made to simulate the atmosphere, is there any indication of that kind of transport in low latitudes, or does the Hadley type circulation do all the transports?

Riehl: There's a heat source there extending from top to bottom so you just have a very narrow layer, with strong upward motion in this case by heat source, so it is enforced. That was the steady three-wave experiment, and later on Fultz took data for one experiment with index cycle which a later student of mine, Russell Elsberry, was to pursue further. It got so far, but in the end the whole thing sort of stopped before reaching a solution. I really to the present day don't know why.

Simpson: It seems like a fascinating subject.

Riehl: Your work [that is Joanne's] on the locations of the clouds in relation to the vertical wind shear showed essentially the two-dimensional horizontal vorticity, the upshear ascent, the downshear descent. I don't think I ever made real use of that until much later. I worked in Spain during an experiment (WMO) on cumulus clouds there and had observations of the towers and their displacement by means of Russian radar, and we knew the movement and the position of the troughs from the Spanish meteorological charts, which were copies of what I think was put out in Britain. One could see in the course of daytime hours, which are involved here, that the clouds had a dynamic component back toward the trough, and not such a small one at that, which led me to wonder about something that I didn't do earlier (still hasn't been done); for instance, the waves in the easterlies sometimes move faster, sometimes slower than the current, and I think that perhaps also relates precisely to this windshear.

Simpson: I think that that is a very interesting point and it certainly seems to relate to the work being done on the waves and super cloud clusters that the Japanese are talking about in the Pacific. The super cloud clusters propagate and modulate the speed of the waves. However, there are not yet really enough observations to see if any of these theories are correct or not. It is interesting to find in the July 1st *Journal of Atmospheric Sciences*, there's an observational paper in which the writers are using ozone as a conservative tracer. This seems to verify some of that earlier work on the enrainment on the upshear side and outflow on the downshear side of cumulus clouds.

Let's talk about hurricanes a little bit. I think you were advising Reichelderfer when the Weather Services, Weather Bureau in those days, in the mid-fifties, was thinking of doing a hurricane project.

Riehl: The thing is that there were quite a lot of hurricanes in the years '54 and '55. From which William Gray can take some confidence for his annual predictions now, since those were years in which the Walker oscillation in the Pacific was very strong. Strong easterlies and cold ocean temperatures were connected with many tropical cyclones, and 1950 was somewhat similar. We all have looked at tropical cyclones from early on and figured it was not perhaps the most important thing in tropical meteorology, but nevertheless, the focal point of great interest soon after 1955 was the active thought given to having the research pursued more energetically by a Weather Bureau Institute. I think most active in that was Robert Simpson in getting this started. He had a special name for it too at the time. [discussion about what the Project was called in its early days—later known as The National Hurricane Project].

That project was approved then by the Weather Bureau and by Congress and put into operation in 1956. The actual data gathering started in 1957. It reached the high point in 1958, and then upon the withdrawal if the military from that, it went down in 1959, progressing later to civilian aircraft, which have kept on, I think to the present day with some of this. Among the most interesting questions when the aircraft first were flying was, what does the interior of a hurricane look like? Since no rawinsonde had ever penetrated there, because it was quite impossible to launch and to follow up on a balloon under such conditions. One had a few eye soundings in bug eyes; one was in Tampa, Florida, but the region of the most active convection in the main hurricane clouds was unknown. So the flight pattern which was evolved was mainly the cloverleaf, but also squares and circumnavigations were adopted with flights at different levels.

On the B-50's or similar aircraft, the meteorologist occupied the navigator's seat and essentially directed the operation, but the captain of the ship always had control of what he was really going to do. Furthermore, they had a B-47 there, in which a meteorologist never flew due to lack of space. That was most interesting data which came out of the cloud pictures and wind data and everything that was there showing for the first time really clearly how the high altitude outflow materializes, and all of that.

Simpson: Tell about some of the flights that you actually made into hurricanes, and in particular the one where you noticed that apparently there was one single, huge chimney tower in the eyewall that you wrote about afterwards.

Riehl: When it started, the part I was involved in, there were quite a few people flying, of course. In 1957, I was pursuing something that might have become a hurricane, which finally did for a short time, named FRIEDA, in the western Atlantic. It was sufficiently close to the coast that one could reach it in one day and get back again. Access was always a difficult problem, since there was no use going a long distance and having a very short time in the storm and to have to come out again.

FRIEDA, by the end of this flight, persisted to getting to more than tropical storm strength, indicating very clearly that there was threshold value. This was near 1000 millibars or so and beyond this threshold value it didn't go. Then later on, as seen from charts, it was moving farther north, where it bogged down and eliminated a trough in westerlies which was trying to capture it. The trough went down and then the storm became a hurricane briefly before going north in the Atlantic Ocean.

Among the various things I have thought about and written also on hurricanes, the one early on, which was published in the *Journal of Applied Physics* in 1950, still seems to be right to me today. We have read about the Norwegian theory of hurricane formation, which was inverted waves with easterlies increasing with the height in the tropics and then essentially something like a polar front cyclone developing in the opposite direction. That couldn't very well be so. Once a little cold air at the surface gets into circulation you will get an ascent which is colder than the mean tropical atmosphere which shuts off everything immediately. Now consider, however, that the release if potential energy, as this would be, is not in the low troposphere but in the high troposphere. Essentially a concept once again based on what Rossby had, what he called "swimming icebergs" around the 200mb level. Supposing such an iceberg collapsed, by 500mb, it is nothing any more. Then it would be possible to have the release of potential energy, which would stimulate something like a tropical storm to go over some kind of a limit and then develop what we afterward found to be the so-called isothermal expansion of the air. This happens as the air crosses from the outside to the inside of the storm. But it needs sort of a jog to begin with to embark on this phase at all. Now the observations are quite old, but inside the eye you get the same temperature as outside, something like 27 degrees C. But you get it at a much lower pressure so the potential temperature is much higher, but early investigators did not seem to be impressed with that. Then Horace Byers, in a textbook, mentioned that essentially the whole hurricane looked like a huge cumulus cloud. I don't think he mentioned that there must have been a large sensible heat release from the outside of the hurricane to the inside to essentially hold the temperature at a constant, perhaps 2 constant. And, as a matter of fact, when we worked on this on the hurricane of 1958, the DAISY Hurricane, we found that the more you go toward the center, the more the sensible heat transfer actually declines because of the rise of moisture in the air; the vapor pressure gradient between the ocean and atmosphere diminished when the wind speed increases. There have been a lot of arguments about that since then, especially what they call the large sensible heat transport; I don't know if any conclusion has ever been reached on that. It seems to me that, as far as I'm concerned, I've stuck with the large sensible heat transport. I don't consider it very large when you go from the air-sea temperature difference of a few tenths to an air-sea temperature difference of 2-3 degrees, which is an increase by a factor of 10, that there shouldn't have been a factor of 10 increase of the sensible heat transfer and the much larger ratio of sensible to latent heat transfer is observed, than for instance is found in the trades.

Simpson: At the time we developed that idea and applied it to modeling the hurricane boundary layer (based on the DAISY observations that we wrote several papers on), this caused quite a lot of controversy. I remember, in particular, George Carrier and his disciples at Harvard describing the hurricane as a totally hydrodynamic vortex without putting the thermodynamics in. And then we went back and showed that you couldn't possibly get ascents of clouds up to the levels that they got to without ascending a moist adiabat with a much higher θ_w, which could only happen if you had the heat source that you just described. Within the last two or three years, Kerry Emanuel did a much more elegant mathematical model of the very thing that you and I did earlier, perhaps in a cruder way. Now everybody takes for granted that this is so and that the extra heat source exists. In fact, Kerry Emanuel's prediction, is that if we get global warming, we will get a lot more hurricanes that are more intense, which may be taking the whole thing just a bit too far, but we will come back to what you think about global warming later.

The work that you did on hurricanes then, was in collaboration with a lot of people, including myself.

Riehl: That was the most important collaboration, really. The whole model that came out of the three DAISY flights (or four DAISY flights) in 1958. The storm, starting from nothing and intensifying as it moved slowly up the east coast, was first missed when we were all at a boat party on a Sunday looking at crocodiles. When we came back, it was a tropical storm in the Bahamas. So the next morning, we went out in a hurry, and that's when I saw the first measured hurricane winds I ever did. Winds were then measured by the Doppler radar. This was since replaced, and there has been some question about quite how accurate the Doppler radar is. I don't know that it makes too much difference at low levels, but for high altitude flights, it was the recent navigation improvements that are important, plus the Doppler radar tended to give out when looking at the ocean surface from 30 or 40 thousand feet. So the storm (DAISY) was first found just east of Miami, more or less. At a reasonable distance, various cloverleafs were flown in it. And, the next day it reached much more intensity, and finally had a great big eye. The earlier eye, as happens often, was covered, with only the low levels covered with clouds at 500mb. Already then there was the suggestion, I don't think was ever published, that the eye develops from the surface up and not from the top down. It isn't stratospheric air that comes down at first. It is the surface development near the ground and near the end it may include air from the stratosphere. It was proven eventually when the small admixture of tritium from the stratosphere had been observed in the surface data. That is really the start. With a stack of two airplanes, B-47 on top and a B-50 doing two-three levels underneath, gave us the possibility to produce a vertical sounding. This was a very nice sounding with the exception that the aircraft data at 237mb were lying a little bit on the warm side of the sounding; which, at least for my part, I corrected later by using an ice adiabat for the upper levels from -10^{o} upward and then it was clear through. Why the ice adiabat was not used at that time, I am still puzzled about, since it was a small correction and obviously easy to do. Other than that, you have a very satisfactory balance of noncontroversial and also controversial things like the momentum transport into the hurricane. Some wondered, and perhaps still do, about the role of horizontal correlations between the tangential and the radial winds. There are always diagrams that don't get published. Later in 1958, there was a storm named HELENE, which crossed up the forecasters in a bad way at the end. But earlier, I had been flown with William Gray, a passenger who was supposed to get his Master's Thesis from that (I don't remember if he did), but this storm was intensifying while we were there the second time. It was beginning to approach 100 mph on one side, so we did a lot of low-level flying in that. We went through the eye wall on the eastern side, encountered thunderstorms, and finally did a complete circumnavigation which I don't think was ever published. It showed a complete zero correlation between radial and tangential circulations.

Simpson: At the low inflow layer?

Riehl: That was really done at 1000 feet. It showed simply that there was absolutely no eddy correlation whatsoever. Investigations of other storms by a Navy student I had at the time showed similar things just from the data that were available. But there's nothing like, as you say, doing your own investigations.

As you said earlier, I perhaps had to do some aircraft investigations. But I would say that far from having to do it, I was very anxious to do it and I desired very much to do something like that which produced a bit of life in an otherwise perhaps rather dreary research environment, compared to laboratory work where you constantly are involved in producing new things. I have earlier done a bit of flying out in the Pacific Ocean in hurricanes with some other people, so this has been a very satisfactory experience. It was too bad when later on at the end, the Navy, who was sponsoring these flights, saw me as being too old at the age of 45 or after to fly in hurricanes any more. For the last flight I made in 1960, the Chief Naval Officer in Norfolk as much as got himself authorization of the Admiral in charge of Naval health in Washington before he would let me go on a mission, saying that, at your age you're supposed to sit behind a desk and not up in front on airplanes.

Simpson: One of the great strengths of your work, which I think and many other people think made it so significant, you are one of the few meteorologists that has done work on data, work on observations that you made yourself on aircraft flights, laboratory experiments like the "Dish Pan" experiment you were describing, and also thinking about theories. You did some very and important work on the initial deepening and development of hurricanes mainly involving processes at upper levels interacting with processes at lower levels. Now that seems the way peoples' thinking is going, about the origin and development of hurricanes. Do you want to make a comment on that?

Riehl: That is the one I mentioned earlier that was already in this 1950 analysis of hurricane formation that I wrote. But then we got to the first satellite picture of temperatures in a 1960 hurricane called ANNA. It showed very clearly, that as this storm was beginning a cold cyclone nearby went into collapse. Then we were paid a visit by Dr. Yanai from Japan, who came for a year, and he had another storm development of a Caribbean wave when in the southwestern Caribbean there was a strong vortex. A cyclonic vortex one day, the next day it was gone and the storm intensified.

We have had also cases where that seemed to be true and then a tropical cyclone vanished from one day to the next. That was true in 1956 at the beginning of the season, early August, that a not-so-large hurricane was developing just north of the Greater Antilles, and then an upper air cyclone with its roots down came southwestward from Bermuda, was superimposed on the hurricane, which immediately disappeared. Later it reappeared again when it got away from the upper cyclone.

Simpson: So in the late 1950's and in about 1960, there was a big transition in your activities, moving from Chicago to CSU. Before we leave Chicago to start to talk about CSU, you had a lot of people that were graduate students of yours at Chicago that went on to be (many of them) significant contributors to tropical meteorology. I can't remember how many there were, but we can remember some certainly that did outstanding work. I can also remember several where you said I wrote a very nice thesis under the pseudonym of so-and-so. You had a vast combination of students, there were quite a large number of people that came over from India, Japan, and other places, too.

Riehl: I had a Japanese working for us for a degree. Had one from India, Krishnamurti.

Simpson: Also, what about Gangopadaya?

Riehl: Gangopadaya. Yes, that was a Master student.

Simpson: Right, and there was another one who later became the Director of the Indian Weather Service.

Riehl: Koteswaram.

Simpson: That's right.

Riehl: He was a jet stream man more than anything else. So we had these people come in from abroad to Chicago. Otherwise, for the Doctor's thesis, there was Noel LaSeur and I think there was Robert Simpson, and yourself.

Simpson: Chuck Jordan, Krish.

Riehl: Krishnamurti I had mentioned. And William Gray came close to it, but he had to finish his degree there after I left.

Simpson: Somehow, in the late 1950's, you were interested in this area (Colorado) partly because of your lifelong devotion to climbing mountains. How did you get involved with HAO (High Altitude Observatory) and Walter Orr Roberts and start to think about moving to this area?

Riehl: Of course, Walter Orr Roberts I had already met in 1950. We were taking a month vacation from Chicago, drove west in the mountains, among other things. I came to Boulder to start looking at the campus, and there they had still these wooden barracks from wartime. One of the barracks had a sign outside on it "High Altitude Observatory". And I thought, what's that? I walked inside and some chap got up from a drafting table, came over and said. "What can I do for you?" This turned out to be Walter Orr Roberts. We kept in touch after that and Walter Orr Roberts invited me to give the first course in meteorology in Colorado in the summer of 1955—the summer session.

Simpson: At the University of Colorado?

Riehl: Yes, it was then the High Altitude Observatory. It was also mentioned in the WMO Bulletin that there was also interest in the High Altitude Observatory whether one could make forecasts for the Sun. Considering a solar day being 27 Earth days and since cyclones here want to keep up for one day, quite nicely, so the Sun should do that for one solar day. However, it didn't. So that was dropped again. But the course was the first one in Colorado.

Simpson: How many students came and was it introductory?

Riehl: It was quite introductory. It was quite a large group. There must have been 20 to 30 people. And at the time, I also went around and visited Fort Collins, Colorado State University. Looked around there on the campus and went on again. Nothing further.

I didn't have such a great preoccupation with mountains really. I came to Chicago in 1942 to stay 6 months, and in the end stayed almost 20 years. That's enough to stay any place.

So in 1957, we came back for another summer session and then also went to CSU. That coincided with the time that I had been doing the work with Dave Fultz and the rotating basin in Chicago. And in Fort Collins, they were doing wind tunnel work on the flow of the air across mountains, from the front range of Colorado, in particular, over Long's Peak and down. So they were interested in model experiments on that subject.

Simpson: That was Professor Cermak?

Riehl: Cermak, yes. At that time the Engineering Research Center at CSU was very strong and beginning to locate a base in the foothills. So some time after that I had the inquiry from them whether I wouldn't like to come there and essentially open a meteorological activity. They had then already some cloud physics activity by Lewis Grant and Richard Schleusener, but nothing else at all.

Simpson: Those two guys were in the Civil Engineering Department?

Riehl: I had really thought more of Boulder, but then also there was this thing about starting up the National Center for Atmospheric Research. This didn't make it look to good for an institute in meteorology to be located right on site. You would have the chaps from the NCAR (where we are sitting here now), just breathing down your neck all of the time, but nothing came of it in any event since High Altitude Observatory people didn't want any of it. They felt that meteorology then was much more popular than high altitude solar observations. The tail would wag the dog.

Simpson: They felt threatened that there would be more funding and more interest in the meteorology than in their field.

Riehl: So they kept essentially a Meteorology Department out of there. They had a couple of people in atmospheric science—Julius London and Bernhard Haurwitz who came out then and this was continued but always at the quite lower level adjunct to high altitude rather than high altitude becoming adjunct to meteorology.

I felt I had been at Chicago long enough, and that was also the time when the Department of Meteorology in Chicago was coming to an end. The University and the Dean of Sciences were wanting to mix up meteorology and geology in one Department. That didn't sound good from the viewpoint of a future there. It then became the Department of Geophysical Sciences after a while. That was later. At any rate, having the opportunity, and being invited there, I considered taking it on.

Simpson: Were the people in the University administration at CSU, was Ray Chamberlain, involved at that time?

Riehl: Yes, President Morgan was involved. He paid me a special visit in Chicago to discuss this thing. I can say that never in my life have I applied for a position and only always responded to invitations. Feeling somewhat "Chinesey" about that. If you apply for a position and then you don't get it, you lose face. Of course, you know Northcote Parkinson on that particular subject.

END OF TAPE 1, SIDE 2

Interview of Herbert Riehl

TAPE 2, SIDE 1

Simpson: We are talking a little more now about the hot towers on other planets and the work that Bob Grossman's doing on that, and Herbert's mentioning that it may be easier to count the number of hot towers on the other planets than from satellite imagery of the Earth clouds.

Riehl: The thing is going to be the one-in thousand ratio again.

Simpson: What data did you just recently say you were going over?

Riehl: I was looking at the *Atlas of Highly Reflective Clouds* produced by Rolando Garcia here at the CIRES Institute. A very nice Atlas for trying to correlate satellite clouds with the Venezuela precipitation. It gave you, unfortunately, not much better than zero correlation, but often these things are qualitative, and that can't be helped. I couldn't do any better than Rolando Garcia, probably not nearly as well. But it may be so that on Jupiter and Neptune they can be seen and counted much more easily, that they are much more distinct there.

Simpson: Is this because of the high, cold cirrus that so often makes it difficult to identify the raining cloud cores on satellite imagery of the Earth?

Riehl: Partly cirrus and partly things that almost come up to this highly reflective cloud stage and don't quite make it, some do and don't. The quantitative aspect of it is difficult.

Simpson: You were just starting to say how they invited you to go to CSU.

Riehl: Anyway, there was this invitation, and then there came another permanent factor, in the form of my then 5-year old daughter, who insisted in contracting pneumonia in Chicago and wound up in an oxygen tent, so for her the much drier climate was suggested. That was a real factor, also, in accepting there. I mustn't leave the personal factor out of it. It wasn't just a purely university political or scientific decision. Besides, something new, something different, I might make a go of that.

Simpson: Did the Colorado State University administration leave you pretty free to decide what parts of atmospheric science, or how much of it, you were going to cover and at what level of degree—bachelor or masters? Or did they say you could have a number of appointments when you came there?

Riehl: Three faculty appointments and M.S., Ph.D. degrees if all went well, as it did. (see next pages)

Simpson: And laboratory work similar to that of Cermak was doing?

Riehl: Yes, which in the end never came off, because at that time the computer modeling took over and the computer people said they are much better in stimulation than laboratory people. That didn't do Dave Fultz any good, and they moved to their new building in Chicago. I last visited him when he had a huge quantity of equipment on an enormous floor in the basement there and no funding.

Simpson: No research money for that lab. What do you feel about that retrospectively? I kind of feel it might have been a mistake to let that laboratory experiment die out.

Riehl: I definitely agree. It's quite erroneous to put this computer modeling ahead of everything. We go so far and then unfortunately this has now reacted in all work being just fiddling with computer models and the like and not the actual physics so much. I think that it is a very poor thing. Apart from that, the University administration in Fort Collins pretty much let us do what we wanted, as long as we brought in enough money to give them an overhead, and also us an overhead. The decisions on degree programs there was first on faculty, and that was mine.

Simpson: How many faculty persons did you bring in at first?

Riehl: We brought in three: Elmar Reiter from Innsbruck; Ferdinand Baer, a student of Platzman's from Chicago; and William Marlatt from Rutgers, student of Erwin Biel.

Simpson: Marlatt was then a climatologically oriented person?

Riehl: Climate authority. He was supposed to be the interface with what Colorado State's agricultural and forestry oriented faculties in general. So he did that too but then he left, but I understand he's back again these days.

Simpson: So when you first went there, your program was part of the Civil Engineering Department.

Riehl: That's correct, we were an Institute of the Civil Engineering Department. And Richard Schleusener and Lewis Grant were also participating there. Schleusener went in the end to take an administrative position at the University of South Dakota. But Lewis Grant stayed with the program to the present day.

Simpson: And the work that they were doing was primarily in weather modification/precipitation modification.

Riehl: Yes, that was Schleusener. Schleusener more hail storms in summer, and Lewis Grant, snow modification in winter.

Simpson: Did you feel supportive of their work and that that was a worthwhile project, or did you have doubts about it?

Riehl: In this work, as also in the hurricane modification attempt, I did as the saying goes "straddle the fence." I didn't know if it was any good, but it seemed good enough to warrant a try. Since they were funding themselves, I had no problems that way. I had to help with the funding of other people, and also others that came into the Civil Engineering Department, Hydrology Section, like Yeigevich. Also helped him get an Office of Naval Research Contract. But not for the weather modification people at that time, so we let that run its course. But with regard to the degree program, we decided early on we did not want a Bachelor's Degree. There really is no employment for a person with Bachelor's in Meteorology that's worthwhile, and I think there still isn't. Later on we started giving undergraduate courses in Meteorology and or Atmospheric Science, some of them call it Atmospheric Sciences (I don't know where the "s" came from). There was a requirement of one science course for a large number of their Social Science and Humanities students, and we thought we could satisfy that requirement. Actually, we started out the first year with only three graduate students. Two of whom couldn't take Ferd Baer's theoretical course and the eliminated themselves, and only one, James Rasmussen, survived. He's become sort of an international figure in recent years. The Degree Programs for Master's degree first, and later Ph.D. degree, were put together by the faculty and they had to be approved by the graduate school Dean, and the University in general. The first degree went through perhaps in 1963 and the second one a little later, and that a Ph.D. degree, and that was very nice. So by 1965 or 1966, we had the whole Degree Program together.

Simpson: Did you believe that there should be coverage of the complete range of aspects of meteorology within the Department, or did you feel the Department should have specific focuses and strength in certain areas?

Riehl: Well, the latter, but at the choice of the professors, such as: modeling by Ferdinand Baer, general circulation; Elmar Reiter's various exploits; and finally we also took on something on the Denver air pollution. Weather modification gradually became less and less. In the end, we found we had really not enough radiation people present. We tried to get hold of more, but it was difficult. That was actually while I was on sabbatical leave in Europe. Von der Haar and Cox came from the University of Wisconsin, which was very strong and is strong in radiation and satellites, and they are a very useful addition here to the present day.

Simpson: Your Department at CSU was very strong, and is still very strong in radiation and has some excellent people. Who came first? Cox or Van der Haar?

Riehl: I think they came together. Von der Haar had also been offered a position of Administrative Assistant to Walter Orr Roberts. He was vacillating between these for a while. In the end, I think he made what I consider, the right decision. He has continued his university work. He has been very successful in research and teaching, instrumentation, putting up satellite dishes and satellite services that the CSU department offers nowadays.

Simpson: We noticed on our visit to the CSU department much more than in most university departments, they provide the students with opportunities to use observations and to make observations, and I think that has been one of the great strengths of that department.

Riehl: Exactly.

Simpson: Your work on tropical meteorology, did you continue that work at CSU? I mean, you published several books on the subject.

Riehl: I started with a two-dimensional hurricane model in 1963. We still had our collaboration going on with a Woods Hole photographer, Claude Ronne, going all around the Pacific Ocean and taking observations in routine aircraft transports; he got access to a window on both sides of the aircraft. People are wondering to the present day why the two of us here didn't do these flights. Of course, we were not photographic experts, but at least they feel we should have controlled the photographic expert. But I think he did extremely well. The photographs showed where and under what circumstances the cumulonimbus clouds occurred, and under what circumstances with respect to the prevailing general circulation. Under certain conditions, clouds eroded and did absolutely nothing. This material was then published in a book at the University of California at Los Angeles by you (Joanne). This book is out of print and my copy has disappeared and I can't get another one.

Simpson: Someone has stolen mine and I can't get another one.

Riehl: However, when the first experiment with the ATS satellite began in about 1967, in the area of Christmas Island (and such places), they conveniently forgot about this 1963 book and all the papers before that which were published in the Journals and everything else. They just simplified the 1958 paper on the cumulonimbus to say that all CU clouds that grow up become big cumulonimbi; of course, they didn't, so they decided this wasn't any good at all. The made corresponding noises and only found out later, whoever did find out, that we had never intended such a thing, on the contrary, we put out a lot of literature against all of these towers producing big tropical rains.

Simpson: They should have gone to your Tropical class. Because one of the first things you pointed out was that despite all the conditional instability that was available in the tropical troposphere, most clouds terminated after a few kilometers.

Riehl: Yes. Then with the large wind shears aloft, they just eroded. We saw whole big forests of dead clouds tops, upper halves of clouds, and so on, about an upper

(200mb level) cyclone situated between Guam and Wake. So that was very clear, that was very misconstrued later. But, as far as I know, not to the present day.

Simpson: I can't quite remember, but I think it was the people that made the experiment in the Line Islands that were making those statements.

Riehl: Yes, precisely that.

Simpson: But I think it is discouraging to find out that people have to go out and relearn the same things that we found out about way back in the 1950's. What time did Bill Gray come to CSU and start out his tropical student group there?

Riehl: He came about 1962, later than the others. I don't know how many students he had to begin with, but he got going, to a large extent, after I went on sabbatical leave in Europe. In 1966, the first student came from Australia. He first had expected to find me but he (Gray) was present. That was the beginning of that.

Simpson: Yes, he had quite a large number of very good Australian graduate students. As a result of their training at CSU, Australian work in tropical meteorology has been really outstanding. When you went to sabbatical in Europe in 1966, which countries did you go to?

Riehl: I went to England, the Imperial College, London.

Simpson**:** Who was there at that time?

Riehl: Professor Sheppard was the Department head.

Simpson**:** Was Frank Ludlam still there and Richard Scorer?

Riehl**:** Yes. Scorer and Ludlam were there and the man who became the head of the Meteorological Service, Mason. So they had a very strong group that Brunt had put together in wartime. It was mostly, but not entirely, there anymore. Eady wasn't there anymore. Still that was one of the strongest groups in meteorology ever put together anywhere.

Simpson**:** Yes, it was. I went there, Lou Kaplan went there, Dave Atlas went there, you went there. We were attracted to go there because it was one of the strongest departments or strongest groups anywhere in the world, and now it's completely faded out and really doesn't exist anymore.

Riehl**:** There is very little of it left in Imperial College. The center shifted to the University of Reading.

Simpson**:** Bob Pearce and you did some work together at that time?

Riehl: Yes, also, a paper published on tropical interactions (I don't know if it was ever fully published), interactions between synoptic and mesoscale weather elements in the tropics.

Simpson: At what time did you begin to collaborate with Alan Betts and do the work in Venezuela on VIMHEX?

Riehl: After I came back from Europe from the sabbatical, there was a planning of an International Tropical Conference in Caracas that same autumn. I went down to Caracas where I made the acquaintance of the people there. The meteorology was and still is in the hands of the Air Force. I met various people and saw there was a need of doing an experiment there on land. I went from Caracas, to Maracay where the Air Force Headquarters was located. This went on the next year, then the plans were firmed up to have special experiment during the rainy season of 1969 on land and partly overlapped with the BOMEX sea experiment. Essentially to find out about tropical cumulus clouds and whatever you could find out about the. Frequent rawinsonde observations had not existed before, during, and after radar echoes came over. Once an hour, that was the minimum time one could do then, and I understand that is about the minimum one can do now.

Simpson: That was, as I recall, one of the first, if not the first, serious combined radar and sounding experiments to look at the cloud and its environment over tropical land masses compared to tropical oceans.

Riehl: All of these soundings going on for several months, I had never seen before. Serial soundings of various sorts were already employed much earlier by Jac Bjerknes and Erik Palmén, in northern Europe.

Simpson: But not focused on cloud systems.

Riehl: Not at all, it had other purposes in higher latitudes. No, I thought we had pretty much the first detailed experiments on that basis with the hourly soundings. One station hourly soundings providing cross-sections and all, and the radar to go with it. It was a primitive radar by today's standards, but it provided good, what you might call PPI and RHI resolutions that were just fine.

Simpson: What did you find out from that experiment with regard to the similarities or differences between the precipitation process and clouds in relations to their environment over the tropical land masses versus the ocean? People are still debating about that today.

Riehl: We hadn't really found out too much about the ocean, so the land investigations sort of stood on its own. Later really amplified somewhat by the German ship observations over the ocean.

Simpson: That was the ATEX experiment?

Riehl: That was really not in the convective area either, but in the trades.

Simpson: You had a lot of different and interesting projects that have involved South America. You and Byers did something about water resources down there. Then you and Alan Betts did the VIMHEX. Then you've done more things and are planning to do more things down there.

Riehl: I might just add the atmosphere ahead of the radar echo, ahead of any potential storm was more saturated than average and more likely to precipitate and to produce a thunderstorm, but of course the thicker the moist layer the weaker the thunderstorm. Bigger storms come when the upper atmosphere is quite dry and the whole lower troposphere convects up. That's on the American continent, not Venezuela, not at all. With a very deep, moist layer, you are guaranteed to have a precipitation. Then you got every indication of the strong mixing downdrafts, they were very noticeable. For the first time perhaps, we got an indication that some of the downdrafts which overshot their dry adiabat sort of stuck out in the low levels as a very warm layer for about 50mb or so.

Simpson: That was sort of and earlier discovery of things of people then looked at in GATE.

Riehl: That's correct. There was a lot of that found in GATE. So that was one of the things there. The radar observations showed the general relation between the daily heating and the extent of the radar echoes, the maximum radar echoes in height and area, they were observed at 6:00 o'clock in the evening.

Simpson: So there is quite a different diurnal cycle there than you would find over most parts of the oceans.

Riehl: It was definitely the land heating which brought on the convection. Nocturnal maxima didn't really exist. There were a few days with evening clouds and some radar echoes, but not much of it. But synoptic systems produced rain of any time of night or day.

Simpson: We used to have a lot of discussions and controversy/arguments about the diurnal cycle of tropical cloudiness over the oceans.

Riehl: The trade wind inversion, of course, like in the northeast Pacific, is higher in the middle of the night than in the daytime. We really thought that the convection was more nocturnal, and still do in most areas, but I guess not universally. Especially, somewhere near land area, it sometimes does something different.

Simpson: I agree with you completely. In the GATE area, for example, I think the maximum was around noon time or a little after, but that was so close to the African continent. And on a lot of islands like Borneo in the "Maritime Continent", the presence of land and ocean together make the diurnal cycle quite weird sometimes.

It seems like over the really open ocean most of the time, it is probably a nighttime maximum.

Riehl: Yes, exactly. That was the first Venezuela experiment in 1969.

Simpson: Did you do more than one Venezuela experiment?

Riehl: The next experiment was in 1972 with Alan Betts.

Simpson**:** Oh, I see, he was present only on the second one.

Riehl: No, he was present on both, but he was essentially the Chief Senior Scientist on the second one.

Simpson: Did you start your collaboration with him at Imperial College when he was there as a graduate student, or when he came to CSU later?

Riehl: I corresponded with Pearce on the subject and I was told that you can have one or two graduate students, one was Miller, he's that sort of nice, quiet type; then the very bright, belligerent type, Alan Betts (Pearce's remark). In the end, I took Betts who sounded more challenging, then afterwards, Miller also came.

Simpson: Betts stayed at CSU for a number of years?

Riehl: Yes, Betts stayed there for some years, but the bureaucracy became too much for him, the administration of students and everything, the degree programs, student's advisory committees, etc. So he decided to get out of there and become an individual investigator. He certainly could have stayed on and become a professor at CSU, but such was not to his liking.

Simpson: Did you continue to get support from the University administration? It seemed to me that Ray Chamberlain, whom I met (I met Morgan also), that they were, compared to other universities at least, very actively supportive of the atmospheric science program.

Riehl: Yes, they were. And they could be so in the early years when there also was a Democratic governor and legislature in Denver, so that funding at the state level went through, and also we had state funding. Later, as it were, the state legislature has turned Republican, and to the present day, very much so. There was one Republican governor, we now have Democratic governors, but Republican state legislatures which has changed the financial picture very considerably to the detriment of the University. The state funding, which was available at first and free tuition for the graduate students and everything, that all came to an end. Half of the overhead money for the department and all those things which made life very agreeable and easy, did not exist later, and apparently there were quite a lot more upheavals.

Simpson: The situation when you first went there, the state legislature was supportive of the University. Did this mean that the state paid the salaries of the professors and they didn't have to go out and seek their salaries from contracts?

Riehl: They never paid the full professor's salary, only for the teaching duties. It was something like four or five months, in the academic year of three quarters. The rest we had to go out and provide from research projects and grants, which at that time, on the federal level, was similarly fairly easy to come by.

Simpson: So never, even from the beginning, the professor's salaries were not entirely paid by the department? The professors had to pay part of their own salary from their own grants. In normally good times, not super good or super bad, other factors being equal, do you think it's a good idea?

Riehl: Yes, I think it is a good idea. I have seen other institutions which paid faculty members nine months of the year. We had that in Chicago, it may not have worked out badly there. But you can point to many departments and institutions not having anything to do with atmospheric science when it comes to that, where the nine-months' tenure faculty just continued living its life, in no danger of being forced out, or anything of the sort. It just became antiquated and it had to be put up with until retirement time. There was a lot of stirring about that. I think rightfully so. That the professor has to arrange for part of his funding and that of his associated students, etc., is, in my opinion, not a bad idea at all. At least wasn't. Maybe the funding environment has become so bad now. I am told by professors now, they spend too much of their time writing proposals, there is almost no time left to do anything else. That, of course, is obviously unhealthy. In the former days, you spent very little time writing proposals, one or two pages was enough, and the proposal was never rejected, if you had cleared the way for it beforehand.

Simpson: Looking at it retroactively over the years that we have been involved in meteorology, seems like back in the old days when we were supported mainly by Office of Naval Research, that maybe we had to put down one page, or maybe a telephone call. Over the years, it has become more and more rigid, more and more proposals, and longer proposals and more chance of not having them accepted.

Riehl: The so-called peer review and everything which didn't exist either at first, or very little of it. I was quite happy in that climate. Of course, I never thought then that the climate would change. But it did.

Simpson: What year did you decide that you didn't want to be department chairman anymore? That you served your share of that kind of work? Had you stayed there as department chair for about eight years?

Riehl: Eight years, yes. That was again taking the cue from Rossby and from the American presidencies, I said eight years is enough. Then, also, I had observed then certain other department heads in meteorology who had stayed much longer who had been quite innovative to begin with and then stayed in a holding pattern, with the effect that their whole show went down again; because they hung on! Not naming any names.

Simpson: You mean the department as a whole declined because the chair stayed there too long. Yes, I can think of examples.

Riehl: Yes, there are examples, we leave them nameless. At CSU I had put the degree program in place. I had advised President Morgan; at the time I assumed this responsibility, I was going to stay in it only until I had exhausted my ideas on what should be done there. Now what was done there was greatly facilitated by being a new activity. If you move in on top of something that already has pre-existence, and so many pairs of eyes are staring you in the face and are interested in nothing but the retention of their own perks, privileges and what not, you have a much more difficult time to get anyplace. You have to somehow step aside form what's there and start up something different. You have to deal with these people always wanting to continue as they have done. There are very many examples of that sort.

Simpson: Yes, I think that is a very good point. I was just reading in the last few days the 20-year anniversary write-ups that the CSU faculty members, including yourself, made very fine contributions to, and I have just returned from visiting there yesterday, and the department seems to be as productive, as lively, as innovative; it seems to have actually less difficulty in obtaining good graduate students than most other atmospheric science departments that I have visited in the past year. Now this violates one of Rossby's principles, or ideas, or theories. I remember him saying that really the great age of any department or any institution usually is about ten years. What do you think is different about CSU that it has been able to keep this tremendously productive atmosphere for longer than most other places?

Riehl: The spirit in which it was started has remained alive there. Not to a small extent, by just about all of the other older faculty getting out of the way. Giving it to the younger people. I go back to see Northcote Parkinson, this is a case he didn't consider.

Simpson: Northcote Parkinson really doesn't seem to have taken over.

Riehl: But it has at so many other institutions.

Simpson: Yes, but why, really?

Riehl: The older faculty left. I left. Reiter essentially made himself independent, Ferdinand Baer left, Marlatt left, Schleusener left. Von der Haar, Cox and Gray are still there. Lew Grant has never been in the administration, really, essentially sort of separate in his cloud physics work. So there has been the opportunity and there has always been the effort to bring in new people, from the bottom up, and let them grow.

Simpson: Then there must have been a good choice as to who the new people were. None of them turned out to be duds.

Riehl: Yes, that was pretty deliberate. One older man was brought in for chemistry. Corrin, and he died after awhile. He was not very productive. They should have brought in a young chemist, not an old chemist. I had problems with him. For instance, you observe here in the Fort Collins area sometimes days with very poor visibility. Days when the Denver air pollution could not have reached Fort Collins. I asked Mike Corrin, please, what is the composition of this? Can we make observations of that, and so on? He never made the slightest move, and to the present day I don't think anybody knows where this very poor visibility often comes from. Not wet haze, not Denver air pollution, not sand blown up from the desert, what is it? Another question mark.

Simpson: You think that one of the keys to success that department heads there have used is to pick young people, bright young people, and that the older people get out of the way after a period.

Riehl: As assistant professor, in, and as professor after awhile, out. Exactly.

END OF TAPE 2, SIDE 1

Interview of Herbert Riehl

TAPE 2, SIDE 2

Simpson: OK, we were talking about the department at CSU and the recipe for how to keep a department from going stale. Also, it must have been that the university administration remained benign toward the department all these years. Although the funding situation has certainly become much worse everywhere.

Riehl: Yes, they have had variable luck with administration, I think. For the first few years, we all had an advantage because there was no dean. The Dean of Physical Sciences had been sent to Southeast Asia with the SEATO Project and well-known Professor Maurice Albertson was really the chief political civil engineer and scientist. At Colorado State, he instituted projects in Southeast Asia. The Dean went there and I think he also instituted participation of Colorado State University in Beirut. [I think that's correct, but I don't want that to be part of the interview.] I went directly to the Dean of Faculties, a very nice man with whom we also played bridge.

Simpson: What [*sic*] was Ray Chamberlain at that time?

Riehl: Ray Chamberlain was a very young PhD, the first PhD Colorado State ever had. He was taking care of the paperwork and some of the functions as Dean of Engineering, but none of the Department heads there, Civil, Electrical, Agricultural and Mechanical Engineering, took him at true value as anything like a dean. Not at all.

Simpson: Eventually he became the Vice President and the President of the University.

Riehl: That was much later, yes.

Simpson: Was he one of the original or early supporters of the work in atmospheric sciences?

Riehl: Yes, he always has been.

Simpson: Another interesting point about the CSU Department, which also applied to Chicago people, would say how can you possibly do tropical meteorology in Chicago or Fort Collins? The fact remains that a world center of tropical meteorology was at Chicago for awhile and certainly is in Fort Collins now.

Riehl: In those days really it was much better to go take the observations in the tropics for awhile and then return to the place which had the data facilities, computers and what not to do the research. This has sort of changed somewhat over the years, which is good. You now can go to the facilities within the tropics, which are wellequipped, and you can do your work there. But on a whole, I think it is still preferable to do your main effort out of the center of tropical climates. Even in Brazil they do that. They have a lot of investigations in hydrology of rivers, and the weather in the Amazon Basin and the equatorial zone. But the Institute studying this is in São Paulo.

Simpson: That is a good thing. I think you're still working with some people at Institutes down in South America and are planning to go down there again before long.

Riehl: If it is possible to plan in that direction. Of course, we had another Venezuela project here that is really organized by the Geography head, Roger Barry, for the last two years on the longer-term aspects of Venezuelan precipitation. We thought that was easy. Of course, the situation is quite different from the real monsoon countries, where the flow from the cold ocean on the hot continent makes everything very simple, even though they cry about it. In the Caribbean in the summertime, there is a strong upper cyclone which, however, can't be there always. So there would be no hurricanes or waves. But the upper climatic cyclone is produced by the whole Caribbean region being enclosed in summer by warm air over the northern and southern continents. Now South America doesn't change much, summer to winter. The region of 10 to 30 degrees is the region covered by ocean. To the north, there is the North American continent with large changes of temperature from winter to summer. Squeezed between these two deep, warm regimes to the north and south, the intermediate ocean, reacts by putting in the cold cyclonic circulation. We thought this was a great starting point for looking at the Venezuela rainfall. Everything, as expected, fell into place, except the precipitation, which would not cooperate in the least. Finally then it turned out that the Southern Hemisphere all the time is trying to send the monsoon signal into the Northern Hemisphere, gets totally confused about it because it can't get anyplace with it. This is far more of an influence on the Venezuela precipitation. In that whole northern part of South America itself, or the Caribbean, in spite of much probing has little to do with El Niños. We finished this investigation as far as one can. We need another project to do more.

Simpson: Another observational project in Venezuela or looking at the larger-scale circulation?

Riehl: The latter. You have to look at a much wider scale to get more of the controls.

Simpson: You just finished saying that you think that the ocean temperature anomalies in large scale events similar to El Niño have no repercussions on the precipitation in Venezuela?

Riehl: It might be hard to find them, you know. When it comes to ocean temperatures, they have had this big project here at CIRES, getting all the ocean temperatures from the middle 1850's to the present time. Something I once had tried myself when Woodrow Jacobs was in charge of the Oceanography office and also the storage facilities in Washington, but at that time they said they couldn't do anything like getting all historical data from the Atlantic Ocean but not for any other ocean. But then the computers came in so heavily and made everything so easy that 10-15 years after that they could proceed with the greatest of ease collecting the ships' data for the whole world.

Simpson: This leads me to a question that I have been waiting to fit in to ask you. One of the things of concern to many of us in atmospheric science meteorology in the U.S. is that apparently correct observations that at the European Centre and various other places, that the prediction of the weather on the medium scale, and nowadays even on the mesoscale, it seems that in this country we are getting quite a ways behind the European nations. What do you think might be the reason for that?

Riehl: Of course, the last time I spent a year in Europe was 1985-86. Then comparisons were made between the output from the European centers versus Washington, I couldn't find much difference. I had no suggestion that Washington was falling behind in any respects. I am surprised to hear that. I knew the European centers had drawn about even and also the British Centre and Swedish center, as well as the Russian one, they had improved. But I didn't think that anybody was really superior then to the then Washington center. It would have to be in the last two years.

Simpson: It has. In fact, there was an article in the New York Times, I think it was in 1988 sometime, about a verification of the medium range three-to-seven-day forecast, and that the ECMWF model was consistently doing better than the NMC models, and the NMC models were particularly falling down on the prediction of severe events, rapidly deepening cyclones and such phenomena as that. I also notice, when I go to other places in the world, like Australia and Japan and other countries (Japan has its own models now), but in Australia, that they used the ECMWF products, definitely in preference to NMC.

You were talking about a very important point of transferring research results to operations, and I wonder if we aren't having a problem in this country of making this transfer.

Riehl: Looking back, of course, since I took first the course in 1940, there have been great improvements in the predictions here. In the baroclinic half of the year, the winter systems, also including storm systems. The problem remains mainly with the convective systems of summertime and in mountain effects. Things may have changed, but I was not under the impression that Washington's Weather Service was behind the others in their verifications. Of course, verification itself is a very dubious objective. I've run into some controversies here and there, and again at the present time; of course, that would lead too far right now.

But since we're talking about CSU, as to the product from there, from the early days at least. One of my students was Jim Rasmussen, who was in Geneva for a long time, apparently has returned there now to World Weather Watch, as a Commissioner for the American Meteorological Society. For quite awhile, he had come back from Geneva to Washington to work his way up rapidly. In the 1960's, of course, he was a student, I admitted him. Eventually he made PhD. I also admitted to CSU early on Jerry Mahlman, who I think has now become the successor to Smagorinsky, in Princeton's Numerical Prediction Center.

Simpson: GFDL, Geophysical Fluid Dynamics Laboratory (ERL/NOAA). It's a very distinguished laboratory and Jerry Mahlman is head of it.

Riehl: He is doing well, obviously. And the other ones, students of Marlatt's, is Vincent Salomonson. He has come a long way with NASA.

Simpson: Vince Salomonson was a student from CSU! He's my boss now. That's very interesting. He is the Director of Earth Sciences at GSFC/NASA. That's fascinating.

Riehl: I most certainly admitted him and he did work for Marlatt on testing instruments to go on a NIMBUS satellite, ground testing of it, and everything was done here, right out here in the fields of Colorado. Marlatt and Salomonson, yes.

Simpson: Did Salomonson get his PhD at CSU in your department?

Riehl: I think so, yes.

Simpson: Well, you have a number of people to be proud of.

Riehl: I had nothing much to do with that one, but I think that's correct. He certainly got his start here.

Simpson: Many distinguished Australian tropical meteorologists: Geoff Love, Greg Holland, and John McBride, all got their PhD's at Fort Collins.

Riehl: This is part of the story that has kept the place alive and going so well.

Simpson: It certainly shows that it has been not only a lively department in producing research, but of sending people into operations and influencing operations, which I think is terribly important.

Riehl: Something I like very much is that so many people just have not stayed in academic work but gone on into better positions in the applications. I think perhaps that the CSU model should be recommended to other institutions. Otherwise, you have so much complaint now that students are no longer in any contact with observations; they only know a computer model and nothing else. Perhaps the recommendation to make is to adopt the CSU method.

Simpson: Yes, I think this is a very important point. Many modelers do nothing all their lives except to live with their models and they begin to believe that they are real, that their model is the real world. At CSU there has been a very good balance between observations and study of data and models, so that the modelers all have their feet on the ground and their models are tested by reality. And I think that's an excellent model for other departments to emulate. Some of them are ivory towers that are totally removed from the real world.

Riehl: Including also NCAR. You say now there are some shortcomings of the Washington models with respect to the European Centre. Perhaps that might be the root of it.

Simpson: That's exactly what I was driving at when I asked that question.

Riehl: There has also been quite a bit of German input into the European Centre. Several people I have known as Bachelor's students in Berlin.

Simpson: Who were the German people that have been important at ECMWF?

Riehl: Mostly a chap named Horst Bjettger. That's all I remember at the moment.

Simpson: Because I think that this is one of the things that the European Centre has done to get ahead. It has had leading young people, like Miller and Betts, and other bright, young people, we were talking about including the gentleman you just mentioned. They're right there working closely with the operational people.

Riehl: I was in Hamburg, 1981 and 1982, at the University, but also attended the weather discussions at the Seewarte. They showed the forecasts made by ECMWF. The countries composing this Research Institute became tired of just research after the first few years, and forced them, already then, to make forecasts for the Weather Services. They were forced to do that, they didn't want to. They wanted the Research Institute, but the assortment of nations, all insisted either they forecast or get out of business. So, they have been making forecasts now at least since 1981.

Simpson: That's extremely interesting. Because, again, going back to Rossby, you remember he said that every department should have a mission, and his department at Chicago had the mission of forecasting, and there were map discussions every day. Some people think that certainly one problem that may be existing in regard to NCAR that it doesn't really have a mission like that. Do you think there's anything in that?

Riehl: Yes.

Simpson: It should have a closer relationship to a mission such as forecasting.

Riehl: The mission of NCAR originally was to supply large facilities which universities couldn't afford themselves. They had strayed far from that, but I still would say that is the best mission for them.

Simpson: I remember you and I agreeing on that point back in the 1950's when meeting with Tom Malone and few other people in Hartford, CT, trying to decide what the mission of NCAR should be. You and I were talking about how it should be mainly providing universities with facilities.

Riehl: That was the official objective then. That was fine. Still is. They have the flight facility and such things, radar and so on, the others really were not of the basic needs of NCAR.

Simpson: And I think that's a good point. I think getting back to ECMWF and our weather service, in retrospect it seems to—at least some people think that separating the research and operations in NOAA was not such a good idea because there was not as close a relationship. In fact actually I think CSU is—

Riehl: Is better.

Simpson: Is better.

Riehl: Yes. Well, NOAA still has the separation.

Simpson: And I think that's one of the things that's caused it to lag behind in forecasting.

Riehl: So that requirement that the Institute is writing, actually may forecast, every one of these countries, every one, has helped. So when something went wrong you immediately from your forecast—

Simpson: Yeah, that's another point to go back and look at your forecasts and understand what went wrong with them. And one hears altogether too little of that.

Riehl: Yes, but you're supposed to do that immediately, you see, is something different, that's what they did.

Simpson: Before we finish, I want to ask you one more question. And although you've won all kinds of awards and well-deserved honors for your work like Meisinger Award, I remember when you got that one, and the Rossby Award, which is the highest award that the AMS can give, I think you got that in 1979 if my—is that right? And then you got the Losey Award also, that's not an AMS award, it's a different one.

Riehl: No, it was what was then that later became the Space and Aeronautics Society or so but was then—I have that here someplace.

Simpson: Sure. That award had to do with applications to aeronautics or to forecasting or theoretical principles?

Riehl: Well, I don't remember exactly what it said there. But see I thought I had this—

Simpson: Well, we don't need to dwell on it too long. I wanted to make the emphasis that again that you've gotten recognized not only for American Institute for Aeronautics and Astronautics in 1962, it's in your WMO Bulletin interview. If you think of the periods of work in your life and look at them, is there any particular period that you felt the happiest and most productive with?

Riehl: Well, that's easy certainly. More or less the last half of the 1950s when we went on with the understanding of the tropical convection on the one hand and the understanding of the general circulation by means of laboratory experiments on the other hand.

Simpson: I think there are many of us that would feel that way. I look back on that period as the most exciting time because we had new observations available and we had new ideas to apply to the observations and tests.

Riehl: That's fairly clear. Of course Fort Collins had excitement in a different way. As an organizer you see more than anything else. Starting up a new institute. Developing degree programs, developing a library and so on, so forth, which is quite another facet of the—

Simpson: It's also a tremendous contribution on your own research, which is read and studied by graduate students, and is still studied extensively by graduate students, and then the fact that the students of the students of the students that you originally had are producing exciting work. Another thing that I notice that's amazing is that I have a copy of your first tropical meteorology book, which came out in 1954. I find myself referring to it fairly often. I find other people coming into my room and stealing it fairly often. I find Bob referring to it very often. And it's usually a book's lifetime is about ten years if it's a good book. And it's quite amazing that that book has basically -- there's nothing in it that's wrong, there's more things that we know in more detail since then.

Riehl: That's interesting about it that nothing came out wrong.

Simpson: And therefore you could still base a course in tropical meteorology on it, which in fact I have done, and just add other pieces and publications that are later to fill out some of the things. So I want to thank you very much. I think this has been a very exciting interview with very many things said that will be useful for people to think about, and certainly has given me a lot of things to think about it.

Riehl: Well, it's a relief too because I was afraid that it would end up too much like the *Bulletin* interview, and it really didn't.

Simpson: Is there anything more before we finish that you would like to make comments about? Where the field is going today or advice to others or anything that you would like to say?

Riehl: Well, nothing I think that we haven't already covered.

Simpson: Very good. Well, thanks very much indeed.

END OF INTERVIEW