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Conversations with Bernhard Haurwitz

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CONTENTS

F O R E W O R D A

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In Summer 1982 Mr. Haurwitz responded agreeably to my suggestion that some tape-recorded interviews of him be made. The opportunity to do so came in Spring 1983 during my temporary stay in Boulder as a visitor at the National Center for Atmospheric Research. Six sessions were recorded, each on one 90-minute tape cassette, at Mr. Haurwitz's home in Fort Collins, Colorado.

The purpose of the interviews was to give a technical perspective on Mr. Haurwitz's scientific work, as viewed by him in retrospect. I was able to plan for the individual sessions from an outline prepared by Mr. Haurwitz, in which he arranged his published work in subject-matter categories. His outline is attached as Appendix B to the transcript; Appendix A is a chronological publication list. The topics were taken up in the order shown in the table of contents, above.

Although I came to the sessions with memory refreshed by consulting some of the papers to be discussed, Mr. Haurwitz did not have this advantage, as he had agreed to my suggestion that there be no preparation on his part, in order to give the conversations as much spontaneity as possible. I trust the reader will also appreciate the fact that unrehearsed dialogue is much more halting, fractured, and generally "nonlinear" than the speaker realizes until he reads a verbatim transcript of the recording. The temptation to edit is thus very great, but we have not done so except where slight changes in syntax reveal a meaning that would otherwise be obscure. In a free and easygoing exchange there are also numerous exclamations, asides, non sequiturs, and other irrelevancies, often uttered while the other person is speaking (and on these tapes, I regret to say, usually by me). The transcribers have wisely omitted most of these. Further, I have preferred to delete some of my more tangential comments, where no loss of continuity resulted, in order to keep the focus on Mr. Haurwitz. Locations of such deletions are indicated by "..." on a separate line between paragraphs.

With these exceptions the aim of the editorial process was to bring the transcript into agreement with the spoken words, as nearly as that is feasible. The work of transcription was done at the National Center for Atmospheric Research with admirable skill and ingenuity by Barbara McDonald assisted by Beverly Chavez, Ursula Rosner, and Jan Stewart. It was coordinated by Betty Wilson, Administrator for the Advanced Study Program.

George W. Platzman
Chicago, August 1984

F O R E W O R D B

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As explained in Mr. Platzman's foreword, the interviews were conducted without preparation on my part. Since most of the papers discussed were published more than 35 years ago, it is not surprising that my spontaneous recollections of them were often incomplete or simply wrong.

In order to rectify these lapses in my memory, I prepared some "postscripts" after auditing the tapes. Each of these is inserted in the appropriate place in the transcript and is enclosed in brackets.

I have no objection to scholarly use of either the tapes or the transcripts, but I wish not be be quoted directly. It is my intention to deposit the original tapes in Norlin Library at the University of Colorado in Boulder.

Bernhard Haurwitz
Fort Collins, August 1984

TAPE 1
27 April 1983

Platzman: This is the first of a series of interviews of Bernhard Haurwitz. It is being conducted on Wednesday, April 27, 1983 in Mr. Haurwitz's home in Fort Collins, Colorado by George Platzman, a friend and colleague of Mr. Haurwitz. Here is a brief summary of Mr. Haurwitz's professional career. He received the Ph.D. in 1927 at the University of Leipzig, where he remained as a staff member until 1932. From 1932 to 35 he was a research associate at Harvard's Blue Hill Observatory and from 1935 to 41 a Fellow and Lecturer at the University of Toronto. From 1941 to 47 he was on the faculty of the meteorology department at MIT and from 1947 to 59 he was chairman of the meteorology department at NYU. After this long tenure as a member of the eastern establishment, he migrated to Colorado, where from 1959 to 64 he was on the faculty of the astrogeophysics department at the University of Colorado in Boulder and from 1964 to 73 a staff member at the National Center for Atmospheric Research. Since 1973, Mr. Haurwitz has been on the faculty of the meteorology department at Colorado State University in Fort Collins. During most of this western period, he simultaneously maintained an affiliation with the University of Alaska.

Platzman: Okay. Now I think we can proceed. Did you have some comments about the mistakes I made in that summary?

Haurwitz: Yes.

P: Please proceed.

H: I should like to make two corrections. When I first came to this country in 1932, for the first seven months I was in fact largely at MIT as, uh -- I don't know what my title was, I guess Research Associate --, at the same time as at Blue Hill, and then until 1935 at Blue Hill. In 1935 I went to Canada and was there at the University in the physics department until 1939, and then from 1939 to 1941 in the Canadian Meteorological Service. Correction: it was not 1939, but 1937 that the switch took place.

P: Yes, that's right. The physics department continues to be the home for meteorology at Toronto.

H: It doesn't "continue" to be ... the Meteorological Service needed more meteorologists at that time and so they gave a meteorological course under the auspices of the physics department at the University

of Toronto, and that is how the whole thing started. I don't really know what exactly happened after I left in 1941 to go back to the United States, but I think fairly soon after that this instruction was terminated for a while and there was no meteorologist affiliated in any way with the Toronto physics department.

P: Okay. I understand. Incidentally, when did your affiliation with Alaska begin?

H: It began, actually, in 1964. I went up there in September 1964 for the first time. We drove up there and stayed until the middle of January.

P: That was just about the same time that you came out to Colorado?

H: No, that was about five years after I came out to Colorado. I don't know, I could also add that I really wanted to go to Alaska for quite some time before then. As a matter of fact, the director, C.T. Elvey, of the Geophysical Institute wanted to have me there, but he always made the proposal of my coming out there only two or three months in advance, and at that time, I couldn't really go on such a short notice. So the first time when the new director proposed to me to come, when I met with him, that was Keith Mather, he visited at the High Altitude Observatory in Boulder at that time, and I told him that surely I would like to come but I would have to have advance notice at least about half a year or longer. So he said, well, why don't you come in 14 months? And 14 months was September of 1964.

P: I see, that's how it all started. Bernhard, I think we should proceed now with the technical discussion, but it occurred to me that I should probably preface it with an explanation of how we are organizing the discussion, and for the benefit of the listener or reader of this interview, I'll say that you prepared a very useful outline of the subject matter of your published work and arranged it in subject matter topics, and we're going to follow that outline as a general guide to these interviews. For example, today's interview will deal, we hope, with four topics. The first one, vertical distribution of pressure and temperature in the atmosphere. Secondly, weather analysis and forecasting. Third, symmetry points in the pressure time series; and fourthly, atmospheric radiation. I should also mention that the work that we're going to discuss is primarily based upon your journal publications and, except if we happen to get into it in a tangential way, it isn't intended deliberately to go into the numerous technical reports and non-journal type publications. So, that I hope, will help to explain what we're doing here, and how we're going about it. So, beginning then with the general topic of vertical distribution of pressure and temperature, this ...

H: Changes -- pressure and temperature changes.

P: Changes. Yes, that is a very important distinction, as I think the discussion will reveal. It was very interesting to me to read your doctor's thesis, which I have here, and to realize that this is a whole domain of meteorology, which was important for a great many years, and in the long run undoubtedly helped to sharpen our understanding of the way the upper atmosphere and the lower atmosphere work together. In spite of what were apparently some misconceptions about this in the early days. But let me just ask some questions about the earlier work on the subject. Your thesis starts from Schedler's work.

H: Yes, that's right.

P: I was wondering, when was that work done, Schedler's work, that is?

H: I don't really know off-hand now, it probably was about 4 or 5 years before mine. I would have to look it up in the records.

P: But you won't be helped, Bernhard, because you don't give a date for Schedler's paper. You give the journal reference, but not the date.

H: I would say probably about 4 to 5 years before.

P: Early 20's?

H: Early 20's.

P: Maybe late teens?

H: No, I don't think late teens, probably early 20's.

[Postscript 1-1 by B.H.: Schedler's publications relevant to my Ph.D. thesis appeared in 1917 and 1921.]

P: I notice that you mention it at the beginning that Weickmann pointed you toward this general area. How did Weickmann himself get involved, get interested in the ... ?

H: Well, I can only guess really, as far as that is concerned. The problem was that this time about which we are talking now, the early 20's, was a time when the Norwegian polar-front theory was first developed, of course, and at that time the Norwegian polar-front theory and the polar-front model of Jack Bjerknes and Solberg, and Bergeron,

to name only three, referred only to the lowest part of the atmosphere. The reason for that, of course, at least in retrospect, is entirely clear. There were very few regular, that is, daily upper-air observations. There were no radiosondes yet, so the forecasters had to depend on largely surface data and whatever they could glean from cloud observations and perhaps a few mountain observations. So they stressed very much what was going on in the lower atmosphere, as being apparently most important for weather forecasting and therefore, by implication for the weather itself. Now, on the other hand, what at least at that time, was largely called the Viennese school, or the Austrian school of meteorology put much more weight and emphasis on the upper atmosphere. Upper means in this case, simply largely the upper troposphere. And so there was at that time some argument going between the Norwegians, the Norwegian school and the Austrian school. That is all a little bit oversimplifying it because in Frankfurt, the people in Frankfurt for instance, also were much emphasizing the upper atmosphere and its effect on the lower one. Anyway, there was this argument. Weickmann, I don't know how he came to be, but anyway he was at that time an adherent of the Norwegian polar-front theory. I say at that time — he was an adherent of the polar-front theory. He had been in Norway, in Bergen, with the Bjerkneses and studied there I think for a whole year. So, he thought it would be a good idea to repeat Schedler's investigation with more data, which were available at that time in order to see what really the conditions or what the relation was of the pressure and temperature variations in the upper and in the lower atmosphere -- which seemed to be more influential. He suggested that to me as a topic, which seemed to me a very good thing for a doctor thesis, because it was really quite straightforward, and I wanted to get through as fast as possible.

[Postscript 1-2 by B.H.: In connection with the Frankfurt group of meteorologists and their skepticism concerning the polar-front theory, Stüve, known among other things through his thermodynamic chart, and Mügge, who with Möller designed a radiation chart, must be mentioned.]

P: It was straightforward in the sense -- in what sense do you mean, in the sense of dealing with the observational material?

H: Dealing with the observational material, and it seemed fairly clear to me at that time and that was true, that I would not in any place get stuck, as I might very well if I had taken a mathematical investigation. I don't know if it's of any interest here, but I mentioned before on other occasions a number of times when I first came to Weickmann and talked to him, I said I would like to do a study of waves in a compressible fluid. And he said, well, this was of course of interest to him in connection with his ideas about the symmetry points in waves in the atmosphere, and he said well that would be fine

with him, but he said he also must warn me that I couldn't expect any help from him in this case. If he thought he would get anywhere with that, he would do it himself.

P: Do you mean that he was not mathematically inclined?

H: That's what he implied, which surprised me really because as a matter of fact he got his doctor's degree in mathematics, as far as I know, with a thesis on differential geometry.

P: Well, coming back to Schedler's work itself, I don't recall now whether it's in your thesis or in some later work that you eventually pointed out the fundamental flaw in the kind of reasoning that Schedler was using. Was that in the thesis or was that at a later time?

H: That was actually in the thesis. That was the first part of the thesis. I have only to say in defense of Schedler that he really as far as I remember now, at least, nowhere in his work suggested that the upper atmosphere, the upper layers, must be predominant because of this static effect of upper pressure changes at the surface. That was implied rather by other people. Schedler, I think, just presented the observations -- the data.

P: But did he analyze them in the light of this hydrostatic formula in which the atmosphere is divided up into layers?

H: Well, he did that too. I mean, he didn't do any analysis really, he just made a statistical study. I have much more of a discussion in my doctor's thesis in the first 15-20 pages about what is actually happening when you add some mass or take away some mass in the upper layer, what happens in the lower layers. Of course, looking over it today as I have done, it seems completely trivial to me.

P: From the perspective of history, it's impossible to judge whether a problem was substantial or trivial so long ago, I think. But Schedler: what observational data did he actually have at his disposal at that early time?

H: Well, he probably had about one-half the data I had, but you know, or you probably don't know, that before there were any radiosondes, the various meteorological services in this country, I mean in Europe, and ... also in this country and Canada had what they called international ... days or something, and they had international agreements where for two or three or four or five days they would send up sounding balloons into the upper atmosphere. And those were the data which had to be taken.

P: Yes, I know. I believe you mention at the beginning here the International Aerological Commission, or is that ...

H: Yes, there was an International Aerological Commission.

P: ... that was formed earlier in the century to try to systematize the collection of upper-air data.

H: Yes, as a matter of fact, just when my thesis was written, and also passed by the faculty -- but that is in this connection incidental -- just after that the International Aerological Commission had its meeting in Leipzig, and I gave a paper on my results at that time, which was of course quite appropriate, but more interesting historically was, at the same time there was a Russian, Moltchanov, who was the inventor of the radiosonde, who presented his radiosonde there, in August 1927, or September, I can't remember now.

P: Dines also was concerned with some of the questions involving the correlations between upper and lower atmosphere. Is there any connection between the work that Dines did and the work that Schedler did and the work that Haurwitz did, or did they follow quite different lines of ...?

H: Well, I don't remember now, but I suspect that Schedler also must have computed the same correlation coefficients which Dines computed from his data, and I did the same. When I say the same correlation coefficients, I mean correlation coefficients between the same elements. And as far as I remember my own, for instance, agreed quite well with what Dines had found from much fewer data.

[Postscript 1-3 by B.H.: Schedler, in his 1921 paper, did indeed compute correlations between some of the same meteorological parameters as Dines had presented in his 1919 paper.]

P: Do you think the methods that were used by Schedler and by you and Dines ... those methods were continued in later studies that you did and perhaps others as well, down into the 30's -- we're going to be getting into that in a little bit -- well in fact, that's part of really the same subject and maybe I should go on to that. Do you have any further comments about the technical aspects of your thesis at this point, or shall I go on?

H: Well, I think I would do quite a few things a bit different today.

P: Oh? What do you mean?

H: Well, one thing is I would arrange my data into groups differently. As a matter of fact, I have later done that as an afterthought, among other things, by separating not only when the pressure did this and the temperature did that at the ground, rising one, falling the other, both rising, or both falling, or things like that, but I would rather have grouped them according to regions of falling and rising pressure in front of and behind the low, so that one could put it more into a time perspective.

P: Yes, but you did this, in your later ...

H: I did that later. Schedler in fact had done that, too, in one of his later works, apparently also as an afterthought.

P: Could you state in a nutshell what you think the principal correlation that was found in this work -- what was it?

H: I wouldn't know that I ... I don't know that I found anything in particular about a correlation. There were first of all things which are fairly obvious from considerations of the hydrostatic equation, namely that if the pressure falls and the temperature also falls at the surface, then in general you will find that there is at higher levels a reversal -- if there is a pressure fall down at lower levels there will be a pressure rise at higher levels, and things like that. But, I think the main thing really was not a correlation but rather that there seems to be a zone of greatest activity — the greatest pressure and temperature change -- in the lower troposphere, maybe in the lowest two or three kilometers and again near the tropopause, and in between the layer is fairly neutral -- the layer in between is fairly neutral.

[Postscript 1-4 by B.H.: The statement that if the pressure and the temperature fall at the surface there will be a pressure rise at higher levels indicates that my recollection of specific results of my Doctor's thesis has become rather dim after 55 years. In reality, the pressure fall decreases only slightly with altitude until levels near the tropopause are reached. Contrary to what I said glibly during the interview that behavior is what should be expected from hydrostatics. The reversal of the pressure fall into a rise at higher levels is rather found with a temperature rise at the surface, as would also be expected from hydrostatics.]

P: Yes, that's a very interesting point, and I noticed that in ... which paper was it? I wanted to comment, let me just look at my notes here for a minute. Oh, yes. This is jumping ahead just a little bit but in later work -- which paper is that, now ... I have my list of your publications here ... number 45, which is "Pressure and tempera-

ture variations in the free atmosphere over Boston," which you published in 1939 -- you make this point, that the lower and upper troposphere seem to be the sites of the greatest changes taking place. That's very suggestive of the barotropic-like behavior of the middle troposphere that was the touchstone, so to speak, for the development of numerical weather prediction and for Rossby's ideas about non-divergent atmosphere. I'm just wondering, to what extent Rossby's concepts about this were developed or influenced by some of the observational material that came out of these studies. Have you any feeling for what connection, if any, Rossby had with this subject?

H: Well, of course, Rossby himself had worked on, you might say, disentangling the pressure and temperature changes at various levels and in fact, he devised a method by which that could be done.

P: But, that was his work in fact at about the same time that your thesis was published ... that he developed a little bit. I don't recall, I may be mistaken, but my impression is that he, in that work at least, never came to a discussion of the way the atmosphere actually works.

H: No, I don't think he did. He gave one, at least I remember that he gave I think one or at least a few examples for making such calculations, but I don't remember anything now that he discussed at any great detail.

P: So the point I'm making is that when he later came to look at the atmosphere in terms of its synoptic behavior, you might say, and that was in the 30's ... the speculation I'm trying to make Bernhard, is to what extent he drew inspiration from the results of these studies that we're talking about here. Of course, one can only speculate about that.

H: Well, I'm pretty sure that he must have been aware of how the atmosphere worked at the different levels because he almost regularly attended the map discussions at MIT. So, he was aware what was going on in the atmosphere, I mean, at MIT probably too.

P: Well, now, wait a second, did you overlap him at MIT?

H: Well, of course, Rossby was the one who invited me to come to MIT. In 1932, I was there officially at MIT. If you want to put it that crudely, I was paid by MIT until May 1933. Then Rossby didn't have any more money but Brooks supported me for the next two years until I went to Canada.

P: Yes, of course, I remember that.

H: And while I was officially at Blue Hill Observatory -- you see, I lived in town in Boston and Blue Hill is ... well, it took me about 25 minutes to drive out to Blue Hill -- I went there once or twice a week. Otherwise either worked at home or went to MIT, especially for seminars I went there, so in a way, in effect I was really there.

P: Let me please go on to 1939. In 1939 you published a paper called "Pressure and temperature variations in the free atmosphere and their effect on the life history of cyclones," in the AMS Bulletin. Now that was, to a large extent, a continuation of the type of analysis that you had carried out before, but in particular applied to data from radiosonde ascents in Boston in 1937 and 1938. There was one very interesting aspect of that paper that cropped up at the end, and this goes back to the question that you brought out at the beginning of our discussion here, namely this conflict of view as to whether the lower or the upper atmosphere was the site of the primary pressure changes. And you suggested ... you gave your own opinion about that at the end of this paper, which was namely to favor the polar-front point of view, on the grounds that there was a reasonable physical mechanism for this instability of the polar front, but no one had advanced any theory about the instability of the tropopause, for example. I found that a very interesting comment.

H: I don't even remember that now, but I think ... I believe you.

P: Okay, I found that a very interesting comment, in fact I wrote down here a quotation of what you said that "unstable waves at the tropopause do not appear to be possible." That was in 1939, and it wasn't of course until almost 10 years after, that the theory of baroclinic instability was promulgated, but it occurred to me when I read that, that had there been some inkling at that time, I'm talking now about the 30's, late 30's, about the baroclinic instability process, you might conceivably have come to a different conclusion.

H: Oh, yes.

P: You see the point I'm making?

H: Oh, yea.

P: But, it's understandable that by analogy with the polar front the only reasonable alternative hypothesis was the one that you pointed out, namely that the tropopause stability would have to come into question. Of course in those days, nothing ... it wasn't ... the connection between the tropopause and the jet stream, for example, was essentially unknown although, isn't it true that in Physikalische Hydrodynamik there is a famous cross-section that shows a hint of the

jet stream. Still, I think that full appreciation of the intense baroclinicity of the jet stream region was really unknown at that time.

H: No, there certainly wasn't anything known about that.

[Postscript 1-5 by B.H.: "Physikalische Hydrodynamik" contains a qualitative discussion of waves at the tropopause and their interaction with polar-front waves, based mainly on Schedler's results. (That book appeared 6 years after my thesis.)]

...

P: Well, perhaps it's time to go on to the second topic here. We have plenty of time to start another topic, which is weather analysis and forecasting. The first paper that was available to me in the NCAR library is number 39, and I found that really quite fascinating because this was a paper on maps of the pressure distribution in the middle troposphere applied to the polar anticyclones.

H: Oh, yea. That's a paper by Noble and myself.

P: There you set about using the available upper air data consisting mainly of pilot balloon observations but also supplemented with I think about 15 or 20 airplane observation stations and the time period was February 1937. Based upon these data, you did a layered analysis so to speak of the what I guess we would call thickness maps now, and by this means constructed some pressure distributions for 5,000, 10,000 and 14,000 feet elevations. Now, were these the first such upper air maps, and incidentally, this was for North America, that should be emphasized. Were these the first such maps constructed at those levels over the entire continent?

H: I don't know if they were the first ones of the continent, they certainly were the first ones constructed, or which at least I tried to construct, in Canada. I don't think there had been anything like that before. In fact, what I mostly remember about all that now is that we tried to get the people in the daily forecasting service, the Canadian meteorological forecasting service, we tried to get them interested in it, but as far as the main office, Toronto, was concerned, well that was pretty hopeless, though some of the people told me, who had been at other forecasting centers -- I forget now which one, farther west anyway -- told me that they looked at the map and the information from these maps, but I had hoped that people would at that time start that on a routine basis because I figured I didn't want to go into the daily routine. But I didn't succeed, at least not at that time.

P: Now, from the standpoint of synoptic analysis, how were the airplane observations transmitted?

H: Well, by teletype, I imagine.

P: They were included with the regular synoptic ...

H: Oh, yes. I mean, there was a connection between all the various weather stations. There was in particular one of the members, in fact, he was the one who had told me that they used to look at these maps, D. C. Archibald, you probably haven't heard of him, he didn't publish anything, but he later went into administration and the Controller -- at that time I think it was called the Director of the meteorological service -- John Patterson was quite a friend of his, so I remember also John Patterson, the Director, coming into the office every evening and going down to the teletype and communicating with the various people. So, one time, when my son was born, he sent that over the teletype — "Haurwitz is the proud father of a son" was the message. But that has nothing to do with the paper, really.

P: An earlier paper that I was able to look at just this afternoon, that was number 19, "The recent theory of Giao concerning the formation of precipitation in relation to the polar-front theory." I was kind of vaguely aware of some of Giao's work.

H: Yes, he had done quite a few papers at one time.

P: Yes, this particular paper, I judge from your summary of it, made use apparently of the planetary convergence of the geostrophic wind and the possible effect of that on producing ascending motion and precipitation. And your paper consists of a very reasoned argument as to why the orders of magnitude simply don't work for that kind of a mechanism. Was there any further, I mean, do you have any further comment about that, or was that just a very brief episode?

H: Well that was a very brief episode. As a matter of fact, as far as I can reconstruct it now, the paper came out while I was still in Leipzig, in Germany, and I read this paper at one time and I gave a seminar on it, quite a critical seminar, at the Geophysical Institute, and then later, when I came over here, when I went to one of the meetings of the American Geophysical Union, I thought I would talk about that mainly because in order to go to these meetings, it's better to have some paper to present. That's really all there is to it, I think. I also remember that it was either this or some other papers of Giao about which I corresponded very briefly with Jack Bjerknes, whom I knew of course from my visits to Norway quite well, and from later too, and I commented on it quite critically, and he wrote me

back and said its too bad about Giao because he was a very promising young man. He used a German word which would in English would mean "he got derailed."

P: Was he a Portuguese scientist?

H: Yes, I don't know if he is still alive, but he was Portuguese.

P: During the time, I think it was the time you were at MIT, paper number 56, "Advection of air and the forecasting of pressure changes" in the Journal of Meteorology in 1945. This was quite a detailed and serious study of the importance of advective pressure changes, and your conclusions from that were largely negative. Is that a fair statement?

H: Yes, as I remember, yes.

P: Do you recall what the motivation for that project was?

H: No, I really don't quite know, but it may simply have been that Henry Houghton talked to me about it at that time, and we both agreed — but I may be entirely wrong -- Henry Houghton talked to me and he suggested it might be a good topic, and so I got some collaborators, I think it's Bernhard Haurwitz and collaborators, and did work on it.

P: Bernhard, I think I had better stop this now.

H: Alright.

[End of side 1, tape 1, beginning of side 2]

P: Now, we're on the topic of synoptic ... I mean weather analysis and forecasting, and I was coming to paper number 62, which is your very interesting critique of Kibel's method of forecasting. I remember you commented about that in your talk at NCAR, but I don't know whether I read that paper previously, and I was quite intrigued. First of all, the outcome of your analysis of Kibel's method was negative.

H: Yes, I'm afraid so.

P: What induced you to look into that at all?

H: Again, it's of course quite a while back, but to the best of my recollection I was at that time working on matters of first of all long-range forecasting, which started out with the study of symmetry points as a tool for long-range forecasting and then we were (we mean-

ing Dick Craig and I at that time), we were looking around at other things which we might do, and one of the things, one of the articles which I read was this paper by Kibel, so we decided we would try that too, and we found that for us, it just didn't work properly. I have also to say for Kibel that he had developed the method further than it was shown in that paper. There were just very brief indications at the end of the paper which we had on how to do the refinement of the forecast, and that was just totally unclear. We couldn't derive the formula, and we didn't really quite understand how to apply them, so we never went into that.

P: Did you ever learn whether these refinements as practiced in the Soviet Union were successful?

H: No, I never really followed up later. Kibel, of course, criticized it quite heavily and I got a translation of the critique from the scientific attache at the Russian Embassy. I think he himself, the attache, was a geophysicist or maybe even a meteorologist. I saw him occasionally at meetings.

P: The method itself had a dynamical foundation of sorts.

H: Oh, yes, yes.

P: In practice, it apparently consisted of constructing these theta charts, theta being a linear combination of pressure and temperature.

H: Yes, I remember it only very vaguely now. I also remember incidentally that Lettau -- Heinz Lettau -- pointed out to me that it was very similar to something else which much earlier F.M. Exner in Austria had ...

P: Oh? That I wasn't aware of.

H: Yes, In fact, I think Lettau even wrote some note for the Bulletin at one time.

P: It seems to me that it would be interesting, although I suppose that this is just a quirk of history now, but nevertheless, I think that it would be interesting to know whether Kibel's method had anything in common with the barotropic model.

H: I couldn't say.

P: It isn't apparent, certainly. Looking at what he did, but it's often the case, I think, that when you probe more deeply in some of these very strange historical developments, you find threads that are

traceable to more familiar things. But, getting back to the motivation for the work on advective pressure changes, and also on this Kibel method, do you suppose that since these publications came right after the war, that it would have been the impetus for the wartime need to improve forecasting and perhaps arrangements or contracts that you had at that time?

H: I certainly didn't have ... well, the contract which I had was the one with which I did this Kibel work, too, but I don't think the advective pressure changes paper had anything to do with the contract.

P: Paper number 80 in this same subject of weather analysis and forecasting, is not a technical paper, but is just some remarks that you made at a symposium on coordinating meteorological research and weather forecasting. I was actually quite intrigued by the suggestion that you made at the very end of that. You may have forgotten what that was, but you said that you felt the best thing that could happen at that point would be for research meteorologists to go to forecast centers for tours of duty, so to speak, and vice versa, for forecasters to go to research centers. Was that kind of exchange or collaboration, you might say, ever carried out at NYU or at MIT, to your knowledge?

H: No, not that I know. But I remember we had at one time one of the meteorologists, the forecasters, I think it was for American Airlines, working at NYU ... that was simply because he was a good friend of most of us, Emil Köhler, if you have heard of him. I don't think he has ever published anything — he is a practical meteorologist, but I think he mainly came to work with one of the people at NYU because he wanted to go away and get sort of a leave of absence or a sabbatical, if you wish, from his routine duties.

P: I see.

H: I don't know about M.I.T.

P: You perhaps remember that this was in fact done or tried at the University of Chicago, and I may be a little bit wrong on the dates, but I think beginning in the early 50's, the Chicago forecast office of the Weather Bureau — it was then called the Weather Bureau -- was moved to the campus of the University and there was a very carefully worked out program of collaboration between ... first it involved collaboration between Herbert Riehl and the Weather Bureau people. Later, when Riehl left, that collaboration was taken over by Petterssen. But that, I think, I don't remember exactly how long that lasted, about 10 years or so, proved to be very fruitful.

H: Yea, well I wouldn't be surprised.

P: And, it's interesting that it was precisely I think the kind of thing that you suggested -- when was that, back in 1952. I suppose that you might say that advice is just as good today as it was 20 years ago.

H: Oh, I'm sure it is, but ...

P: Bernhard, the next topic on the list here is symmetry points, and the first paper in that group is number 5, which deals with these long-term periodicities in surface pressure data at mountain and valley stations. This was published in the Beiträge zur Physik der freien Atmosphäre in 1929. I must say that I have to confess I had no familiarity with that particular work before. Do you recall at this late stage what induced you to move in that direction?

H: Well, it was one of the things of course, which was done at Leipzig. After all, Weickmann -- it was his cup of tea or his bag or whatever -- and as a matter of fact, most, no not most but I would say for a while about half of the Ph.D. theses dealt with various aspects of these symmetry points and wave oscillations -- the time oscillations of the pressure. As a matter of fact, I don't really know exactly what prompted me to do this work. I have a suspicion that one of the reasons was that Weickmann asked me to contribute to this Hergesell Festschrift, that is, Hergesell Jubilee Volume. Now in 1929, I had just had my doctor's degree for two years, I was a young man, and Hergesell, whom I probably knew at that time, but only very slightly, he probably didn't know me, he was one of the big men in German meteorology, so I had to do something. It just occurred to me, I'm sure, that I had to have something which I could work on and publish pretty fast and this of course was again one of these fairly straightforward things.

P: But now, what was the point of mountain and valley stations? Were you looking at the vertical ... ?

H: Well, I wanted to see how these waves, especially at that time, the 24-day periodicity and the 36-day periodicity, how they behaved at higher levels, and curiously enough, it came out that they had quite different behavior. I hope I get it right now for the record, I think the 24-hourly one decreased quite rapidly with elevation or perhaps not quite rapidly but probably at the same ratio as the mean pressure itself, while the 36-day period was decreasing in amplitude much less fast, much more slowly. Which presumably had something to do with the idea that the 24-day wave was something which is largely in the lower atmosphere produced by outbreaks or influx of cold and warm air, and the 36-day wave was something like a monsoon effect.

[Postscript 1-6 by B.H.: The statement concerning the amplitude decrease of the 24-day and 36-day waves is considerably oversimplified, due to my poor recollection, compared to the conclusions of paper 5. Also, in light of Lettau's thesis interpreting the 36-day wave as an atmospheric oscillation, its characterization as a monsoon is questionable.]

P: That leads me to ask you about the summary of this subject that you published shortly after that in the Monthly Weather Review in 1933.

H: Well, actually as far as the motivation was concerned, if I may interrupt you here, it is simply that Weickmann told me, "Now when you go to the States, make propaganda for the Institute. Let them know what we are doing."

P: Oh, I see.

H: So, that seemed the logical thing to do.

P: This work of the Leipzig school, on these long-term periodicities, and I gather that Weickmann was primarily responsible for initiating them, has always intrigued me as to what the deeper roots of that might have been, and I have wondered, is there any connection between this and Margules' work. I'm referring know to atmospheric oscillations, because in that work there are possibilities of course for oscillations of very long period, and I'm wondering whether there is any, what shall I say, trail that could be found if one looks at the literature, that would connect Weickmann in the early 20's with Margules in the late 90's.

H: No, I don't think so. However, since you bring that up, one of Weickmann's students, Heinz Lettau, whom I just mentioned a little while ago, he, I should say, tried to explain or if you want to, he explained the 36-daily wave as one of the -- what would we call it now? -- I guess Rossby waves, or the westward-migrating waves in the terms of Margules ...

P: But do you mean that came much later?

H: That came in the 30's, well maybe that was the late 20's. I could easily look it up because I have the paper here, but no, it must have been about 1929 or 30 that he wrote that.

[Postscript 1-7 by B.H.: Lettau's paper on the 36-day wave appeared in the Leipzig series in 1931.]

P: Is that the only attempt to apply Margules in this area?

H: Yes, that was the only attempt. As a matter of fact, interestingly enough, I didn't know enough about that at that time myself, I hadn't read anything by Hough at that time and Lettau obviously didn't and so Lettau did his work just the way Margules did it, rather than applying Hough's method, which of course would have converged a bit better.

P: Margules used trigonometric series, didn't he?

H: Yes.

P: Jumping ahead and into a different topic, is there any connection between your work as a member of the Leipzig school on these periodicities on the one hand and your later work, I think it's the 1936 paper, on atmospheric oscillations? The 1936 paper dealt with a two-layer atmosphere extension, you might say, of Margules and Hough type. Is there any connection between those two things?

H: Well, I think there is. As I said before, when I came to Weickmann, of course, I told him I wanted to work on waves in a compressible atmosphere and he frightened me away, fortunately. Well actually he didn't frighten me away, he said "Fine, go ahead." But since my father was not very much in favor of this whole foolishness of studying, I thought I want to get the doctor degree as quickly as possible and show him that in contrast to other relatives of mine I really managed to get a doctor's degree, so I took something else, but then after I was through with it, and so to speak had a position where I could more or less do what I wanted to, I went back to the study of wave motion.

P: I see, okay. But then, from the way you described that, it doesn't appear there was any connection between the symmetry point type of approach to periodicities and the atmospheric oscillation approach.

H: No, that's true.

[Postscript 1-8 by B.H.: In my 1937 paper (number 36) on atmospheric oscillations specific reference is made to Weickmann's waves.]

P: What eventually did become of that line of research into these long-period waves in the pressure field?

H: Well, it just petered out, I think.

P: Do you think on the basis of your experience with data analysis and tidal theory, where of course you've dealt with the very subtle

statistical matters that are involved in these time series, do you have as much confidence today, as you might have had let's say in the early 30's, with the statistical significance of the results of these symmetry point arguments?

H: Oh, I'm really quite skeptical about it, frankly, today. I mean, there was of course never any attempt to do any statistical analysis of these things. Of course, today one would do all these investigations much different. For one thing, of course, one would put it all on a computer and just grind it out, but one would certainly also seek ... to find the statistical significance.

P: Okay. Paper number 30 I'm not able to ask you about because I wasn't able to locate that and I didn't take the time to read it here.

H: Well, actually I haven't been able ... oh, paper 30, that must have been just a summary of a talk which I gave. I haven't got a copy of that any more, anyway, and I suspect that it probably would just be a rehash of this Monthly Weather Review paper number 16.

P: Alright. That leads then to the final topic of the afternoon, Bernhard, atmospheric radiation, and papers 20 and 21 I did not have access to and didn't take the time to look at them this afternoon. Twenty, that concerns turbidity in North American air masses.

H: Well, the turbidity factor, "Trübungs" factor, was something that Linke introduced in Germany, and when I came to this country in 1932, then worked first part-time and then completely at Blue Hill, Brooks -- Charles F. Brooks -- suggested to me that they had all these radiation data, radiation observations, which somebody ought to work up and I quite agreed with him, that this was something that ought to be done. They had just started at that time and after the series of records from 1933 was available, I started working on that. That was radiation, first of all, the total solar and sky radiation on a horizontal surface and also radiation from the sun, which would give you the Linke turbidity factor, and I wrote this paper which then started Harvard Meteorological Studies number one, and as part of that I also computed, or rather together with Harry Wexler, I also computed turbidity factors and I wrote it up really in German. It also appeared in English.

P: Bernhard, forgive me for asking for some education here. A moment ago if I understood you, you said that you could get the turbidity from two radiation observations — one the direct.

H: No, I'm sorry. I did that wrong and talked too fast. The longer paper, as you can see from the list of things is obviously number 21

-- Daytime Radiation at Blue Hill Observatory -- and that dealt both with the observations, or measurements of solar and sky radiation on a horizontal surface throughout the day and also the measurements which were made 3 or 4 or 5 times a day, I don't know how many times, of the direct solar radiation and the amount of solar radiation falling on a surface.

P: How was that done?

H: With the Smithsonian pyrheliometer.

P: I thought that's what gave you the total solar and sky ...

H: I think it's called a pyrheliometer.

P: There is the Eppley pyrheliometer, that I believe you used the Eppley ... wait a minute, you used the ...

H: I can get the paper. I have it.

P: Perhaps this is getting into too much detail, but what I'm getting at is how do you get at the turbidity from radiation ... ?

H: Well, if you measure the direct solar radiation and, say, when the sun is 60° high -- 30° zenith distance -- you make one measurement. You make another one when it is 30° high and from both these observations you can compute the turbidity factor. Of course you know theoretically anyway, or at least you think you know theoretically, what the extinction, or whatever you call it, would be in a pure atmosphere, where there were just the air molecules. Now when the other junk is in the atmosphere too, you get a stronger absorption, and the Linke turbidity factor actually tells you how much larger the extinction is in the actual atmosphere than in the pure Rayleigh atmosphere.

[Postscript 1-9 by B.H.: Here we omit a somewhat disjointed and confused discussion about the instruments used to obtain the data for these different papers. It suffices to state that an Eppley pyrheliometer was used at Blue Hill Observatory to measure the total (solar plus sky) radiation on a horizontal surface, while a Smithsonian silver disk pyrheliometer was used to measure the solar radiation intensity on a surface normal to the solar beam. The total radiation on Mt. Washington, N.H., discussed in paper 38 is based on observations obtained with an Eppley pyrheliometer.]

...

P: So, that was the basis then, for instance, you did statistics on the monthly averages of hourly radiation, and that was first done for the Mt. Washington data in 1933, 4 and 5, and later you did a similar study for the Blue Hill data.

H: Yes, the Blue Hill, this paper 57 I think there is a sequence to that as a matter of fact, yes 61, 57 and 61 ...

P: 57, 61 and also 66, they follow in a sequence.

H: Well, they were all direct solar radiation.

[Postscript 1-10 by B.H.: Papers 57, 61, 66 deal with total solar and sky radiation on a horizontal surface.]

P: That was for a different purpose, however.

H: Well, actually, my main motivation was, there were all these thousands and thousands of cloud observations at Blue Hill which nobody ever used, and so I thought I would at least do something with them.

P: Right, and I was struck by the way in which you analyzed the data by least squares, fitting this (A over M) (E to the minus BM), where M is the secant mass. And then A and B were the disposable constants chosen by least squares after you had segregated the data in various ways as to cloudiness, cloud density, was the first study and in the second study you did it by cloud type.

H: Yes, because there were all these data floating around.

P: But, in a way this is what we would today call parameterization of radiation, isn't it? It's the kind of thing that could be, in principle, could be used in a prediction model.

H: Yea, I suppose so. As a matter of fact, I think it would probably be something very useful to use for studying the feasibility of solar heating. You see, in many places you have observations of the cloudiness, but not of the incoming solar radiation.

P: Interesting point, I wonder whether ... do you think the solar people have followed in your footsteps?

H: Well, I don't know. At one time when I was sitting peacefully half asleep in my office here, some people from some research organization, I guess a not-for-profit research organization, wanted to know if I had done anything more with all these radiation things, and I

told them no, and that was the end of the conversation. Then, some of the meteorologists at the University of Alaska have a project in fact to measure the solar radiation in Fairbanks and in various other places in Alaska, largely for the purpose of solar heating. I told them to do these things which I had done, but as far as I know, they have never done them. Incidentally, it might sound funny that you think of solar heating in Alaska -- using solar energy in Alaska -- but it is entirely feasible, of course, during the summer and other seasons, too. In fact, one of the people at the Geophysical Institute uses solar energy to heat his house, partly, and also his greenhouse. Well, that's beside the point here.

P: In paper number 57, analysis of the Blue Hill radiation data, you mention that you did the work as a Lawrence Rotch Fellow at Blue Hill, in 1945.

H: It was when the paper came out, so that must have been at the time when I was at MIT.

P: That you were a Rotch Fellow? At MIT? No.

H: No, I was associate professor of meteorology at MIT and I guess also Abbott Lawrence Rotch Fellow at the same time. Double-dipper, they call it.

P: Who was Rotch?

H: Well, he was the original founder of the Blue Hill Observatory. He was, I guess, a fairly well-to-do man, who built the observatory, and then later gave it to Harvard.

P: Do you have any idea what induced him to do such a thing?

H: I don't know, I guess he was interested in meteorology.

P: I think he was, I think he made some of the earliest kite ascents in that part of the world.

H: I don't know very much about him, but he had of course, for a while as his assistant H. H. Clayton, and H.H. Clayton was one of the people who I think started cloud observations at Blue Hill, which of course in the late 19th Century, was the thing to do because it gave you some idea about the motions of the upper air. In fact, there is a reference in F. M. Exner's book on dynamic meteorology at least in the second edition, under law by Clayton-Egnell. Clayton is this Clayton, and Egnell is an Englishman who as Clayton explained to me really went quite a bit farther than he, Clayton.

P: What is this law?

H: The law is simply that the air at all levels transports about the same mass, that is, ρ times V is apparently constant, they decided, Clayton and Egnell both.

P: ρ times V ...

H: Yes, density times velocity.

P: Yea, they're thinking of the vertical variation of that?

...

H: Well, I don't know that it's really a very accurate law. I have seen that reference only in Exner. You see, Exner was the book which I studied thoroughly for my doctor's examination.

P: That reminds me, did you have any other comment you'd like to make about radiation?

H: No. I could just say that for me that always was really just very much a side line. I must say I always liked to work with data.

P: Well, I think that's evident from the very beginning, starting with your doctoral dissertation, and continuing through almost every one of these topics that we've discussed here today, but what you said a moment ago about Exner reminds me to ask you, and now, if I may ... Incidentally, just to interrupt myself for a moment. We have I think maybe 15 minutes left on this tape, so maybe we should use that opportunity to go back and review some of the things we've discussed to see whether there are any other comments that you would like to make about them. But -- Exner, and the vertical distribution of pressure change and temperature change. In your thesis, you mention Exner in this connection. What really was Exner's position on that so-called controversy between the lower and the upper atmosphere?

H: Well, in various parts of his book, dynamic meteorology, he refers to the upper atmosphere as the more important, more influential part affecting the weather in the lower atmosphere, but then there are again also passages where he explains quite well why actually there is this apparent greater importance of pressure change in the upper layers as compared to the lower layer, so I guess the word to characterize his position would have been ambivalent. Of course ... or maybe he just wasn't too careful writing his book. I mean, there are some really wonderful expressions in Exner's book. You know, he had his own theory about the way cyclones originate. One of his ideas was

that actually the flow of the air as it for instance streams across the southern tip of Greenland, the flow of the air would be disturbed and there would be a vortex forming behind Greenland.

P: A wake?

H: Sort of a wake, yes. And he refers to this particular case where ... oh yes, or it could also form he said, if there is an outbreak of cold air from the north, then this cold air would sit squarely in the path of the air streaming from west to east and again a wake would form behind the cold air, and he refers to that as the armpit of the cold tongue, and I translate that literally. The only person who apparently is aware of that was Bergeron, because he quotes Exner in one of his papers.

P: I wasn't aware of that concept.

H: Well, in German it's called the Riegel theory. The Riegel is the bolt with which you lock a door. But I mean nobody ever I think paid much attention to that, it's fair to say.

P: Now, at that time there must have been a picture of the sea level pressure distribution showing something like the Aleutian Low, I would imagine, and he certainly must have been aware of that. Maybe that theory was designed to offer an explanation for the Aleutian Low.

H: No, I think that was in general for the moving cyclones of all latitudes, otherwise he probably would have thought anyway more of Icelandic Low.

P: Did I say Aleutian? I meant Icelandic because of your reference to Greenland. That topic of pressure change -- of the structure of pressure change with height -- I think is a very interesting one. It would be well worthwhile if someone wanted to suggest a useful dissertation on the history of meteorology, to review the role that these investigations played in the development of our knowledge of the way the atmosphere fits together. This is another point I had wanted to ask you about, that just occurred to me now. Margules, and I think that was in 1905 or 03 talked about what in effect was the compensation that takes place in the atmosphere, and I think in many respects, the kind of thing that came out of these studies of pressure change and temperature change was an aspect of the vertical compensation taking place. In that sense, there was a connection between Margules and this work, although I don't recall that it ever appeared in the published work on the subject from the standpoint of the Schedler approach. Of course, Schedler's work developed more of the thermal side of the subject, whereas Margules was looking at the dynamical

side. Did you have any other comment that occurs to you to make about that area?

H: No.

P: Let me see ... symmetry points. Nowadays, people are talking about 5-day waves, I don't know whether anyone is talking about a 24- or 36-day wave.

H: Actually we have a talk one of these...in this Friday seminar, in which you will speak too. Roland Madden has a 50-day wave. Of course, that all reminds me very vividly of the time when I was first in Boston, at Blue Hill Observatory, when we were at one seminar where H. H. Clayton gave a talk, and of course at that time Clayton was quite hep on sunspots and I think it was in that connection, any way, Clayton gave a talk and the question of periods came up very much in this talk, and Carl Rossby afterwards got up and said he had the impression that if one plotted the various periods whose discovery had been claimed, one would really get a continuum. I don't really think that Clayton liked it.

P: What were some of the periods that Clayton was in -- he was a sunspot man, right?

H: Yes, but he had other periods too. There were, well God knows.

P: Did he have anything less than a year?

H: No, I don't really know whether Clayton, in particular, had anything but the 11-year period but it just came up at that time, and it seems to me that it's the same again that ... Well, of course if you have these sophisticated methods, it's very nice to have them, but then you really wonder how much, well I guess you call it power, how much power there is in many of these periods, how significant they are. I showed you this point diagram of the lunar tide for the 14-day period, didn't I. I think I said at that time, I think unless you really can do something like that by just merely seeing it, if you just plot it very unsophisticatedly, then you should be very suspicious. Of course another thing, also, is if you don't have any physical explanation for the periods, the only trouble is the atmosphere has so many degrees of freedom that you can really explain everything.

P: Remind me again about that lunar fortnightly tide, where was that published?

H: Oh, that's in the Zeitschrift für Geophysik.

P: A recent paper, then.

H: That's quite a recent paper. I have a copy right there. If you want it, I'd be glad to give it to you.

P: Well, Bernhard, I think we're coming to the end of our first session here.

[End of side 2, tape 1]

TAPE 2
28 April 1983

Platzman: This is the second of the interviews of Bernhard Haurwitz, and it is being conducted on Thursday, April 28, 1983. We are about to proceed to the next general topic: Vortex motion and tropical cyclones.

P: Did you have any afterthoughts about any aspects of the discussion yesterday?

Haurwitz: No. I had not any thoughts, I must admit.

P: The first paper in this general topic, vortex motion and tropical cyclones, is the one where you give a resume of this apparently quite remarkable work by this man Diro Kitao. I never heard of him before you mentioned him to me some days ago, and I found this summary just fascinating. I'd like to go back and try to learn more about what he did.

H: Well, as you know -- did you want to say something, or shall I?

P: No, please go ahead.

H: As you know, I got acquainted with that in Leipzig through Professor Weickmann, the Director of the Geophysical Institute, and as a matter of fact the way the whole thing came about is that I had gotten my doctor's degree in the summer of 1927 and I wanted to learn dynamic meteorology systematically ...

P: I thought you were going to say Japanese.

H: No, no: dynamic meteorology. The thing is, during the four years, or three years, when I was in Leipzig, Weickmann never got around to his lecture on dynamic meteorology. I had thermodynamics, but as far as dynamics was concerned, I learned it by reading Exner's textbook on Dynamische Meteorologie. So I suggested to Weickmann that I would like to sit in on his lecture and he said, "Well, of course. I would be glad to have you." I think he even said he would at least be sure that one of the students understands something, and I suggested that I might also give a problem session in connection with that, which he said was fine with him, and I did that. During that lecture, he took quite a bit of the work from the papers of this Japanese. For some reason we had the collected works of Kitao's, this Japanese professor, in our library in the Geophysical Institute in

Leipzig. I don't know how it did come there originally; it may originally of course have been there because of Vilhelm Bjerknes, the first director of the Institute -- he may have had it and put it in there. But at any rate, that's the way I did learn about it. Then Weickmann suggested to me since the work of this Kitao was quite widely unknown, it might be nice to publish brief excerpts of it and I said well, I would be glad to do that. So he said go ahead -- and this is how it came about. We sent it then to -- rather, I sent it -- to Gerland's Beiträge, and that's the way the paper originated. There was of course nothing original on my part.

P: You mentioned here that he studied with Helmholtz and Kirchhoff. Where did Helmholtz hold forth? I've forgotten.

H: Well, unless I am very much mistaken, they were both in Berlin.

P: Ah, yes, that's right. Both meaning Kirchhoff and Helmholtz. This summary, which I'd like to discuss in a little more detail in a minute, deals with one aspect of Kitao's work, namely on vortices. But you mention here that there was a lot more. Did you ever follow this up with further ... ?

H: No, as a matter of fact I have not been very complete in what I said before in explaining the history of this paper of mine, this review. But actually he had quite a bit of material in there (Kitao), which was a bit earlier. A bit earlier he dealt with Guldberg and Mohn in Norway -- I don't know if that means anything to you.

[Postscript 2-1 by B.H.: Kitao himself quotes Guldberg and Mohn's study at the beginning of his first paper, regretting that it was not available to him.]

P: Oh, yes.

H: I thought it would, and probably to anybody who listens to this it will mean something too. And when I wrote my report originally, I wrote it, in fact, including quite a bit of those results in a much shorter presentation, much abbreviated, and I also indicated for the printer, if it would be published, that it should be in smaller print, because it seemed less interesting than this stuff which at least at that time, which is in there, was quite well known and especially was treated quite adequately by Guldberg and Mohn. In fact, the paper was first sent to Exner, editor of the Meteorologische Zeitschrift, the organ of the German and Austrian meteorological societies. And Exner objected to it because he said there was too much mathematics in it, because most of the discussion about these vortices is largely mathe-

matical. There is in this paper of Kitao's and consequently my review, there is no attempt at application to observations. So Exner objected to that and said, curiously enough, the only applications or reference to observations is in the small print. There are, of course, such things as what we call geostrophic wind now and gradient wind, for instance, and which I had on purpose put smaller because it would seem that that would be less interesting for people who know some dynamic meteorology.

P: So I gather that that version of your summary is not the one that was eventually published here.

H: No. Then I just decided -- well I talked to Weickmann of course about it. He was more or less the sponsor and that was the first longer publication I had after my doctor's thesis. And I said, there really is no point in being so elaborate about these results of Kitao's, which he found for himself, even though Guldberg and Mohn had already derived them elsewhere. He agreed -- I don't know if that was my idea, or he said to me "Well, why don't you send it to Conrad?" Conrad was the editor of Gerland's Beiträge at that time.

P: But without the fine print.

H: Yes, without the fine print. If you want to read that, you have to go the original papers. As a matter of fact, I really hadn't looked that up, unfortunately, before this talk or before the last time. I think there is an error somewhere in that connection -- in Kitao -- where he says that the deflective force of the earth's rotation only acts on the meridional component of the velocity vector, not on the longitudinal one, the west-east motion.

[Postscript 2-2 by B.H.: My recollection is wrong. Kitao does not say that the Coriolis force acts only on the meridional component of motion. What Kitao claims to have derived, on p. 149 of his first paper, is that an air current which is purely longitudinal remains so and is not deflected by the earth's rotation. This conclusion is briefly considered in my paper now under discussion (#4, p. 85).]

...

P: Maybe after we discuss this a little bit, we can talk about what other work he did that was not summarized here.

H: Well, I'm afraid I won't be a very good source of information because I simply haven't looked at it for years.

P: Okay. Whatever became of such things as your manuscript material, for instance you were speaking of the "fine print" ... ?

H: Well, that's very sad, in retrospect now. First of all, after the paper was typed, I would of course have a copy but would throw the manuscript away, and then after the paper was published I put the typescript away ...

P: You threw it away ...

H: Yes.

P: That has the great virtue that you don't have to carry around a lot of cartons full of old papers wherever you go ...

H: Well, of course, in part this stems from the time when I came over to this country. I came for seven months and well, I'm still here, and there was no way in Germany to keep these things.

P: This is anticipating actually the next paper on vortices, but in that next paper you point out that Kitao in this study assumed the cross section to be small. In my very quick reading of this, I didn't catch that assumption, but it is so then that he's dealing essentially with not exactly point vortices, because he does make some allowance for the internal structure of the vortex in terms of the vertical motion that can take place there, but in order to derive the formulas for the interaction between the vortices, he assumes that their cross section is small. And in that sense, is it correct then to interpret the work as a slight generalization of the vortex filament problem?

H: I would think so, yes. And as I say -- or I didn't say, so far, but I think I have mentioned it before at some other occasion -- that much of what he has in there or of the material which I describe in there, is already found in some lectures of Kirchhoff's. Yes, we had the lectures of Kirchhoff, which apparently were printed, we had those in our library too, in Leipzig, and I looked at them and remember having seen some of these results which he describes there.

[Postscript 2-3 by B.H.: With respect to the size of the vortex area Kitao assumes that the cross section of each area is small compared to the distance of the vortices from each other. (See my paper #4, p. 94 and Kitao's second paper, p. 361.) This assumption permits the development of relatively simple equations for the motion of vortices relative to each other. Since Kitao does assume vertical motion in the vortex areas it would probably not be entirely correct to speak about vortex filaments.]

...

P: I found it quite interesting, the use of Helmholtz potentials, which are so closely identified with Helmholtz's name. And much of the formulas that you describe here are based upon these stream function and velocity potential representation of the velocity field. Do you recall whether that was your first contact with this way of representing the motion? ... or I imagine that the ...

H: I don't really think so, because I must have had that in some lectures, in particular, in some lecture on hydrodynamics, that was mostly technical hydrodynamics in Leipzig. Also, I took vector analysis in my first term at the university.

P: Well, you point out here that Kitao apparently did not use vector notation, and this made the reading of his work very tedious, so ...

H: Yes, it would have been quite a bit easier.

P: This is one thing that you did in your summary, to recast it in vector notation. I noticed that right at the very beginning he derives, or you derive, a summary of his work ... the vorticity equation, and also what we now call the divergence equation to go along with it. Of course, those things, I trust, were already quite well known through Helnholtz's work.

H: Oh, I would think so, yes.

P: The relation between the frictionally-driven secondary flow and the primary circulatory flow is interesting in the way he works that out, and he gets an expression for the velocity potential of ... the divergent horizontal flow associated with the transverse circulation. In terms -- he gets that expression for that velocity potential -- in terms of the stream function of the primary flow and a multiplicative factor involving the friction coefficient and the Coriolis parameter. This is actually quite a neat result that is somewhat reminiscent of discussions of frictionally-driven secondary circulation that one sees in text books nowadays. And one thing that to my mind would have made his treatment even more interesting would be, but here I may be mistaken, a discussion of the time-dependence of the circulatory flow, but am I right or wrong in thinking that he didn't consider that, that he considered the circulatory flow to be steady? He only considered really, essentially, steady-state.

H: Yes. Well because he has this friction effect in there, the motion decays with time.

P: True, but apart from that, there is no mechanism to change the gradient wind balance in the vortex itself. Of course, if such a

mechanism had been considered, such as heat sources or something of that kind, then he would have been led to a more general consideration of secondary flows driven by other mechanisms than friction and this would have been very similar to the kind of analysis made in modern times by Eliassen. I was going to ask you what other work you can think of, not in any detail, but what other problems did he tackle? Do you recall, at all? Did you mention something about the general circulation?

H: Well, it probably has something in there, I don't remember now. It would be a very easy matter, I have the book here.

P: Oh, you showed it to me, yes.

H: I have all the papers here.

P: I wonder whether he was associated with Oberbeck in some way.

H: Well, I wouldn't know, because I'm not clear now, actually. Kitao was about the turn of the 20th Century, and Oberbeck must have been about the same time. I would have to look at the reference which he gives. ...

[Postscript 2-4 by B.H.: In his first paper Kitao also computes air trajectories in vortices. In his last paper he considers effects of vertical motion on the individual storm and also time-dependent motions. A somewhat cursory search through the 279 pages of Kitao's three papers does not show direct references to the "general circulation." But the first article contains references to the trade wind and doldrum belts and to the possbility of air motions between high and low latitudes. Prof. Weickmann used the basic equations as formulated by Kitao for his discussion of the general circulation of the atmosphere in his article on Mechanics and Thermodynamics of the Atmosphere in Gutenberg's Textbook on Geophysics (see paper 18). Kitao mentions Oberbeck repeatedly in his papers, but I doubt that the two met.]

P: Well, I'd like to look at that book, actually. I didn't have any more questions about Kitao's work. Did you have any other comments about it?

H: No, of course, as I suppose you want to briefly mention this here too, which is really the direct sequel of the early one.

P: Actually, almost immediately after you did this summary of Kitao's work, you published paper number 7, which is on the motion of vortices with finite cross-sections, and there you remove this restriction that Kitao had imposed.

H: Yes. Well, the idea if I remember that still, is that it seemed to me at that time and Kitao — although I couldn't put my finger on that -- I'm sure Kitao also thought of tropical hurricane motions when there are two tropical hurricanes not very far from each other. But, it seemed to me then that it would be better if one wanted to study something like that, to have rather vortices of a finite extent. Then, of course, the simple Rankine vortex seemed the simplest one.

P: Well, it's interesting that in this group that we're speaking of now, paper number 7, published in 1930, is a theoretical study, a study of the theory of the problem. Much later, and we'll come to this eventually in this same general topic, paper number 75 published in 1951 deals with the same problem, but now in light of data that had in the interval become available on hurricane motion — but we'll get to that in due time.

H: As a matter of fact the reason why that came up suddenly later again was because one of the students came to me talking about a master's thesis, work for a master's thesis, so I still had this in mind, this kind of thing, and gave it to him. The student incidentally is ... what's his name, he was just department chairman at Seattle, after Fleagle.

P: I read his name and I knew it but I have forgotten it -- I think I have that paper right here.

H: Badgley. Yes, he did it. As a matter of fact, no, it probably can't have been his master's degree because he was the one who got a master's degree but without writing his thesis, which was a mistake of the records office.

P: In those days a thesis was required. Nowadays they often dispense with it.

H: Well, at NYU it was not required, except by certain departments. You see, the department could have more stringent regulations than the school, of course, and we had a more stringent regulation, but one day Frank Badgley came to me and said, "Well, thanks for the master's degree." So I looked at him and said, "Well, we had talked about it but you weren't going to take a master's degree; you were going to go right for the doctor's degree." He was obviously completely qualified to get a doctor's degree, so there was no reason for him to take the safe way out. So it turned out that the records office just had goofed; they should of course have waited for a letter from me to confirm that he had fulfilled all the requirements. Well, anyway ...

P: Now, in this paper, number 7, based upon Kitao's work, I believe — is it true that you maintained his assumption about the inflow into the vortex?

H: Yes. What I used to call, or what we in Germany and Austria at that time used to call Guldberg/Mohn's assumption about the friction -- that the friction is proportional to the wind vector.

P: I noticed that you referred to it that way; I wasn't aware that that device was connected in meteorology with them.

H: Well, probably among meteorologists they were the first ones to introduce it, and I heard only later that it was Rayleigh.

P: Yes, it's now often called Rayleigh friction, although perhaps historically it would also be correct to refer to it as Newton friction. Laplace certainly did use it.

H: Yes, Laplace certainly had it. In fact, after I looked at Laplace's five volumes, once, I found it in the article in the book on tides.

P: Well, you were very brave to try to wade through that very difficult notation.

H: I didn't really wade through.

P: One question I had about paper number 7: If these vortices are of finite extent, would there then not be a deformation which one induces upon the other, so that the shape of the circulation of the vortex would change?

H: Well, I suppose the pressure field would be, and the streamline field would ... yes, it would be different. I don't know. I don't think the assumption is there that the vortices are infinitely far apart from each other, is it?

P: No, because I think you do consider the center of vortex mass and the rotation around the ... but that's an aspect that you didn't get into.

H: No, because everything was strictly symmetrical.

[Postscript 2-5 by B.H.: Although in paper number 7 each individual vortex is assumed to have circular symmetry, the superposition of the two symmetrical vortices disturbs the symmetry of each vortex to a certain extent.]

P: I don't have any other specific questions about this, Bernhard. Oh -- yes. One of the features of the problem in common with Kitao's analysis was the fact that because of the frictional secondary circulation the vortices tend to move either toward each other or away from each other, depending upon whether they're cyclonic or anticyclonic. Do you think that — maybe this is getting too far ahead, but I noticed that in the more recent paper number 75 -- we'll come to that shortly -- you discard this feature of the problem. Is that because you think there is no observational evidence?

H: No, I never tried the observational evidence, but I'm not really quite clear now what happened, but I remember that when I computed the pressure field, I found that the solution which I get for the pressure field is not single-valued. And I really didn't know what to do about it at that time, so ... it's a bit nebulous now, because that is about 25 years past. But I decided about that time -- I looked at some of the maps of Badgley, as he looked at some of the maps, and he decided — or we decided — that we better don't study or don't try to find out anything about the approach to the two systems. It was essentially that I felt that there was a mathematical difficulty there. As a matter of fact I might also say with respect to this latest paper which we just discussed: you will remember that by just looking at it, first of all there are some earlier data which ... there were about 50 cases or so, I think, of earlier data and only about 8 or 9 cases of newer data.

P: You're referring to the later paper?

H: Yes, that's the later paper, yes. Oh, we don't talk about that?

P: Well, we can; it's fine.

H: Well, I was just going to say, actually, the earlier observations which were just taken from weather maps when there was probably much guessing there, gave not very good results. By good I mean that really the theory didn't seem to be very good. The theory was much better with the newer data.

P: Yes, I remember that from that later paper. Partly it may be that, as I recall, the earlier data were based upon ... it doesn't fit a later paper?

H: Yes. Well, we can wait if you want to come back to it.

P: The earlier data were based upon the historical weather map series, and it's rather difficult to pick off pressure values from those maps. They're simply not large enough to do accurate work.

H: Yes. Well, I don't know if you noticed that in the later paper -- that was work which I did probably two or three months ago.

P: Three months ago from when?

H: From now, from today.

P: No, I didn't know. On those sheets?

[Postscript 2-6 by B.H.: In the discussion of paper number 75 dealing with observations of binary tropical cyclones the "sheets" referred to in our discussion are plots of computed against observed angular velocities of the cyclones about their common "center of gravity." For the newer 9 cyclone pairs the correlation between observed and computed angular velocities is 0.76, not bad, but not good either!]

H: Yes.

P: I saw those sheets but I didn't look at them. Perhaps I should have.

H: Well, then I didn't date them or anything. I made some calculations for the 9 newer hurricane pairs. The correlation didn't come out too badly between calculated and observed motion. The correlation was about 0.8, which isn't very good.

P: It's interesting that you say that, because as I turned the pages of the paper I was waiting to see a scatter diagram. I see that's what you have here.

H: Yes. The older one is lousy.

P: Ah, yes. I overlooked this. I don't know why I didn't look at these sheets. Yes, that's a very good scatter.

H: Well, the trouble with me these days is that I'm not at all ambitious to produce, still.

P: Bernhard, this brings us to the Ekman spiral in a circular vortex. And this in fact has led to some other quite interesting work later on, having to do with tropical cyclones. I'm curious to know, if you can remember, what got you on to that problem. I don't think you make that clear here.

H: No, as a matter of fact, you must have noticed that this paper in particular is just a very brief summary. Well, no ... I have a vague recollection what I wanted to do is I wanted to find an explanation

why ... well, to put it that way ... the hurricane has this eye of the storm, as it's called.

P: So you had that idea right from the beginning.

H: Yes. And I thought of all sorts of things now -- I don't know if there are any good explanations today. What I had in mind was that there should be something which comes directly out of the hydrodynamic equations, which tells you that this influx into the hurricane in the outer part has got to stop somewhere. But from what I could find at that time, I just couldn't get anything. I thought finally that maybe with friction one could do something, and then I came to this Ekman spiral for a gradient wind motion. And that is how this paper came about.

P: I see.

H: ... as really a separate problem. I might of course say that ... this is probably also not clear from that paper ... but at the time this was written, this paper, there was also something else, namely, the coefficient of eddy viscosity at that time was often determined by determining the height of the geostrophic wind level, or gradient wind level. And this paper then shows that this can be dangerous if you have curved flow. As a matter of fact that's one of the points which Ertel makes in connection with that. You know Ertel wrote a book, a short book, on dynamic meteorology. He mentions that in there.

P: Now we're talking about paper number 25 published in Gerland's Beiträge "On the change of wind with elevation under the influence of viscosity in curved air currents," which is a detailed account of the summary published as paper number 22. I must say that although I looked at this work many, many years ago, I had not recalled that the effect of curvature is a great as it is, on lowering the gradient wind level.

H: Well, of course, it is ... the examples which are taken in there are pretty extreme.

P: It's also interesting, I thought, that while the correspondence between wind direction in the Ekman spiral and the elevation at which that wind direction occurs is quite different in the curved case as it is in the straight case, the shape of the spiral, however, is almost the same. That's as you can see, from this diagram here. ... At the end of the paper, you may recall that you ... well, first of all, let me say that the mechanics of doing this exactly, taking the gradient wind relation verbatim without approximation, is rather difficult, actually, and it leads to some rather exotic mathematics, such as zeta functions and theta functions.

H: Actually, of course, anticipating this contribution to it, makes it easier to simplify it later on. I have to admit that the simplification is due to a mathematician. We can come to that later, if you think ...

P: At the end, however, and now I'm going back to paper number 25, at the end of that paper you show how essentially the same results could be obtained by perturbation methods.

H: Yes. As a matter of fact, that was done at the time when I was still at Blue Hill Observatory but worked quite often at MIT and you find a reference to Rossby there.

P: Yeah, I noticed that.

H: Rossby suggested that to me, but I had already done it too. Now there is, however, one thing which is very discouraging about this approximation which came out much later. You see, there is a fairly far-reaching assumption in there about the horizontal convergence of the basic gradient wind. Let's see — the radial wind, I think it is. Oh yes, here: this assumption here, that the vertical velocity is this, and ... is that V?

P: V sub r.

H: ... is also a function of Z, the height, and r only, that is, proportional to the distance from the center. And this makes it a very special wind distribution.

[Postscript 2-7 by B.H.: Actually the special assumption introduced in paper 25 to obtain a solution for the Ekman spiral in curved air currents states that both the tangential and the radial component increase with the distance r from the center directly as r.]

P: V sub r is the tangential velocity, so this is a solid rotation.

H: No, V sub r must be the radial.

P: You're right; it's the radial.

H: Yeah, the reason for that is that I really want to get constant coefficients in the equation so that I get ... a differential equation for the vertical distribution, which has constant coefficients with respect to r. I have ... a few years ago, I did make some calculations, dropping that assumption, but well then the equation becomes of course much more complicated. One would probably have to use numerical methods to solve it. I tried to use the same approxima-

tion method as is given in there at the end -- as is given in paper 25 — but unfortunately the terms which are neglected are quite large. And I was quite conscious of that because I discussed that paper later. That paper as I say was written in Boston still, but not very long afterwards I went to Toronto to the Canadian Meteorological Service and I gave a seminar on this paper in the Department of Applied Mathematics. And the professor of applied mathematics, Synge, he was then the professor there, and he asked me, "Have you been curious enough to check from the solution how good your approximations are?" And they turn out quite well in this particular case, but as I said, a few years ago when I dropped some of the simplifications, the simplifications were not as good any more.

P: I understand. And the particular case you refer to again is the one where the radial velocity is linear in r. That implies, doesn't it, that the horizontal divergence distribution is independent of r, so that all over this vortex you have essentially the same vertical motion everywhere, and it's only a function of Z.

H: Yes.

P: Well, I think that's certainly reasonable for the internal part of the vortex circulation. But getting back to the perturbation analysis, the impression that I have from that as compared with the rectilinear case of the Ekman spiral, you can summarize the difference by saying that instead of the Coriolis parameter, which appears in the rectilinear case, you substitute the absolute vorticity of the flow.

H: Yeah. In fact, let's see: it's Haltiner and Martin who wrote the dynamic meteorology, isn't it?

P: Yes.

H: If I recall that now correctly, they describe it exactly that way in their book. Which is, of course, a very good way to describe it.

P: So, paper number 26 is the one you mentioned a moment ago, the simplification of the exact treatment of the problem.

H: Yeah, and that came about because I talked to a friend of mine in Toronto, as a matter of fact, the only other German Jewish refugee, who was at the university at that time ...

P: Who was that?

H: His name was Richard Brauer. He was then ... he is probably five or six years older than I am, and he was then an assistant professor

for mathematics. We got quite well acquainted, and I told him about this problem at some time and how I had solved it, and he said, "Well, this is certainly right, but you know there is a much simpler way," and then he just proceeded to give this method. So I said well heck, I really ought to publish that, I should give your name, but he implied that it seemed to him such a trivial thing that he didn't want to be mentioned. As a matter of fact, the interesting thing is that he was not a specialist in elliptic functions. He was actually an algebraist; in fact, I read that he died about two or three years ago, and just a year before his death he got one of the medals of science so he must have been a very good mathematician.

[Postscript 2-8 by B.H.: Richard Brauer was about 4 years older than I. He died in 1977.]

P: Did he remain in Toronto?

H: No, I don't know where he went from Toronto, but he ended up at Harvard.

P: Continuing on the same topic, namely, the way in which curved flow, particularly in the vortex, changes the structure of the Ekman boundary layer, we come now to paper number 27. I think this is number 27.

H: No, 27, sorry, would be this.

P: Sorry ... that is this one ...

H: That is still on the subject of the Ekman spiral.

P: Here, now, you take this subject into the realm of observational data ...

H: Yes.

P: Trying to find ... now this was published in 1936.

H: Yes, that was all pretty close together.

P: But I mean not a great deal was available in terms of ...

H: Yes, that was the trouble, of course. At that time, there was I think really nothing known about the upper atmosphere over a tropical cyclone.

P: Yes. But now, here you bring out more explicitly the fact that the gradient wind level is lowered as you go in toward the center of the vortex, and this gets back to, as you described a moment ago, your original motivation for looking at this in terms of the eye of a hurricane. And I thought it was quite remarkable that you estimated that given a sufficiently strong curvature and a sufficiently strong wind, the gradient wind level could be as low as a hundred meters.

H: I don't remember that any more. May I?

P: Yes, please. I made a note of that because it struck me as so remarkable. In effect, it says that the inflow has almost virtually ceased at that point, I suppose you might say.

H: Oh, yes, I do remember something about that now.

P: You had two categories of data here, one coming from rather weakly curved systems, the cyclones and anticyclones, midlatitude, and the other case you referred to as a cyclone in the Arabian Sea. Now, I trust that that is a tropical storm.

H: I imagine so.

P: Because you talk about radii as small as 40 km.

H: Yeah, well, I think the reason for that is, didn't they call the tropical storms in the Arabian Sea "cyclones," for some reason.

P: I think that's true, yes.

H: Maybe it should have been cyclones in quotation marks.

[Postscript 2-9 by B.H.: The "cyclone in the Arabian Sea" is indeed a tropical storm judging from the wind intensities reported by Chambers and reproduced in Hann-Süring's Lehrbuch.]

P: So in the first case you test the theory, the theory being the way in which curvature affects the angle between the surface wind and the pressure gradient. You test that from data taken from Loomis, a statistical study of Loomis, which is reproduced in Hann.

H: Oh, well, then I probably just took it from Hann. Well, at that time, at least for anybody who studied in central Europe, of course, as far as observational material was concerned, if it wasn't in Hann, it didn't exist. Well, that was a very nice feeling, you know: you looked ... have you ever seen Hann? It's about as thick as that.

P: Yes, I have a copy of it.

H: Oh.

P: What else did I want to say here? Oh, yes: now you also looked at this case we were speaking of a moment ago, the cyclone in the Arabian Sea, that the data for that dated back to May 1881, which certainly indicates how difficult it was in those days to come by suitable data for any study like this. But there is one interesting aspect of this that you remark on. I wonder whether you can ...

[End of side 1, tape 2, beginning of side 2]

P: ... I was asking you whether you can recall the explanation as to why the angle between the surface wind and the gradient increased again after having decreased with distance?

H: Yes, I think I can. Because, actually, this formula here is not the formula which I used -- that is, this formula which is based on the perturbation theory, or even if it were the more accurate one -- but it is the one where this expression $\delta \upsilon_g / \delta r$, that is, the change of the distribution of the gradient wind with the distance from the center — that is, of course, assumed in the mathematical solution which I give ... assumed to be equal υ_g/r, but in making this computation, I have in fact used the data.

P: I see. Right.

H: Yes, here you can see on page 210 of this particular issue of the Gerland's Beiträge, this is the formula which I actually used, and there is this expression which means a and this means b and in the b this appears and that explains this curious behavior of the angle psi.

P: Okay, it was then a case of, you might say, variation of parameters, so to speak.

H: Yes, I'm praying that you can apply the solution when it comes from a non-varying parameter.

P: Well, it usually works, unless there's an unexpected change of sign. Did you have any other comment about that particular paper number 27?

H: No.

P: This now leads to a different topic, but still not totally different because it deals with vortices, and that's paper number 32.

H: Excuse me. You don't want to talk about this paper here first, 75?

P: Now, I should have in front of me the list of the titles of the papers and the numbers — that's one thing I don't have. I'm taking them in chronological order. In subject matter order, you're right: 75 really should come first, and let's do that. We have already talked about that a little bit. It gets back to your old work with Kitao, and this is now a practical application of that to tropical cyclones. I thought it was very ingenious the way you transformed the kinematic aspects of Kitao's procedure into a more dynamical context by bringing in the pressure field of ...

H: Yes ... of course, you have to do that.

P: You had to do that, or else you can't use the data. But, by doing that, by introducing the gradient wind into the streamline representation of the flow that was involved in your earlier study, you were able then to find a way to determine the intensity of the vortices as needed in the Kitao formalism. Then you proceeded to work with the Northern Hemisphere historical map series and found an amazing total of 87 cases. I had no idea that you would find that many. Now, what was that, 40 years of data?

H: I imagine so. I think, now as we go on talking about that, I think what did happen and what the connection between this and the earlier paper is, that toward the end of the war, about 1945, I was invited to come to the Institute for Tropical Meteorology, where Herb Riehl was the Director at that time, and he probably showed me ... definitely, and that's mentioned in the paper, first of all, he had a case of 2 hurricanes, Susan and Ruth, I think they were, I think that's right, but he also must have shown me some of the much earlier historical maps, and so it occurred to me at that time that that might be something worth looking into, and then when Badgely came along as a graduate student, I suggested that work to him and, of course, as you might expect him to do, he did it quite competently.

[Postscript 2-10 by B.H.: The first set of data for the study of binary tropical cyclones (paper 75) comprises 87 cases extending over the years 1900 to 1938. But it should be kept in mind that some cases may refer to the same cyclone pair which may have been observed for a number of days. See, for instance, the sequence from the 24th to 29th of August 1928 in Table 1 of paper #75.]

P: Yes. Well, what we're talking about here is an attempt to use the historical map series to read off pressure data sufficiently well to substitute in the formula that gives the rate of rotation of these vortex pairs, or hurricane pairs. And the 87 cases you found in historical weather map series, as you say, "the overall agreement between the calculated and observed rates of rotation is poor." But

then, realizing that that data set is not ideal, you went on to more modern data available from the interval 1945 to 50, I think, 1950 was the last case you used. And there, these 9 cases, where better synoptic data were available, you found much better agreement, and that's this scatter diagram that you were just showing me.

H: Yes, because there is also the temptation with such things, when one has made a calculation to repeat it again with slightly different measurements, but I have manfully resisted the temptation. I have in fact ... there is one case mentioned in this book where, in retrospect looking at one of the cases again, it would have been better, quite obviously, to take other values, I forgot for what instance in the paper and would have gotten a better agreement but it just doesn't seem honest to go ahead and do that.

P: I'm not sufficiently familiar with forecasting techniques for tropical storms, other than the use of steering-type models, but I wonder whether such interactions of hurricane pairs have .. I mean, I wonder what the forecaster does when he encounters a situation like that in practice.

H: I don't really know either, I have never asked anybody.

P: Well let's go back then, if we can to paper number 32, where you are now getting into quite a different subject. That one is on the height of tropical ...

H: Excuse me. This is not 32.

P: Isn't it?

H: No, 32 is this paper here which is connected with them ...

P: Oh, dear. I'm confused again. Just a moment, something's mixed up here. Oh, I did skip that. Why did I skip that? I guess because I was trying to maintain this continuous line of discussion of the Kitao problem. Ok, let's go back to paper 23, "The height of tropical cyclones and the eye of the storm." I must say that reading this, I got a feeling that this was a problem that gave you a lot of pleasure, and that feeling was reinforced by reading the later paper, which is number 32 on the same subject. This strikes me, this analysis of the hydrostatics, you might say, of the tropical cyclone, strikes me as being a very fundamental treatment of a very important problem. And, in a way, it's amazing that it had never been looked at quantitatively in this way before.

H: No. I think Köppen suggested that it must be so that the hurricanes must reach fairly high ...

P: But I mean he didn't sit down and actually do the calculation.

H: Of course, Köppen wasn't ... I don't know how much mathematics he really knew. You know, originally he was a botanist and became a climatologist later, but it's amazing that nobody else did it before.

P: You mean that Köppen was another case of a man who didn't like to see quadratic equations?

H: I don't know about that. He had of course his son-in-law, who probably knew quite a bit of mathematics: Alfred Wegener. You know?

P: Yes. The continent man.

H: Yeah, the continent man also the author of the first thermodynamics of the atmosphere.

P: Well, I knew he was a meteorologist, but I didn't know that he was the first such author. When was that?

H: I think the first edition of that book was around 1911 or 12. Incidentally, I think he was first a lecturer in astronomy. Would you like me to look it up?

[Postscript 2-11 by B.H.: Wegener's "Thermodynamik der Atmosphäre" appeared first in 1911.]

P: Not just now. We'll check that later. Anyhow, essentially what the question you pose here is, given the pressure distribution at the surface and the very large pressure drop associated, I mean that is found in a tropical cyclone, in a hurricane, how high must you go in order to reach a level of equalization of the pressures?

H: Yes, where the pressure is uniform horizontally.

P: And, unless you go high enough, you will decide that you have to have totally unreasonable temperature distribution in the ... and you made a number of typical calculations that brought out that point. The general conclusion that you came to, as I gather from this, is that something of the order of 8 to 10 km, in other words, you have to go essentially up to the tropopause before ...

H: Yes, that is right. Of course, there are also some sort of incidental calculations, which today, certainly, everybody would consider as entirely unnecessary, like the influence of water vapor, which is of course lighter there, but even that couldn't have much of an effect, and the other one is the vertical accelerations of the air.

P: Well, that in itself, the question of how high is the level of equalization, is really a basic question, but you went on from there to do something that I find extremely interesting, namely to, again using hydrostatics, to infer from the surface pressure distribution and a reasonable assumption about the temperature inside and outside the hurricane, to infer the shape of the funnel. You remember this picture here.

H: Oh, yes.

P: Do you know whether that had ever been attempted before?

H: I don't think so.

P: I am not aware that it had, and it certainly is an ingenious way of getting an insight into vertical structure that would not otherwise be possible.

H: I always feel today when I talk about this paper, which isn't very often any more, that in a way, today if anybody sees it, it must sound like what they call flogging a dead horse. Because people simply don't realize that at that time there was absolutely no upper air observation in a tropical hurricane.

P: Not only that, I don't think it's -- that of course is the crux of the matter, but I think that it's also true that it's very difficult for a person who has not lived through the evolution of a chain of ideas to appreciate how difficult it is to sort out basic ideas about a problem in the beginning. Once it's been done and once it finds its way into the textbooks, then everything is more or less obvious. Today, the school children are learning relativity theory ... This is very closely connected with paper number 32 on the structure of tropical cyclones, a very short note published in the Quarterly Journal. And, in a way, it puts the cap on this purely statical treatment of the problem by showing that this is entirely consistent with dynamics of the circulation. I was quite amused by Brunt's ... do you remember that?

H: Oh, yes, I remember. Well, this is where that, you see, I put that on. Then whenever I sent out reprints, I sent this [mimeographed note (BH)] along because I was a bit annoyed since that appeared in the Quarterly Journal ...

P: Without giving you a chance ... was he the editor at that time?

H: I think he was the editor, but I'm not quite sure, he may not even have been. At any rate, the thing is that he hadn't read the other paper.

[Postscript 2-12 by B.H.: At the time when my note (number 32) in the QJRMS on the structure of tropical cyclones appeared (1936), the editor was Sir Gilbert Walker, not Sir David Brunt.]

P: That's exactly ... in my notes here Bernhard I say, "Brunt's comment indicates he may not have read number 23."

H: Yes, well I think this is quite true, and I think that was my first publication in the Quarterly Journal. When was that anyway, 1936. Yes, I became a member of the Royal Meteorological Society in 1935, and I thought I ought to send something there, and so I got this in and then I had this footnote by Brunt and I was, as I say, quite mad because I could not reply in the Journal any more, so I ...

P: Why didn't he publish this?

H: I don't know why that wasn't published, but at any rate, for years I never published anything in the Quarterly Journal.

P: Well, I would guess that he must have been the editor at that time and that he regarded this as the editor's prerogative to make these sly comments. But, in this paper, what you do is to apply, well what is essentially the vortex form of Margules' formula and to show that it leads to the same funnel structure as from your other studies.

H: Incidentally, since you mention Margules' formula, you know that was really derived first by somebody else.

P: No, I didn't know that.

H: There was some schoolteacher in Upper Silesia in the late 19th century who was interested in ocean currents and he derived Margules' formula, well not for the atmosphere but for the ocean, but it's the same thing. The reference to that is in the book by Neumann and Pierson on oceanography. I'm particularly interested in that because the schoolteacher is from the same former Prussian province as I am.

P: I see. Did Margules know that?

H: No, I'm sure Margules didn't know. Well, after all if he had known, he certainly would have quoted that man. It was in a very obscure publication. At that time the schools, sometimes -- the higher schools, gymnasiums and so -- would publish papers of their teachers. They were, if course, all people with degrees.

P: Bernhard, we've come to about the end of the discussion of this topic, which I think is the major one for us today, on vortices. Did you have any other general comments about that subject?

H: No.

P: Your last publication that's in that category is the one on the motion of binary tropical cyclones, in 1951. And you haven't returned to that since then?

H: No.

P: But certainly this work on the vertical structure of the hurricane must have given you a lot of pleasure.

H: As a matter of fact, I never was so much impressed by it until Herb Riehl ... Whenever he talked about hurricanes, for a long time -- I don't think he would do it now any more -- but for 10, 20 years after that came out, always started out with this paper.

P: Well, starting out with the basics is a good idea. Bernhard, let's go to topic F on turbulence and viscosity, and the first item there is paper number 28 which I have here somewhere.

H: This must be your copy.

P: That's my copy, yes. Actually it has Rossby's name on it, but ... I have expropriated it. "The daily temperature period for a linear variation of the austausch coefficient." This was published in 1936. You mention here that Taylor and Schmidt treated this problem of the vertical propagation of the daily temperature wave and that the results, you say, show that this assumption [of a uniform austausch (GWP)] is inconsistent with the observations. Do you recall what they did about it, if anything? Did they themselves try to carry it farther than that?

H: I don't think so. Certainly not Schmidt. Because Schmidt died fairly early ... Wilhelm Schmidt, I think he was only 45 or 40 years old.

P: Was he killed in the war?

H: No, no. He collapsed one day from a stroke ... not a stroke, a heart attack. For that matter, Exner too. Exner died when he was in his early 50's, 52 or so.

P: That was in 1931 or 32?

H: It must have been 1932.

P: And Schmidt before that?

H: I don't know. I think Schmidt died later ...

[Postscript 2-13 by B.H.: Wilhelm Schmidt of "Austausch" fame died in 1936 at the age of 53.]

P: Well, you are in fact able by assuming the eddy viscosity to increase, or to vary linearly with height, to solve the problem in terms of Bessel functions with complex arguments. And then you can determine the amplitude and phase of the wave as a function of height on those conditions and compare it with what you would get with a uniform viscosity.

H: Yes.

P: What you do not do here, and I wondered whether you did that at some later time, is to compare the results of that theory with observational data.

H: Well, I didn't have anything in there? I don't think I ever have done that. No, apparently there really isn't anything. I never have done that. I have sometime just done it in manuscript form, but I never bothered publishing it.

...

P: Is it possible to deduce by perhaps some least squares procedure what distribution of eddy viscosity would be most consistent with that Fickian equation?

H: Well, that probably would be possible. I don't know that anybody has ever done that. I think Sydney Chapman of all people, at one time took the equation and tried to determine from the equation, from observations, variable in altitude and time, to determine the coefficient of turbulent mass exchange and its variations.

[Postscript 2-14 by B.H.: I remember having used the results of paper number 28 to compute a height-variable mass-exchange coefficient on the basis of some data published in the QJRMS some years later. But I no longer have these calculations, only a graph showing the variation of the coefficient with altitude (which unfortunately depends upon whether the temperature amplitude or phase is used for the computation!).]

P: Was he looking at temperature, or something else?

H: I think, I'm sure he was looking at temperature. The only trouble was that the determinations, the values were very inaccurate and he

decided he didn't get anywhere. One of the troubles is, of course, you get a second derivative with respect to altitude, and that's even worse than the first derivative to determine ...

P: Yeah, if you try to do it that way. But I wonder now, how deep a layer would you need in order to cover the full regime of vertical variation of the eddy viscosity. You would need to go up, what, to several hundred meters, I suppose.

H: I suppose so, yes.

P: And the Eiffel Tower data of course are historically very important. They may not go high enough.

H: They are only 300 meters. Of course, as far as this paper is concerned, I think that's typical of the kind of problems which one attacked at that time. One always looked for problems which could be solved in "known" functions.

P: Well, now are you aware ... True, yes, and that of course, well, that was more of necessity than choice. There was little alternative.

H: Yes.

P: Are you aware of boundary layer practitioners having made use of this approach?

H: No, I don't ... didn't hear of anybody. Of course, Hilding Köhler has done quite a bit with variable ... quite a few integrations with variable austausch coefficients.

P: Who was that, Bernhard?

H: Hilding Köhler?

P: Oh. The cloud physicist.

H: Yes, I guess he was a cloud physicist.

P: Later on he was a cloud physicist. I did not have access to number 70 which is "Vertical distribution of temperature and humidity over the Caribbean Sea," jointly with Bunker, Malkus, and Stommel. I remember having seen that paper at the time and I probably have it at home.

H: Well, I have only a fairly small contribution in there. What I did try to do in there is explain, I think it was temperature and may-

be also humidity distributions by means of the, well shall we call it the Austausch theory?

P: I see. In the sea surface boundary layer.

H: Yes.

P: And number 95 I also did not locate which is frictional effects, oh, I beg your pardon, I do have that. "Frictional effects and the meridional circulation of the mesosphere." I have that right here. You pose the problem here at the beginning as that the winter temperatures in the upper mesosphere at high latitudes are so high, relatively speaking, that they cannot be easily explained, and since they cannot be explained on the basis of solar heating of that season, it must require some dynamical interpretation.

H: Yes.

P: Now, upper mesosphere. What elevations did you really have in mind there?

H: Well, about 80 kilometers.

P: And what are ... you don't show any typical temperature distribution. At least you don't show a diagram. But you have this table here. At 80 kilometers at White Sands, 200 Kelvin, at Fort Churchill, 250. This is wintertime. The legend doesn't say, but I assume that that's wintertime.

H: Yes, I'm sure. I imagine that would be wintertime.

[Postscript 2-15 by B.H.: Paper 95 is largely based on wind observations, and the temperatures at Churchill and White Sands are used merely to make some order-of-magnitude calculations. The temperatures quoted and used are winter temperatures. It would be useful to repeat such calculations with the data now available.]

P: So that is the problem. A 50 degree disparity.

H: Yes. Well, there was some question about if one could get any vertical motions -- if they could explain these things, as far I remember. Let's see, which year was that, around 1959?

P: This is 1961.

H: Oh yes, 1961. That was when I was at the Rand Corporation. I remember we just talked about that some day and I started talking

about the effects of friction -- viscosity -- in the atmosphere on the wind distribution, if the wind profile is curved like that or if it's curved the other way and well, it seemed then, if I remember correctly, as if there was some possibility that this curvature could induce some motions against the gradient which might produce in their turn some vertical descent and then one of the people at Rand had just constructed a diagram of wind observations, wind data, Batten, I think he's probably even mentioned in there, he gave me them and I made these calculations which led to these things anyway ... meridional circulations ... north and southward motions. It all is very vague in my mind now.

P: Well, at any rate, you found that a frictionally driven circulation based, as you say, on the curvature of the zonal velocity -- curvature in the vertical sense ...

H: Yes.

P: ... could account quantitatively for enough, I mean the circulation produced by that could account quantitatively for enough descent in polar regions to ...

H: ... account for the temperature dynamically. Well, of course, that was in 1961 and God knows what the wind-distribution quotations are these days.

P: Well, alright, these winds were based mainly on rocket soundings at that time, weren't they? Did you continue any work along these lines, upper atmospheric work?

H: No, not really, not along these lines ... of course, noctilucent clouds are upper air ...

P: I think time requires us to move to the papers for topic K, miscellaneous dynamic meteorology, and there are two papers I'd like to touch on briefly. One is number 59, which is ... The title of that is "Horizontal wind shear and the generation of vorticity." You mentioned before that you had been a visitor at the Institute of Tropical Meteorology in Puerto Rico, and this is work apparently that you got involved in at that time.

H: Yes, I don't really know anything about this any more.

P: Well, at any rate, what you consider there is the possibility that the connection between vorticity change and convergence and divergence, or vortex-tube stretching, or whatever you want to call it, depends not on, not strictly on the relative vorticity but on the ab-

solute vorticity, and in low latitudes where f is sufficiently small, the relative vorticity part of the basic flow can make a difference.

H: Yeah, that must have been something that came up in conversation with Herb Riehl, I guess, seeing from the reference.

P: The other paper in this series of miscellaneous dynamic meteorology that I have to refer to here is number 63. I did not locate number 60 "On the relation between the wind field and pressure changes."

H: Didn't I give you a copy of this? I don't know what's in there now, but ...

P: No, I don't think we could find that for some reason. Of course, I could undoubtedly find that in the library. It was not here. At any rate, do you recall what that was about, Bernhard? "On the relation ..." — number 60.

H: Yes, I know what you mean. No, I can't really say I do.

[Postscript 2-16 by B.H.: Paper 60 deals with the so-called "isallobaric wind" which enjoyed some popularity in the thirties thanks to work by Brunt and Douglas (although it was already derived by Hesselberg while in Leipzig considerably earlier.) This deviation from the geostrophic wind is supposed to be perpendicular to the isallobars. However, Möller and Sieber found in a statistical investigation of pilot balloon ascents that the isallobaric wind appears to be parallel to the isallobars. Ertel subsequently gave a theory which explained the results by Möller and Sieber. However, their results were in turn questioned by Berson because the use of pilot balloon data implies a preponderance of anticyclonic cases.

A statistical check at MIT where I was at that time was inconclusive because of the difficulty involved in obtaining reliable values of the geostrophic deviation (which is supposedly due to the isallobaric effect). Therefore, an attempt was made to compare, on the basis of synoptic data, the terms neglected in the derivation by Brunt and Douglas with those retained. The result is that both sets of terms are very much of the same order of magnitude so that the isallobaric wind concept is not a good correction to the geostrophic wind. This conclusion also implies that the isallobaric wind does not offer a satisfactory explanation of the distribution of regions of convergence and divergence in moving cyclones. But such an explanation can be given on the basis of the distribution of the acceleration of motion.

The paper considers only the isallobaric wind according to Brunt and Douglas. If there is still an interest in the isallobaric wind according to Ertel, it would be useful to make a similar study of orders of magnitude of relevant terms for that case.]

P: Okay, we'll leave that for another time and proceed to the sea breeze paper, number 63, "Comments on the sea breeze circulation." I found that quite intriguing as is true of so many of your papers. You come right down to the brass tacks of fundamentals of what really makes things go. And one intriguing starting point here is your discussion of what you call the equilibrium sea breeze. I had never heard that before ...

H: I hadn't either ... I thought that ...

[Postscript 2-17 by B.H.: The paper 63 has received two comments in JAS. The first comment, by N. R. Beers, in the same vol. 4, p. 74, points out that my statement in the footnote on p. 6 is not right, inasmuch as the periodic term at latitude 30° would not be infinite, but would merely increase linearly to infinity. This is true, but really irrelevant for the discussion in the paper (although I should not have been so sloppy!) The second comment is by G. E. Forsythe, in a paper of his in vol. 6 of JAS, p. 337. My reply is found in vol. 7, p. 164.]

P: It's a very good way to establish a contrast between what really happens and what would happen if you didn't take account of all the important dynamics. Did you pursue the sea breeze problem? I have the feeling that there was ... wasn't there a series of technical reports on that?

H: There was one technical report which I wrote on it later.

P: And what ... did that carry this discussion a little bit farther?

H: Yes, but I don't really remember now what's in there anyway.

[Postscript 2-18 by B.H.: This scientific report, entitled "A Linear Sea Breeze Model," was undertaken as part of a project sponsored by the U.S. Army Signal Corps R&D Laboratory and was intended to be conducted parallel with a numerical calculation of a nonlinear sea-breeze model. In contrast to the simple consideration of paper 63, changes in the horizontal and vertical directions are possible now, but it is assumed that no changes occur in the y-direction (that is parallel to the coastline). However, the wind component in the y-direction is not assumed zero since the role of the Coriolis force is to be taken into account. Formal solutions are given. But

numerical examples are given only for the case of no earth's notation when the analytical expressions for the computation of numerical examples are considerably simplified. The numerical examples resemble the results of Jeffreys (QJRM 48, 1922) who also neglected the effect of the Coriolis force.]

P: In this paper, as you point out at the end, you limit yourself to discussing the time dependence of the circulation without any reference to the spatial structure ...

H: No, the later paper does deal with spatial connections, but I never published that. I think it may actually have been because that was just before I left New York University and came to Colorado. I didn't bother.

P: Yeah. I suppose one might say that realistic treatment of the sea breeze nowadays requires computational models.

H: Yes.

P: But, neverthless, it's remarkable how much can be learned by such very simple things as what you did here.

H: The whole thing originated as a matter of fact because toward the end of the war the weather station at MIT -- you had a weather station at Chicago too, yes, in fact, we talked about it yesterday briefly -- but the weather station at MIT and its officer really didn't have anything to do, any prescribed duties anymore, so he decided to study the sea breeze in Boston and I still remember he once came to me and said, "Now one thing which we have definitely established is that the sea breeze intensity has nothing to do with the temperature contrast between water and land." He was joking of course. They couldn't find it in their data. He didn't really mean it.

P: That's a case where the data must be wrong and the theory correct.

H: Yes.

P: But have you ever been tempted to return to that problem? The sea breeze problem has always been an intriguing one to me.

H: Well, I don't think ... I have been probably tempted but I certainly never did and I don't really think I am very enamored by using a computer.

P: Bernhard, is there any general comment to make ... that you would like to make about the various topics today?

H: No, I mean, all I can say is that all these things -- tropical hurricanes, tropical cyclones as well as these things, sea breeze, etc. -- have really progressed quite a bit more in the last 30, 40 years. I feel pretty much out of it now.

P: ... one thing that I did want to bring out yesterday is ... we were talking about the pressure and temperature variations, vertical distribution of pressure and temperature variations, and is it correct that the work done by Penner later on was an outgrowth of those studies of yours?

H: Yes, especially since I was his thesis advisor. Well, he was a good man, I don't know what happened to him finally, but ...

P: Well, hm, didn't he remain in the Canadian met service?

H: Oh, yeah, he was with the Canadian Meteorological Service. I almost suspect by now he must be retired too.

P: Do you know whether he continued to work along those lines?

H: I don't think so. ...

P: Yesterday we were talking about Kibel and Kibel's forecasting system. Do I remember correctly that Kibel developed a what shall I say, a Rossby wave formula at a much earlier time than what we're talking about? I mean, Kibel's forecasting theory was a wartime development, wasn't it ... ?

H: Yes.

P: But did he not claim or had it not been ... oh no, not Kibel ...

H: Blinova.

P: That's Blinova, sorry.

H: Yes. Well, Blinova got the same formula as I did.

P: That's it.

H: About two or three years later, I think. Incidentally, I don't know when she got it, of course. I don't know if she was in Leningrad or in Moscow, but at any rate during the war she probably had less calm and quiet to work than I had in Toronto. So she may very well have gotten it at the same time and then she really developed the thing much ... the whole idea of what we call Rossby waves today -- she developed much farther than I did at that time.

P: Was she his wife?

H: They were married, but I don't know when. I met her when she came here for some meeting, after the war. But well first of all she is very shy. Secondly, she doesn't speak any English ...

[Postscript 2-19 by B.H.: In connection with Blinova's visit to the U.S., "here" means "Boulder".]

P: Those two things might have been a serious impediment to communication!

H: Yes, and she might of course have said, "He is very shy and secondly, he doesn't speak any Russian." So we never got together.

P: Well, that subject matter area of course is one that we'll get into later on in considerable detail, but I'm glad to be straightened out on that now.

[End of side 2, tape 2]

TAPE 3
4 May 1983

Platzman: This is the third in a series of interviews of Bernhard Haurwitz. Today is Wednesday, May 4th. Bernhard, the first topic today [is] atmospheric wave motion, short and synoptic scales. That subtitle is intended to exclude planetary waves, which is a separate topic and we'll come to that in due course. Do you have any afterthoughts or comments about our previous interviews?

Haurwitz: No.

P: So, as we've done before, I propose to discuss the papers on this topic of atmospheric wave motion in chronological order. That may not be the most logical order, but it certainly is a convenient one.

H: That's fine with me.

P: Okay, then the first paper is number 8, which is one of your early papers after your doctoral dissertation published in 1930, I believe, on the calculation of oscillatory motion in air and water. This is in some ways a curious paper because it starts out ... the basis for it apparently was criticism that you had of some work of Exner's.

H: Yes, that's right.

P: And I noticed in a footnote about half way through that you comment that after this work had been done, Exner died, but apparently not as a result of this work, because you said that he knew about your criticism.

H: No, I didn't really say that. I said that he probably would have [concurred], if he had had the chance to see it. That is what the footnote in there implies. If he had had the chance to see it, he would no doubt have agreed with me, but I don't think he had ever seen it. As a matter of fact, that was at the time when I had also written a paper where I extended some work by Alfred Wegener on the wavelength of billow clouds.

P: Yes.

H: And by showing that by taking into account the compressibility of the atmosphere -- the stratification in the vertical -- you get better agreement with the observations than Wegener did. And Wegener just before this paper was published, died too. That was on his last

Greenland expedition. There was a third incident like that, but I really have forgotten that, right now. But that was kind of a touchy subject because Exner was of course the great man in dynamic meteorology, quite rightly so ...

P: Yes, but I think you're right though in saying that had he read your comments about his work, he would doubtless have agreed, because he was not so limited.

H: Oh, I think he would have agreed, and as a matter of fact, there was another man in Frankfurt by the name of Becker. I was just looking at that paper again. There is a footnote to it. And he had done some similar work -- I don't think it had ever been published yet. And so I had him in mind too when I wrote that criticism.

P: Well, the subject, for the benefit of our listeners, is ... and this is something that ... I mean, I don't want to be too conscious of the fact that there is a tape recorder going here, but on the other hand, it probably would help in terms of the transcript being more readable occasionally to mention the general subject that we're talking about, so the subject here is the use of particle mass-point analogies to atmospheric flow. That's a very interesting subject because there is a long tradition of trying to view certain problems of meteorology in that way. ... One can, as you have done here and as apparently Exner did, specify the pressure gradient as being not zero but some assigned function, and then you get a slightly more complicated problem but it's essentially of the same kind. But then, in a slightly different context, you have such things as the absolute vorticity trajectories.

H: Yea.

P: Now that's a, in a way, that's a particle dynamics viewpoint although I think one could probably say that it has some perhaps more dynamical substance to it.

H: Yea, I would say hydrodynamical substance.

P: I notice that you mentioned ... it's very interesting that you mentioned the what was then recently published paper by Courant, Friedrichs, and Lewy who, some of whom at least, were your colleagues ... not your colleagues, but for a time were I think colleagues in ...

H: At New York University.

P: No, I'm thinking of Göttingen.

H: Well, in Göttingen Courant was not my colleague, he was my teacher, I took courses with him, and Hans Lewy was a student and he was about one year ahead of me, except he was in mathematics, but, of course, at that time I was already thinking of myself as a geophysicist.

P: But now, in connection with that reference to the Courant, Friedrichs, Lewy paper, that is in this paragraph here.

H: Yes, I remember that. I really don't quite know why that came in there because it is now, it seems to me or it seemed to me when I looked at it a few days ago, it seemed a bit extraneous to the subject matter.

P: The sense in which I got it, but correct me please, is that you were saying that alternative to considering a Lagrangian mass point analysis, one could express the problem in terms of n-degrees of freedom in which you essentially, we would say today, discretize the Eulerian equations. Then you say you would be confronted with the problem of trying to show that in the limit as n goes to infinity, you have meaningful results. I think it's in that sense -- is that correct?

H: Yes, I think so.

...

P: ... Well this brings us to paper number 10. Now, I did not have available and did not just now take the time to look at paper number 9, which is also in this general group of papers, I didn't have a copy of that. For some reason, and I'll have to ask why that is, the NCAR Library does not have the Met Zeit [Meteorologische Zeitschrift]. Do you understand why they don't have any of it?

H: Well, it may simply not be available.

P: Well, I'm sure that's true. It's a defunct journal and probably hard to get, but it's rather inconvenient not to be able to ...

H: I don't know which paper ...

P: Well, I'm looking that up now, it's ...

H: Oh, I know.

P: Could you comment on that?

H: Well, it's really the same as the paper number 12.

P: Oh, fine, because we'll get to that in just a moment.

H: Paper number 9 is simply one ... well it looks like two pages here ... but there is what I mentioned before, this result about the better agreement between observed and computed wavelengths of billow clouds in the case of an isothermally stratified atmosphere, as a matter of fact, isothermally stratified and adiabatic ... changes of state, and what is printed in the Meteorologische Zeitschrift is merely the abstract, so to speak, of a paper I gave at ... I think it was Vienna, as a matter of fact.

P: So are you saying that number 12 is the full text of that ...

H: Yes it was really the full text.

P: Okay, fine. Then that simplifies matters. Papers 9 and 12, then must ... no, that's not exactly true: I was going to say they were your first works on billow clouds, but 10 also has toward the end at least, it also has some discussion of that. And that brings us to number 10, which, you have in your notes here, is your Habilitations thesis.

H: Well, it's a Latin word, of course, which means simply that it is a thesis which you submit in order to become a lecturer at a University, which at that time at least was an unpaid position. It really was nothing but to give you the permission to give courses at the University.

P: Is that practice still followed?

H: The practice is still followed, except today the people who become lecturers -- Privatdozent it's called in German -- also get a salary for teaching.

P: No, what I mean is, is the practice of submitting a special ...

H: Oh, yes. You have to submit a paper for that.

P: This is beyond your doctoral degree.

H: That is beyond the doctoral dissertation. The requirement is that in order to become a lecturer at a German university, the requirement is that first of all you have to have a doctor's degree; secondly you have to present evidence for three years after completing your doctor's degree that you have also done scientific work. In other words, you have to have some papers. And then you apply at a university for being admitted to lecture. Not the university ... you

don't apply to the university but to the faculty. I would, for instance, have applied to the philosophical faculty, in which geophysics was, and I would apply for becoming a lecturer in geophysics. ... Now the faculty then can say right away, no, we don't want you; we don't need anybody. They don't have in any way to prove that. The way in which it really goes in general is, of course, at that time there was in geophysics of course only one Ordinarius -- one full professor for the subject. If the professor sponsored you, so to speak, or had no strenuous objection, then you could go through that, then you would submit your thesis. The faculty would appoint three persons to pass on this thesis. In my case it was in geophysics, Weickmann, of course. Then there was a mathematician appointed, Lichtenstein, who was a hydrodynamicist and other things too, and the astronomer Bauschinger. I don't know if he is well known or so. After that is through you have an oral examination. I don't know how long you want to go on with that ...

P: I think that's ...

H: You have to have an oral examination by ... well, in theory, the whole faculty, that is, all the people in the, in this case the philosophical faculty, all those who are interested can come. First of all you present a paper, a brief paper, it shouldn't be longer than 20 minutes, and then they ask you questions. Now, I didn't realize that before, but I got good advice. I had planned to talk something on cosmic rays about which I knew next to nothing, but a geophysicist, a younger geophysicist, Bartels, said to me, "Well, you're crazy if you do that. You have people like Heisenberg probably sitting in there, or Debye. If they ask you anything about atomic physics, will you really know it?" He said, "For heaven's sakes, now what is the most specialized thing ... what does Weickmann do?" So I talked about symmetry points. That was excellent advice. Everybody was probably bored stiff. And after I was through Weickmann, who sponsored me, as I said, asked the collected dignitaries if they had any questions. Well and, Lichtenstein piped up with a question -- well, I won't mention the question. I still remember it. And I said, "Well, it has something to do with the seasonal temperature change, I'm sure, but nobody really knows for certain," and turned to Weickmann. Weickmann said, "Well, I don't know it either." And that was that. Weickmann then asked, "Herr Kollege Debye, do you have a question?" No, he didn't he said. Well, everything seemed very clear. So, then I was sent out for about 10 minutes, and — later I was at some such things myself when some other poor victim went through that, and they probably told jokes — and then they called me again and said, well, this was fine and I could now proceed to my ... well, it was really called Probevorlesung, that is a test lecture. The test lecture ... shall I go on with that?

P: Thank you, Bernhard, but I think we should proceed with this very substantial ... actually, it's a monograph of 106 pages. Really very substantial effort, published as a separate publication of the Geophysical Institute, Leipzig, and the general subject, the title is on the theory of wave motion in air and water, and the subject matter is the treatment of problems in which you have a stratified medium, either consisting of continuous stratification, or a superposition of layers, sometimes homogeneous, sometimes not. And with the added important feature of a basic flow present to one degree or other. And this is a very comprehensive treatment of problems of this kind, and in the course of that you bring up the problem of billow clouds, which is certainly one area to which that general subject is most naturally applied. And this is, I guess, your, what shall I say, your introduction, not your introduction, but your introducing this subject into your publications. And it's a theme that you carried on for quite a number of years, even down through the more recent years when you became involved in the study of noctilucent clouds.

H: Originally, of course, the preoccupation with wave motions and stratified fluids which have a basic current, came from the idea of the theory of the polar front and cyclone development as waves on the polar front, and most of what is in there, just as in Bjerknes' book, Physikalische Hydrodynamik, physical hydrodynamics, was really some gradual approach toward solving the cyclone problem.

P: A step in that process. Among the things that you considered there were first a fluid that is stratified, but incompressible, and at first with a zonal flow that does not depend upon height. Then, you went on to consider, and in that, the course of doing that, you considered first one layer, then two, then even three layers, in some cases bounded both below and above by rigid surfaces, in other cases bounded below by a rigid and above by a free surface. And finally you went on to consider the case of an isothermal, of a gas, rather than an incompressible medium, isothermally stratified. And you -- I don't know to what extent, I don't recall, yes, I think you did give some quantitative illustrations applying, in a preliminary way, applying that in a preliminary way, to billow clouds.

H: Yes. But only for the isothermal case. The reason for that is that in the isothermal case the differential equations have constant coefficients, which makes it a lot easier.

P: Well, as I said before, this seems to have been the starting point for a whole line of research, the traces of which can be seen for quite a number of years in your subsequent work. Do you recall what it was that got you interested in the subject of billow clouds?

H: No, I cannot say so particularly. It just seemed to be a subject to which some of these things in this particular paper we are talking about, to which they could be applied. Of course, I had looked in the sky and had seen billow clouds, and I probably, I am sure I have also read Wegener's ... in fact I know I have read about Wegener's work on billow clouds because I had read his Thermodynamics and he talks about the billow clouds and his calculations there. Also, he refers in that to a hydrodynamics book by a physicist by the name of Wien which I bought just because Wegener mentioned it. Though he doesn't really do anything with it, and I must admit I never did anything with Wien's book either.

P: Is that the Wien of Wien's Law?

H: No. I don't think it's that Wien.

[Postscript 3-1 by B.H.: What started me on my "Habilitationsschrift", number 10 was the Norwegian wave theory of cyclones as mentioned in the interview. The billow clouds were just an afterthought. Most likely I had been reading Wegener's paper in the Beiträge zur Physik der freien Atmosphäre and was struck by the fact that his "ideal inversion" (also mentioned in his "Thermodynamik") has the effect to increase the static stability, thus decreasing the wavelength. Since Wegener considers incompressible and homogeneous atmospheric layers, the static stability assumed is really too small. That can be corrected by considering more realistic atmospheric layers with vertically decreasing density and adiabatic changes of state, as my calculations show. (Wien is in fact the discoverer of Wien's displacement law.)]

P: Well, if we can go on to the next paper in this sequence here, which we have previously mentioned, on billow clouds, number 12, which is "On the wavelength of billow clouds," and dating back to that same period, 1931, this is a much more substantial effort directed specifically at the problem of billow clouds. And in this case the emphasis is on, the particular emphasis is on the effect of stratification in addition to the density discontinuity, on influencing the wave length calculated for billow clouds.

H: I say, is that the paper where I have already ... excuse me ... where I have already different lapse rates?

P: No. That's the next one. And in the next paper, published in 1932, on the wave motion on an interface between two air layers with linearly varying temperature, in other words uniform lapse rates on each side. And this is an extension in other words of the previous treatment of billow clouds.

H: Yes. Well, there I realized for the first time why -- I should of course realized it earlier -- that you can really assume that some of the coefficients which are variable can be considered as constant for the small extent which the layers in the atmosphere have. And I also at that time, I think I briefly indicate that in there, I really had a method where I could even estimate the error. That is, I had really a series development for, to allow for that, which made the thing at least for me intellectually much more honest, if you want to put it that way.

P: Yes. This is something like the variation of parameters. You mean this expansion here?

H: Yes, that's right. I read about it in this book by Schlesinger, a book in differential equations quoted in there. As a matter of fact, this paper was a paper for a Bjerknes Festschrift, for his 70th birthday.

P: In 1932.

H: In 1932, yes.

P: The billow cloud subject is continued, then, in paper number 24.

H: Yes, of course it would have fitted very well already in the paper for the Bjerknes Festschrift, except I was working with a deadline then and I don't really like to ...

[Postscript 3-2 by B.H.: Here I am discussing paper 14, while G.P. is actually referring to paper 24. The method used in paper 14 to treat the wavelengths of billow clouds is really developed in paper 15 (a contribution to V. Bjerknes' Jubilee volume for his 70th birthday).]

P: What would have fitted it into that?

H: Well, making a different, additional chapter in this paper, applied to the billow clouds.

[Postscript 3-3 by B.H.: The paper dealing with the wave motion at an interface between two layers with different lapse rates is number 15, its application to the wavelength of billow clouds is given in paper 14 which really postdates paper 15 as far as the research on it is concerned.]

P: Oh, oh, I see.

H: So I wrote another paper. In fact, I mostly rather preferred writing fairly short papers. My theory was at that time -- it is still -- you have a much better chance that people will read a short paper than a longer one. A longer one they will say, well that's very interesting, I must read it, and put it aside and two years later push the whole pile off the desk.

...

P: The next paper in this sequence, 1935, is on the application of billow clouds -- this is 24 -- on the application of billow clouds to some microbarogram observations that were made at Blue Hill Observatory that indicated fluctuations with a period of about 16 minutes. And this is an interesting case, I thought, because as you point out, ordinarily one does not have any means of detecting billow clouds at the ground because they usually occur in elevations where by the time the wave reaches the ground, it's more or less attenuated.

H: Yes.

P. But in this case, the inversion or temperature jump apparently was at a very low elevation, something like -- what was that -- it was a dome of cold air, and you estimated the elevation to be, dear me, I thought I wrote that down, but I didn't. And I don't find it here quickly, but it was at a very low elevation, something of the order of what? A kilometer or less perhaps.

H: It probably was.

P: A few hundred meters. Probably a few hundred meters. It couldn't be more than that because these are short waves and they would essentially disappear after one wavelength.

H: Yes.

[Postscript 3-4 by B.H.: The inversion measured by airplane at Boston was at 75 m height, considerably below the top of Blue Hill at 195 m, but as conjectured in paper 24 the inversion may have been higher over Blue Hill.]

P: The next paper, Bernhard, in the list on this subject of atmospheric wave motions changes the topic here quite abruptly. You wrote a review discussion of the Norwegian wave theory of cyclones, published in the ... Did you want to say something more about this?

H: No.

P: ... published in the Bulletin, and this is something that I found really quite fascinating.

H: I'm glad to hear that. I remember vaguely this paper came about because it was felt that there should be some American publication on the Norwegian theory of cyclones, on air-mass analysis and all that, and Namias and various other people wrote that, and this was really meant as a part or in addition to that series.

P: That's very interesting because I noticed when I was taking this paper out of the journal, copying it, that the very next paper is one on isentropic analysis by Rossby and collaborators.

H: Yes.

P: So you're saying that this was a companion to these other ... ?

H: Yes. It was certainly meant as that. It was also meant to be quite popular, by popular I mean something which the average meteorologist ... weather forecaster ...

P: Written for the layman.

H: Yes, the layman in dynamic meteorology.

P: Well, there are quite a few things I would like to ask you about. This was 1937.

H: Yes, it probably was written about 1936.

P: Yes. Now, Bernhard I did not look up, and I wish I had, but perhaps you remember that in Physikalische Hydrodynamik, there of course is a discussion of cyclone theory and an attempt there to summarize the state of the theory as it existed at that time, but there was also or rather, I ask you, was there also an attempt to give an overview, a verbal overview of the subject that is similar to what you have done here ... ?

[Postscript 3-5 by B.H.: The paper discussed now is number 37. At the end of Chap. 12 of "Physikalische Hydrodynamik" there is a mainly qualitative discussion of the cyclone problem. It refers partly to the preceding mathematical chapters, and outlines the following chapters containing mathematical models which more closely approach the cyclone problem than what proceeds it. But because of its frequent reference to earlier mathematical results it is hardly a simple introduction to polar front theory.]

H: No. I have it here, by the way, Physikalische Hydrodynamik.

P: I don't think so. So the point I'm getting at is, isn't this then the very first time that anyone tried really to explain in plain clear language what was really going on?

H: Well, this probably is true for the American continent, but I would hesitate to say that it's really true for Europe, say Germany or any other of the European countries because people might have written something in well, let's say, some meteorologically obscure journals -- journals like Umschau, of course that didn't exist at that time, but similar things existed. So something like that may have existed.

P: Well, now this was a time when I think Solberg's work on this subject, had essentially come to an end.

[Postscript 3-6 by B.H.: Solberg indicated in his 1928 publication (Geofysiske Publ. vol. 5, number 9), where he reported on his integration of the atmospheric perturbation equations, his plans for a second part. But no such sequel appeared, as far as I know.]

H: Yes.

P: He had done, by this time he had done everything that he was going to do on the polar front problem, as far as I know.

H: Yes. I think that's true. I mean he then just decided, well he never said in so many words, but he decided he didn't get any farther.

P: Right. And although the polar front theory had of course been almost universally adopted by the time that you wrote this article in the middle of the 1930s, and therefore in that sense, had been triumphant in its success in displacing ... Yes, go ahead.

H: I said yes, that it has been universally adopted but now considering that I was in Canada at the time, the situation there was that the official Canadian forecasters didn't make any use of air mass analysis and such things yet. There were people, the younger people in the Canadian meteorological service did use it for their work, but it just went into it very gradually ...

P: I see. Was that an exception, do you think?

H: I don't really think so. I think it probably ...

P: From the standpoint of practical forecasting technique ...

H: Yes, well at that time of course I have no idea how it really was in Germany, since the Nazis were already in power, it might very well

have been that it had been generally adopted, air-mass analysis and frontal analysis had been generally adopted by the German meteorological service, but when I was still in Germany in 1932 there were places — it was in for instance the Prussian meteorological service -- some of the observatories of the forecasting centers would use polar front theory, others were not.

P: Interesting. Well, the point that I was going to make is that, to whatever extent the Norwegian polar front theory, or model, perhaps I should say model, was accepted, one can hardly say that there was a similar success in the theory of that.

H: Yes, that's quite true. That is very rarely put that way.

P: So you have a situation where the underlying plausability of the proposal ... people were, many people at least, were quite willing to accept. But there was no corresponding conviction that could be derived from a relatively complete theory of ...

H: No, there was nothing there.

P: And what you attempted to do here is to get at the essence of what that theory might be, and to discuss it without the use of mathematics. You first introduce the essential physical processes involved -- those connected with stratification, the gravitational stability associated with that, the instability associated with shear, and the stabilizing effect of the earth's rotation. And you then tried to bring all these things together to show in what circumstances one or the other or in combination, these were operating to produce what we observed. There's ... it struck me in reading this, Bernhard, that you point out, that the effect of the earth's rotation is to tilt the orbital planes into the horizontal as the wavelength increases, and that this weakens the ability of the gravitational stability to exert itself and therefore opens the door, so to speak, for the shearing instability.

H: Yes. But of course there is a parenthesis missing in there, I hope.

[Postscript 3-7 by B.H.: It should be stated explicitly that the effect of reducing gravitational stability because of the tilt of the orbital plane by the Coriolis force has been pointed out earlier by others.]

P: Oh. What is that?

H: Well, that is what should be added there, I think. I don't think it has ever been proved that actually as the tilt occurs the wave will become unstable again.

P: Well, the point that struck me, was that kind of argument is exactly the kind of argument that can be used to describe the baroclinic instability. The fundamental difference is that in the one case, in the polar front case, the argument is — and your description of it corresponds to that -- is that the energy source for the instability is from the shearing motion itself. Whereas in the baroclinic instability problem, the energy source is the gravitational energy of the mass distribution. But the arguments that you gave here for the polar front theory could so easily be converted into a similar discussion for the baroclinic instability problem.

H: Yes.

P: Now this leads to a number of ...

H: Yes, well of course, there is something where the baroclinic theory of course has a great advantage over the Norwegian polar front, mathematical polar front theory: it doesn't have to deal with these horrible cold layers in the form of a wedge lying on the earth, which Solberg tried ... well, Solberg tried to integrate the motion of such things and never succeeded. Probably today one could but nobody ...

P: Well, this is an extremely difficult problem, and it's not surprising that he had the trouble that he had with it.

H: No, I don't fault him for it, certainly.

P: I think the first person, who as far as I know, who had any success at all with it, and that was only limited, was the Russian ...

H: Kotchin?

P: Kotchin, yes. But then in more recent times, there is the work by the Danish Eliasen who I think made the first significant advance after Kotchin.

H: I see.

P: But, it struck me for the first time in reading your paper here Bernhard, as you point out, Solberg was unable to get anywhere with the problem when the front intersects the ground. So he had this frontal serface in a state of suspended animation, but you know, that makes it look very much like the model for baroclinic instability, just as you said a moment ago ...

H: Yes, I know ...

P: ... and one wonders, he had everything. As far as the physics of the problem were concerned, so far as I know, he had everything in there.

H: Yes.

P: Why didn't he get the baroclinic instability out of it?

H: Well maybe just because he wasn't looking for it.

P: You don't need a continuous stratification to do that.

H: No.

P: It's puzzling. He must have either overlooked something or else made some restrictive assumption that somehow prevented him from getting that result. But it is somewhat paradoxical that, where he was trying to solve the problem of -- I mean the frontal problem — he actually had a model that was more nearly like the model for baroclinic instability. Getting back to the frontal problem itself, and the role that the shearing instability ... and here, this is really in the form of a question rather than a comment ... do we today know enough about the polar front instability to be able to say that ... what the energy source really is? Is it primarily the energy of the basic flow or is it only partly that, and partly gravitational?

H: Well, I have the impression that today nobody is concerned about the source of energy or the ... for the cyclone as originating from a polar front. The idea today is that the polar front is just incidental to the ... it's formation is just incidental to the cyclone development, and the cyclone development is not due to the polar front anymore.

[Postscript 3-8 by B.H.: There is, of course, also the energy becoming available as the lighter warm sector air is lifted above the colder and heavier air in the occlusion process, as was discussed much earlier by Margules.]

P: No.

H: Of course, Rossby really started with these ideas already in the 1930s when I was here, and then of course Charney developed it quite a bit further too.

P: Well, I found this very thought provoking, and we could go on discussing this but ... oh dear me, this is probably going to stop, but I think I'll just continue until it does. The next paper, number 53 is called "The Effect of a gradual wind change on the stability of waves," and this relates more to the polar front problem again, and you consider there what the consequences are if the transition, I mean if the front, is not really a strict discontinuity but a transition zone. And you take ... to illustrate the point, you take one of Rayleigh's ... what do your call those -- not step profiles ...

H: Jet?

P: No. I'm thinking of, I forget the term ... anyway, a profile that's built by taking linearly varying velocities -- uniform shears or uniform vorticity layers, and this particular profile is one that mimics a flexed velocity profile.

H: As a matter of fact, you know this whole thing as I found out later, had already been discussed by Rayleigh in the Theory of Sound. The only real addition which I have is that I have in my equations the Coriolis force, and that doesn't have any effect.

[Postscript 3-9 by B.H.: The problem of a transition layer and its effect on wave stability has also been discussed by Chandrasekhar.]

...

[End of side 1, tape 3, beginning of side 2]

P: Now, you were about to make a comment.

H: One of the things which, I believe, I mentioned in this paper also is that if the wave is sufficiently long compared to the layer of transition between the two main layers, then the wave really will not be noticeably affected by the "shearing instability" which in a way is very unfavorable for the wave theory of cyclones.

[Postscript 3-10 by B.H.: This statement about the instability is merely a faulty recollection of paper 53. If the wavelength is more than about five times the width of the transition layer, shearing instability will occur.]

H (continued): I don't know if that is in here, if I said that in here in so many words ... No, I don't quite put it that way; it says here ... "In the case of cyclone waves, the stabilizing effect of the stratification decreases with increasing wavelength, because with increasing wavelength the wave motion is tilted more toward horizontal

position. Therefore, the lower limit for the wavelength of the unstable cyclone waves should be larger the more diffuse the frontal zone."

[Postscript 3-11 by B.H.: What I am reading here from paper 53 refers to the tilting effect of the Coriolis force.]

H (continued): Now, apparently Rossby was at this meeting, or I must have presented the same results somewhere else, because he pointed out afterwards to me that this really ... just what I said before, he pointed out to me that this really is quite unfavorable for the Norwegian wave theory of cyclones.

P: Yes. And I think you also discussed the long wavelength limit of the instability. Don't you show -- don't you reply -- this is that that gets shorter -- or is that wrong?

H: I don't know.

[Postscript 3-12 by B.H.: In the case of sufficiently long waves the effect of the transitional zone is to reduce the instability, i.e., the exponential amplitude growth is slowed down.]

P: Anyway, overall the effect is to narrow the range of instability.

H: And of course as a matter of fact there is also one region of maximum instability. Which had already been discussed by Lord Rayleigh.

P: Rossby I suppose was glad to grasp at any straw that might have been unfavorable to the polar front theory.

H: Yes.

P: Did you have any other comments about that?

H: No.

P: Paper number 64 is the next in this sequence, and that is on "Internal waves in the atmosphere and convection patterns." Here you are in a way back to the subject of billow clouds, but now the question is to what extent the observation of convection patterns in the atmosphere, cloud patterns, can be interpreted in terms of billow clouds on the one hand and convection rolls on the other. And as I understand, the upshot of what you have done here is that when one considers the wind, the basic wind to have different directions in the two waves, then you can get very complex interface phenomena that can

resemble cellular convection and even polygonal type convection. But the one instance in which the interface theory, the internal wave theory, or the billow cloud type theory, is not able to cope, is when the rolls are aligned parallel to the shear.

H: Yes, well, that is one thing. The other thing which really I am concerned about there was that it seems to be sort of an accident which way the wave crest of the billow cloud will go, compared to the direction of the wind shear. And, well I just showed that actually for the computation of the wavelength of billow clouds, the best position is, so to speak, when the billow cloud is perpendicular to the wind shear. And in the vicinity of that, there isn't ... that is, if the wave makes a bit more or a bit less of an angle than an angle of 90 degrees, then the wavelength will not change materially, substantially.

P: But you do point out, I think, that the observations seem to favor a direction that is more or less at right angles to the direction of the shear.

H: Yes. But partly that case, the whole thing, came about because Wegener made of course the same assumption that the billow clouds drift with the mean wind, that is the sum of the wind in the upper and lower layer divided by 2. And he said that is -- now I have to stop and think how to translate that into English -- it is obvious by inspection, by intuition or so, that's as close as I can come to "a postulate" of the Anschauung; that's the same Anschauung as in Weltanschauung, of course. But in this case it means something else. And I tried to show a bit better why this particular reason -- by this particular position of the billow, namely 90 degrees to the wind shear, why that would appear so often.

[Postscript 3-13 by B.H.: Wegener's "obvious" assumption that the wave system of the billow clouds moves with the speed of the mean wind means that this mean wind is the wave velocity. It is this assumption that enables the wave length to be computed. Otherwise the theory would only give a relation between wavelength and wave speed. It is not entirely obvious that the wave speed should coincide with the mean wind speed, but paper 64 should make it more plausible. Also, the fact that the wavelength given by the theory agrees reasonably well with the observations is a further fact strengthening the theory.]

P: Well, was Wegener's analysis, well, you say he said it was obvious that it was the arithmatic mean -- was he talking vectorially or scalarly?

H: I think he was actually talking scalarly, but he probably just didn't think about the winds in the two layers having different directions, except either the same direction or 180 degrees different.

P: Isn't it true that convection rolls do in fact, or can and do in fact align themselves with the shear?

H: I don't know. They may very well.

P: So, uh. But I think the main point of your paper, correct me if I am wrong, was to show that if the billow cloud theory is generalized, sufficiently, by taking account of the different winds, wind directions, particularly, then you can get phenomena that make the cloud patterns resemble those that are due to convection cells.

H: Yes, that certainly is what I wanted to do.

P: It's sometimes hard to separate what you ... Again, on the billow cloud subject is paper number 72 where in collaboration with Emmons and Spilhaus you call attention to some data obtained when constant level balloons were beginning to come into use, and these are observations in May and July of 1948 that indicate that fluctuations in periods, 5 to 10 minute fluctuations. Did I say billow clouds? I shouldn't have said that. These are in fact simply pure buoyancy oscillations. And you were able to interpret the results in the light of relatively straightforward buoyancy theory. Modified, perhaps, to some extent as Solberg apparently did by -- or was it Godske -- Solberg, by taking account of the lateral extent. Coming down practically to modern times, there is paper number 123 published in 1973, you remember that.

H: Well, that was something which I really did just quite incidentally. That's about what really seemed to be standing oscillations in a lake of cold air in Fairbanks, near Fairbanks, Alaska.

P: Was this occasioned by one of your visits to..

H: Yes, well when I was there at one time, they told me about these temperature observations which they had on one or two of the slopes which surrounds the Tanana Valley. The Tanana is one of the rivers there and, well, these temperature oscillations ... these temperatures were oscillating, and it seemed a good idea to try whether it would come out as a seiche; I don't know how you pronounce it ... and it seemed to come out really well. As a matter of fact I did really much more than that, because I worked the whole thing out for a compressible fluid, but then I also worked it out ... well, in fact I had

I think originally worked it out like it is stated there, just for a compressible fluid, to see whether the order of magnitude at least would come out alright and it did, and then I found that for a compressible fluid it comes out practically the same for these dimensions. So for this particular paper then -- which incidentally is also for a jubilee volume, for Andrew Thomson, the former director of the Canadian meteorological service -- that seemed enough. And the other part of the manuscript is really lost anyway.

P: Lost in what sense?

H: Well, I don't know where it is.

P: Oh, I see. Okay. Actually, your model is one in which the basin is open on one end.

H: Yes. Well, that is the way it is. The hills, for one thing, aren't very high. The highest one, Ester Dome, is about a bit over 2,000 feet and Fairbanks itself, the flood plain, is about 600 feet and so the hills are only about a thousand feet higher, but on one side, let's see; it's to the south, it's open.

P: This is the kind of situation that I think occasionally occurs in the Black Hills area, in some part I don't remember exactly where, possibly near Fargo -- Fargo? or Rapid City, that would be near the Black Hills -- where the configuration of the topography lends itself to this. ...

H: Well, of course, occasionally, and not so rarely, one can actually observe such oscillations at least on the campus of the university in Fairbanks, because the campus is a bit higher than the environment, about 300 feet higher. So in winter when it's very cold the ice fog, which they have in Fairbanks when the temperature is say lower than -25° or -30°F, the ice fog will often be below the campus, that is -- and especially in the Geophysical Institute which is the highest building on the campus -- so you can see how the fog, above which you are, how the fog oscillates back and forth. Of course, in general there aren't any quantitative observations of temperature contrasts or wind contrasts so you can't really make a calculation.

P: That's really curious. Bernhard, the last paper in the sequence is number 124 which you co-authored with Elmar Reiter in 1974, "Internal gravity waves in the atmosphere."

H: Actually, I must confess that I really did very little with that. Elmar really just asked me to go through his calculations and I found a mistake which really didn't make too much difference anyway, but it was a mistake in arithmetic.

P: Well, this has to do with the vertical propagation of internal gravity waves, and it has a bearing on whether gravity wave energy can penetrate into the upper stratosphere and mesosphere. But perhaps in view of what you've just said, and in view of the rapidly diminishing time, should we proceed to the other ...

H: Yes. ...

P: This brings us to another phase of your research, primarily dealing with noctilucent clouds, although there are other topics under this general heading of upper atmosphere matters, not including tides. The first paper in this sequence is on ozone. You commented upon this a few minutes ago, "Atmospheric ozone as a constituent of the atmosphere." This was in the AMS Bulletin in 1938. And, it's a kind of a summary of the state of knowledge of the problem at that time. And dealing specifically with what ... the correlations that were then known between synoptic features as well as specific quantitative things like pressure and temperature on one hand, and ozone. I presume throughout this the quantitative data refer to total ozone.

H: Oh yes, at that time.

P: Well, would you care to comment on this?

H: I even forgot what the occasion of the meeting was. As I say, there was a meeting. The meeting was in fact in Canada, probably in Ottawa, but ...

P: It was at the Ozone Symposium during the Ottawa meeting of the AAAS in 1938.

H: Oh. Well, I must have been simply invited to speak there on ozone, so I said yes, Ottawa is a nice city to visit.

P: Did you ever pursue your interest in ozone further than that?

H: I cannot say I really did. I believe I at one time or twice I wrote some reports on ozone, but I don't remember ever having written any paper which I wanted to publish.

P: I noticed that you refer to the work of Wulf and Deming. Did you ever know Oliver Wulf personally?

H: Oh, yes. Do you know him?

P: Oh, yes, because he was in Chicago for a great many years. A very fine gentlemen.

H: Yes. Apparently he is still ... I don't know if he's going strong. The last time I saw him was in 1961. I know that so well because it was the year in which I was married and about two weeks after my marriage I left Boulder and went to the Rand Corporation for three months. Well, Harry Vestine, a friend of mine, was also a good friend of Oliver Wulf's, so we went to visit Oliver Wulf at Cal Tech. I don't know, he's apparently still alive.

P: When was that? So you last saw him in 1961? I communicated with him fairly recently, and I'll tell you about that later; I won't use up valuable tape.

H: Well, let's just forget about the tapes!

P: In 1941, you wrote a review paper for the Journal of the Aeronautical Sciences on the propagation of sound.

H: Propagation of sound. I don't remember any more in detail how that came about, but at that time there was some discussion about spotting of aircraft and spotting of, I think, also artillery locations and such things. I think mainly though the spotting of aircraft. And of course we did not know, or at least I didn't know, or the people in the department at MIT didn't know anything about radar yet, among other things, probably because it hadn't been invented really. So I believe Henry Houghton suggested to me, it might — or maybe Sverre -- Sverre Petterssen probably, he was the department chairman, suggested to me I should write an article on that and point out the possibilities of locating something by sound. In fact, I think it would be quite unfeasible to have done anything that way, by means of sound, with the high-speed aircraft which they soon developed.

P: Well, do I correctly infer that you were not actually involved in a project?

H: Oh, no.

P: There is one aspect of this that intrigued me very much. This was 1941 and you refer here to the anomalous propagation that had been discovered much earlier that was interpreted ... one explanation of which was the existence of high temperatures in the stratosphere. Of course we now know that there are such high temperatures at the stratopause. But the interesting point is that, and here you quote estimates that were made by three different people — first by Whipple, then also by Gutenberg, and then by Duckert. At 50 km their respectives values were 336 Kelvin, 344, and 349. Now this is very interesting historically. We now know that these are much too high.

But it's interesting historically because it's precisely this kind of information that misled Pekeris, you may recall, in his search for the elusive 8-km equivalent depth eigenmode.

H: Yes. Now, there is perhaps something more perhaps I should mention, that you know, my first lecture after I became lecturer in Leipzig, my first course, which was a rather short course, one hour a week and only for an incomplete term of only two months instead of three, was on the propagation, anomalous propagation of sound through the atmosphere. I was simply interested in this at the time. So, in fact, now that I am thinking of that, it may in fact be that Sverre -- Sverre Petterssen, that is -- knew about that and so suggested I might write an article on this topic for the Journal. And in addition to that of course I had given also later when I was in Toronto first, in 1936 or 37, I had given a course or a series of ten lectures in the physics department on the upper atmosphere where I touched on that subject too.

P: This was after you wrote that monograph isn't it? The monograph was in the mid-30s, wasn't it? 36 or something?

H: Yes, 36 or 37.

P: Do you talk about sound in that monograph?

H: Oh yes. Now you have it the other way around. I wrote ... the monograph was nearly written ... it was just really the lectures, you see. What happened, I gave these lectures and among my more distinguished visitors in that was the director of the David Dunlap Observatory, that's the astronomical observatory at Harvard, Professor Chant who was also the editor of the Journal of the Royal Astronomical Society of Canada, and he said he would like to publish these lectures in a somewhat shortened form, of course. That's how the monograph then came about.

[Postscript 3-14 by B.H.: Correction: Prof. Chant was the director of the David Dunlap Observatory at Toronto, <u>not</u> at Harvard.]

P: Okay. But now, getting back to these high stratopause temperatures. What I do not understand, Bernhard, is what went wrong with the sound, I mean with the inference of those temperatures from anomalous propagation. There must be something in the theory that is not giving us the right answers, but what is it? This is something I never did understand. Was it winds?

H: Well, partly it's winds, I think, but in addition to that, I really don't know any more now what kind of assumption these people

had made. It's simply that the sound apparently is reflected, or refracted, whatever you want from higher up where in fact the temperature does get warmer.

P: Okay, yes. I think I want to look up what the current views on that subject are. Now, we come to noctilucent clouds. Beginning with paper number 93, published in 1961, in Space Science Reviews? ... Planetary and Space Science. And there you examine the hypotheses that were then available to explain the incidence of noctilucent clouds, and the most commonly held, which you did not agree with, was that it was due to the special angle of view. This, in other words, this was essentially an optical explanation. You believed that it was more probable that you had accumulations and depletions of dust in systematic patterns.

[Postscript 3-15 by B.H.: G.P. does obviously not mean the incidence of noctilucent clouds, but the appearance of wave forms in them which constituted the subject of my paper 93. (Did I nod during part of the interview?)]

H: Yes, well, that was because I just like billow clouds, I would say.

P: Yes, well alright, but fair enough, why not? And you showed by very plausible order of magnitude type of argument that the convergence and divergence patterns associated with billow clouds would in fact be capable of causing a sufficient amount of condensation, I mean of ...

H: ... sufficient density. Well, I might just say, incidentally, that the first time that I thought about the possibility of billow clouds in noctilucent clouds, was much earlier. I had been in Norway, you know, in 1929; I had also met Störmer there. He let me come a few times when he made height measurements of aurora which was quite interesting. In addition to that of course Störmer was one of the first people to measure the noctilucent clouds. And after I was back in Germany -- Störmer must evidently have known that I was working on billow clouds -- he wrote me that he had observed billow clouds in noctilucent clouds. I forgot what wavelength he gave, but I think it was something like 9 km, which sounded quite reasonable, incidentally, and he wrote, what can you conclude from that? Well the answer to that of course is really nothing. I think that is typical for Störmer; he wrote me a postcard and all that information was on the postcard. Unfortunately, like everything else, I didn't keep the postcard. But at any rate, then, I don't know just what prompted me at that time to write about it. I probably had read some papers about it. Incidentally, I think when I presented this paper in the seminar

at Boulder, one of the people there of course (I say of course because he would be at the seminars anyway) was Sydney Chapman, and I think that started Sydney Chapman on being interested in noctilucent clouds.

P: I see. Well, by the time your next paper on that subject rolled around, which is number 115 in 1969, quite a lot more data had become available, and it was then apparent that it was becoming more and more difficult to fit noctilucent clouds into the billow cloud mould and that some kind of internal gravity wave mechanism had to be invoked. Also, I guess in the intervening time, the older hypothesis about water being present in some form or other was revised. And it was then believed, in 1969 at least, it was widely held that there was in fact ice condensation on these condensation nuclei -- what they were was not clear. What is the present status of the view as to what the substance involved is?

H: I don't really know, and apparently there has been very little conclusive data. There have been attempts to send up rockets and get samples back, but people who did the sampling hadn't too much experience with studying these things. Though everybody still seems now convinced that the whole thing is that ... that water substance is involved in these things. I say water substance because it probably is ice but it may be something else. I don't really know too much about what it is now.

P: What about the role of dust? Is this still ...

H: Well, I don't really think people, most people think that it is really dust.

P: Not even as condensation nuclei?

H: Well, it may very well be dust, but I wonder if many people still believe that it is dust coming from outside the atmosphere. In fact, there is I think also some idea that the nuclei, the nucleation may really be on some radicals involving hydrogen.

[Postscript 3-16 by B.H.: In other words, the condensation leading to noctilucent clouds may take place on hydrated ions. It is interesting to recall here that Humphreys as early as 1933 suggested that the noctilucent clouds must be made up of water substance.]

P: Bernhard, in this same paper you may recall that you distinguished, I mean the observations point to the fact that there are two quite different forms of noctilucent clouds, one with a very long wavelength of the order of about, what, about 100 km or more, and the

other on the order of 10 km, and there are corresponding differences in the periods, as well as the lateral extent. And you theorize here that the very long wave and the more persistent phenomenon probably is the result of internal gravity wave energy reaching those heights from below. Do you still think that's a valid ...

H: Well, I haven't thought about it; I imagine, I don't see any reason why not.

P: Nothing subsequent to that has caused you to change your mind about it?

H: No.

P: What about the short wave? Have you made any choice as to whether it's a billow cloud mechanism or internal gravity wave mechanism? You're still uncommitted about that?

H: I haven't really thought about it any more. It seems unfortunately there is very little study of these things anyway.

P: I noticed in this paper too, this paper of 1969, you cite the works of Hines. Did you ever work with him?

H: No. I know him personally, but ...

P: This number 118 is published in Kosmos ... is a popular account of the status as of 1970.

H: Where did you get that?

P: Didn't you give it to me?

H: Maybe I gave it to you, yes.

P: Why, don't you have one?

H: I don't know.

P: Well, if you don't you should certainly take this to complete your collection. I don't know where I got this.

H: Well, I'm surprised. When I was a kid, not a kid, a teenager, I subscribed to Kosmos. That's been very long in existence.

P: It has some striking photographs. I'm trying to find these here now.

H: Of course these would be largely I guess Ben Fogle's.

P: You may remember these wonderful pictures here.

H: Oh yes.

P: There were some wonderful ones published many years ago in Tellus taken by George Witt. In color, as these are. Then in 1966, you in collaboration wrote with Fogle what is probably even today the most authoritative review of the subject.

H: I don't know how authoritative it still is, but it certainly is the most complete.

P: Published in Space Science Review. That's quite an extensive monograph with a long bibliography, and it deals with the observational side of the subject in great detail.

H: Yes. Of course it's probably out of date now after 17 years.

P: Well, do you think it is? 1966. Have you followed the subject since then?

H: No.

P: In 19-- oh, this doesn't have a year. Maybe you can remember when that was. It's probably in this ... number 120. It's a short note, a chapter ...

H: Yes. 1972.

P: Okay. A chapter in, what is that?

H: It's a book, Thermospheric Circulations. There was really a meeting which Willis Webb called and people talked about thermospheric circulation and Ben Fogle and I talked too, and this is my contribution.

P: The one element of this that I noticed as far as I could see not present in your earlier discussions is the mechanism of viscous damping. Had you previously talked about that?

H: I don't know.

P: ... which you speculate might particularly affect the ...

H: I wonder, isn't that mentioned in this other paper?

P: It might be. You mean the Fogle one?

H: No, this one here. Let's see, which paper was that now?

P: It might be, Bernhard. I perhaps overlooked it there.

H: It may not be; I just don't know. I just can't tell.

[Postscript 3-17 by B.H.: Viscous damping is already briefly mentioned in connection with noctilucent clouds in paper 115, the Fuglister volume of Deep Sea Research. (I did not find it during the interview because it is a very brief reference.)]

P: No matter. The final paper in this series is the one published in the Flohn volume. What year was this now; this was paper number 121. What year was that?

H: 1973. That was for Flohn's 60th birthday, I think.

P: One interesting point that you make in this paper is the possibility that ... I mean here you discuss the seasonal, not seasonal, but longer-term variations in the appearance, or the incidence, of noctilucent clouds. And you suggest the possibility that this might partly be caused by penetrations of large amounts of water vapor into the stratosphere caused by either volcanic explosive events, or such things as nuclear explosions. Any event that could puncture a hole in the tropopause I suppose is what you really need.

H: I guess so, yes. But I mean the whole thing is of course highly speculative. I just had to write something for ...

P: Noctilucent clouds. The subject of noctilucent clouds clearly captured your interest and held onto it for quite a long time. It is, however, a subject very resistant to a clearcut elucidation. Would you ...

H: Well, surely because, in particular because it's difficult to make observations in situ. At one time they tried it, it was tried to generate noctilucent clouds by sending a rocket out and up and with I don't know how many kilograms of water which would explode around 80 km.

P: Oh, really? I hadn't known that. Was that actually done?

H: Well, it was done, but it was a complete flop. (laughter)

P: Nature is much to clever for us, is that it? You mean, nothing was observed?

H: Nothing was observed, and fortunately, nothing came down in the form of a big block of ice.

P: Well, Bernhard, we've covered two pretty vast territories here. Atmospheric wave motion, small-scale wave motion on the one hand, and noctilucent clouds and other upper atmosphere problems. Have you any ... since there's a little tape time left, uh, let's just talk about it in very general terms. Have you any comments about some of the papers we've been discussing? Actually there are some that I missed.

H: Well, you don't want to go into all of these things.

P: For instance, I missed number 14.

H: Oh. Well, 14 of course is the paper, another paper on billow clouds which is really a sequence of the preceding paper inasmuch as there I make the calculations about the length of billow clouds if there is a lapse rate in one layer or in both layers. And well, I was fortunate to get a few data for that which confirmed the calculations. If I remember correctly now, all the data came from somebody by the name of Tverskoy in one of the Baltic states. He sent them to me. The trouble is, that people when they make observations of billow clouds always measure the temperature discontinuity and the wind discontinuity, but not the lapse rate, because nobody thought it had any effect.

P: Another paper I missed, Bernhard, is number 43. Could you comment on that? The title of that is "The interaction between the polar front and the tropopause."

H: That, if I remember now, was an attempt to see how, what the mutual effect of cyclones, wave cyclones, Norwegian wave cyclones in the lower atmosphere, and temperature and pressure changes in the upper atmosphere would be. And I did that by the methods of perturbation theory. Well, it's the perturbation equations. I don't remember anything much ever came out of it. It seemed a lot of algebra and I never seemed to be getting anywhere.

P: Well, were you treating the atmosphere as a multi-layer ... ?

H: I don't know how many layers I had, probably only two or maybe three. Probably three. I would have to look it up now.

[Postscript 3-18 by B.H.: The fluid system in paper 43 consisted of two layers, wth a rigid lower surface, an internal surface of discontinuity in wind and density, and a free upper surface. The internal discontinuity was interpreted as the polar front surface, the free upper boundary as the tropopause.]

P: Yeah, this was 1939. Another paper that I missed in this sequence is — not in that wave sequence but on the upper atmosphere — is number 102. And you have a comment about that here.

H: Oh, well, that is really just a scientific report of the Geophysical Institute. Well, what I wanted to do there, I did write that the first time I was in Alaska, that was in 1964-65. And at that time Ben Fogle was working on his thesis which dealt with noctilucent clouds, and I wanted to write down as much of the theory as possible, the theory involving waves, and had hoped that people, particularly Ben Fogle, then would be able to make some observations on wavelengths, etc., which could be used to develop further the theory. But of course I should really have known better. If you want to apply any theoretical results to data — of your theoretical results to data -- you have to be right there when the application is made.

P: Do you know what is going on at the present time in Alaska on this subject?

H: Nothing, as far as I know. Occasionally they of course see noctilucent clouds and somebody might even take a picture once in a while, but Ben Fogle is gone, and there doesn't seem to be too much interest in it.

P: Is there no systematic program for..?

H: I think there is some sort of program in noctilucent clouds, but on this continent that is now run by somebody in the Canadian Meteorological Service. Which is really logical, because most of the United States is of course just a bit too far south to see them, except for Alaska.

P: Yes. I noticed that somewhere you mentioned the optimum latitude as being 60°, is it? Or is that the minimum latitude?

H: No, it wouldn't be the minimum latitude. It seems the minimum would be probably 50. Because if you go too far north, then you don't have any night at the time when the noctilucent clouds are most frequent. What I really wonder about, though, is I certainly must have seen noctilucent clouds in Germany as a teenager, and in particular I must have seen them because I certainly looked at the sky a lot since I was interested in astronomy. But I just didn't know about them. I saw the zodiacal light quite often for some period.

P: Were you ever a keen observer, I don't mean in a professional sense, of the atmosphere?

H: No. No, I was interested in astronomy. I had a telescope and used that.

P: I think Eric Palmén mentioned to me once that as a boy, he was enraptured by what was going on in the atmosphere, and was a very patient observer.

H: No, I mean I had of course a bit of interest in the atmosphere because I wanted to make sure that it would be clear in the evening, as an astronomer.

P: Any comments about the wave motion discussion, Bernhard?

H: No, as usual I really don't have anything to say about it today.

[Postscript 3-19 by B.H.: At the time when I began and did much of my work on waves the interest was mainly confined to surface and interface waves (the latter were then mostly called "internal" waves), presumably because of the preoccupation with the Norwegian wave theory of cyclones and the theory of billow clouds. My interests in the waves which now are called "internal" began much later.]

P: There was ... you mentioned earlier that in connection with this discussion of the interplay between convection and billow, there was a -- at the end of that paper, which was I think a presentation to the New York Academy -- there is a long discussion by Abdullah. Was he at that time already with you at NYU?

H: Yes. I think at that time ...

[Postscript 3-20 by B.H.: At the time when paper 64 was presented I was still at MIT and so was Abdullah who was working towards his Ph.D. degree. But later, after I was at NYU, Abdullah visited there twice, both times for a year, if I remember correctly.]

[End of side 2, tape 3]

TAPE 4
9 May 1983

Platzman: This is the fourth interview of Mr. Bernhard Haurwitz, being conducted on Monday, May 9, 1983. Bernhard, I could not locate number 33 from the Met Zeit. As a matter of fact, just now, in looking at your reprint collection, I didn't even find it there.

Haurwitz: I haven't got it either.

P: Do you recall what you did there?

H: Well, not exactly, but I was just very briefly discussing standing oscillations, I think -- not standing -- zonal oscillations in a stratified atmosphere, on a spherical earth. It's really just a little bit of what later has been written in this paper in Gerland's Beiträge in 1937. Well, I did that a few times during that period of my life, to send things, very brief excerpts of what I was doing, to the Meteorologische Zeitschrift.

[Postscript 4-1 by B.H.: Originally the material presented in paper 33 was written as part of a "Festschrift" for L. Weickmann which was brought out in manuscript form by the members of the Geophysical Institute of Leipzig University at the occasion of his 50th birthday in 1932.]

P: I understand. So from the standpoint of substance, can we assume that paper number 36, the big paper in Gerland's Beiträge, contains whatever was in number 33?

H: Yes, and considerably more. It's a small subset, you might say.

P: Okay. Now, 35, I also did not have access to that until I was able just a few minutes ago to refer to it in your reprint collection, and I must confess that I never have seen that paper, and I found it most curious. You are commenting there -- because of the fact that you are commenting there on work by Ertel, and I no sooner got to the bottom of the first page when out of the page popped the barotropic vorticity equation, on the sphere, except that, it had the wrong sign on the beta term, and then you go on to say that ... then shortly after that you give the equation with the correct sign, and it seems to me if I read it correctly, you imply that either one could have come out of Ertel's theory.

[Postscript 4-2 by B.H.: Looking at paper 35 and the two equations referred to by G.P., I don't see that either of the two can be called

the barotropic vorticity equation. But this does not matter as far as the following discussion is concerned.]

H: Well, yes. Well, I hate to say that, especially about somebody who isn't around to comment on it, but Ertel's theory is just ridiculous -- what he does there is just ridiculous -- I wouldn't even call it a theory.

P: I've never seen that. What did he do?

H: Well ... I don't exactly remember it now -- but what he did is he cross-differentiated the two horizontal momentum equations and added them to get what is really the barotropic equation, the barotropic vorticity equation, and then he put everything which he didn't like on the right-hand side and considered that the forcing term, and said that for the time being we set the forcing term equal to zero.

[Postscript 4-3 by B.H.: The description of Ertel's procedure which I gave from memory is not correct. He differentiates both horizontal momentum equations partially with respect to time and substitutes in each of these new equations for the first-order time derivatives of the velocity components the expressions given by the original equations. On the two new equations Ertel performs the scalar div operation and writes all the terms containing pressure on the left-hand side, the other terms on the right-hand side which he later treats as given or zero.]

P: What didn't he like? Presumably the non-linear terms.

H: Well, among other things, but there were more things, if I remember correctly. And, well, it seemed to me completely absurd, and at that time I was in Toronto and went to the applied mathematics department chairman Professor Synge, with whom I was quite friendly and asked him what he thought about it, and he pointed out some things which I also had already noticed. You see, some of the things which he had on the right-hand side were of course pressure terms. He still had them in. And Synge pointed out, if you know the pressure field to begin with, then you really don't have to go through all this rigamarole. Besides, there is really no reason why he subtracted the two momentum equations. "Why didn't he add them?" Synge said. You would of course get a different term on the right-hand side, but if you put it equal to zero later, it doesn't make any difference.

[Postscript 4-4 by B.H.: My recollection is faulty here, too: "pressure terms" and "pressure field" should be replaced by "velocity terms" and "velocity field".]

P: But he must have been on the trail of something, because he came up with the vorticity equation in a form implying, in fact, that somewhere along the way he had done the geostrophic approximation.

H: Yeah, well, but he doesn't ...

P: Maybe he didn't discuss it in those terms.

H: No, he didn't discuss it in any terms.

P: Well, this is, I was quite astonished, and this is a point that will come up again in just a moment, when we discuss the next paper, which I guess we might as well do, number 36, which is your, I refer to it as your big paper. It is actually a very substantial paper of almost 40 pages in Gerland's Beiträge. And it's a paper that certainly is very well known today among people working in planetary wave theory.

H: Incidentally, could I just go back to this Ertel thing? I might also mention, when I wrote this comment on Ertel's paper I sent it to Conrad, who was the editor of Gerland's Beiträge, and I don't remember whether I sent a copy to Ertel himself, or Conrad certainly sent a copy, or the comment itself, to Ertel, and Ertel never replied to it, and after a year -- after less than a year -- I wrote to Conrad and asked if Ertel had done something and Conrad said no, not yet, he would write to Ertel again and say that if he didn't reply soon, he, Conrad, would publish my comments without Ertel. So I suppose Ertel didn't have anything to say about it. It was quite strange. Of course, there are a few other places where Ertel made mistakes: in connection with these pressure variations aloft and near the surface. Rossby pointed that out.

P: My impression of Ertel's work is that -- and you don't have to comment on this unless you want to -- that it tended to be very formalistic and mathematically oriented, with the physical picture often lost track of.

H: I think that is true, though he certainly has done a lot of good work.

P: Yes, oh unquestionably. Paper number 36, 1937. Here you tackle the problem that was studied by Margules and Hough, and you are particularly interested there in looking at the ... what we now call the planetary waves solutions and getting approximations ... getting a convenient means of calculating the periods of such waves. They had only been dealt with in a very offhand way, I guess, by Hough, in somewhat more detail by Margules, but even then, not so that one could get good computational results.

H: Yes.

P: Now, I have a number of points I want to discuss about this, Bernhard. I don't know exactly where to begin, but I think one thing I would like to do in the course of our discussion about the subject ... oh, I forgot to mention what the subject is! I have to apologize to the listeners and readers. The first of our topics today is on planetary waves. And this is, I guess you might say, your opening salvo on the subject of planetary waves, and it was quite a salvo. What I would like to do in the course of the discussion, is to try ... one of the things, is to try to trace the evolution of your ideas on this subject, the origin and evolution of your ideas on the subject. To some extent that will be apparent by the nature of the work itself. But, is it true that prior to 1937, prior to this work culminating in this paper in Gerland's Beiträge, you had not made a ... I mean this is a subject that had not concerned you? Is that a fair statement?

H: Well, the way you put it is true, but actually as far as the paper was concerned, my idea was not only to study these planetary waves but also tidal waves. Tidal waves was in fact something ... especially the explanation of the solar semidiurnal pressure oscillation was a subject which really interested me from the time when I was a student. And in fact -- I cannot really say now that this might have been the thing -- the reason I went into meteorology or atmospheric science or whatever you want to call it, and I even have still a small book by Defant, the elder Defant, who writes very briefly about the semidiurnal solar tide, and which is marked on the margin.

P: Is this "Ebbe und Flut"?

H: Oh no, that's ... it's a book called "Meteorology" by Albert Defant in a collection brought out by a -- now forgotten, I suppose -- German publisher by the name of Göschen, who brought out very cheap books, but mostly written by very good authors, and very well written. At any rate, so this paper which I wrote in 1937 which we are talking about now -- the Gerland's Beiträge paper -- the idea was there, whether I couldn't, by considering a stratified atmosphere, find an explanation for the resonance of semidiurnal oscillation. Of course, I was then naturally, I would say, interested in the planetary waves too.

[Postscript 4-5 by B.H.: The "Weickmann Festschrift" of 1932 (see Postscript 4-1) contained already a bit of material dealt with in paper 36.]

P: Yes. So I should mention that the other topic for the day which we'll come to this afternoon is atmospheric tides, and these two

subjects, atmospheric tides and planetary waves are very closely linked through Laplace's tidal equations, so the discussion perhaps will be going back and forth between them, but you say that, if I understand what you just said, that you had an interest in atmospheric tides right from the beginning in your meteorological career as a student, and ... but this ... I assume that this interest was latent until the time that you tackled the subject seriously. In 1937, or published in 1937.

H: Yes, that is true.

P: Was there something at that time that led you more forcefully into this subject.

H: It is quite possible because this is not so long after the time when Bartels -- who is mainly known through his geomagnetic work -- when Bartels published his paper on the atmospheric tides. He had rather a large paper on mainly the lunar but also the solar tide, in ... what is it called, the reports of the Prussian Meteorological Institute, something like that.

P: When was that?

H: I think that came out around 1929 or 1930, and that certainly must have influenced me.

P: It's interesting just as a side comment that 1937 was also the year that Pekeris published his paper on the resonance theory.

H: Yes, right.

P: But of course you weren't aware of that at the time you did your work, I assume.

H: Well, I couldn't really ... I may have been aware of it because I knew, of course, I had known Pekeris when I was at MIT before I went to Toronto. You know Pekeris, of course, was a man who was supposed to go to Germany for the seven months I came to the States, and he very fortunately decided not to go for other reasons which had nothing to do with the later developments in Germany, so I met him, when I came to MIT.

P: I didn't notice whether in your paper of 1937 you refer to Pekeris.

H: No.

P: You don't.

H: At least I am pretty sure I didn't.

P: Now as I mentioned before, your ... in addition to your interest in atmospheric tides, you did want to pay special attention in this paper to the planetary waves ...

H: Yes.

P: ... and on page 220 of this article in Gerland's Beiträge is the famous formula for the propagation speed or period of nondivergent planetary waves. Of course, you mentioned that ... I don't have that, I should have brought that paper in here ... but you mentioned that Hough, I believe you mentioned that Hough had given that formula, and he did.

H: Yes, I imagine so. I could of course get the paper from the back room ...

P. Yes, that's true. Now we have the paper here and I was referring to page 220. I know that there you mention Ertel, and I want to come to that point in just a momeont, but do you also cite Hough or Margules in that connection?

H: No, not on this page here. Ertel, of course ... the reference to Ertel of course is the one which we discussed a little while ago.

P: But now okay. Coming back to that planetary wave formula, you do mention Ertel as having gotten this result with the wrong sign, as a result of his strange manipulations of the vorticity equation.

H: No. I have shown elsewhere that his -- namely Ertel's -- results are based on an erroneous interpretations of his purely formal solution.

P: But don't you say something about the sign having been wrong?

H: Right. Sorry. Yes, it was a negative sign. Yes, that's right.

P: In any case, the point is not really to bring Ertel into this too much, that's not too important, but it is rather, what I had in mind, is to establish the link between this paper of 1937 and the work of Margules and Hough. I don't recall incidentally whether Margules himself gave that formula in the ...

H: No, I don't think so.

P: He never considered the nondivergent case explicitly ...

H: No. Of course he gave quite a lot of numerical results which clearly indicate that he is talking about nondivergent waves, and of course his calculations must have been much more awkward than the calculations when we follow Hough's method. The only person who ever did that again after Margules, at least in meteorology, is Lettau, incidentally. I mention that in the introduction to the translation of the Margules papers which NCAR published, that Lettau in his doctor's thesis wants to explain the 36-day wave, which Weickmann found, and he uses the Margules method, or Laplace's method if you want to.

[Postscript 4-6 by B.H.: Margules' method is also described in H. Koschmieder's book on Dynamic Meteorology, with Lettau's calculation as a specific example.]

P: Well you just mentioned this 36-day wave. That's one of the peridicities that was under discussion, among many others, in the Leipzig school as a result of the impetus of Weickmann's work on symmetry points.

H: Yes.

P: And apparently, I gather from reading your paper of 1937, Lettau wanted to identify that with a particular Margules type wave of n equal 10, k equals 4. The 10-4 harmonic, so to speak.

H: Yes.

P: But you take exception to that, pointing out that there are many factors that could change that wave type, that could change the period.

H: Yes, and of course, there is also this awkward degree of freedom which he has by putting a Rayleigh friction factor in which just gives you the right period. I don't say that it's wrong, but it makes it less convincing certainly.

P: Yes, it's somewhat odd and coincidental, I'm sure, that in your paper in the Journal of Marine Research which you published just a few years later you point to the possible significance of the 5-2 wave, which is just exactly half of the ... (laughter).

H: Well, I'm more modest.

P: Did you have any other comment to make at this point. We may want to return and refer to this paper again, but ...

H: No; the only thing is of course that I really had lots of fun doing that work because I had to do all of this on a rather slow calculator.

P: What kind of calculator?

H: Oh, an old Monroe which ...

P: One of these you turn by hand?

H: No, it had electricity, but it took quite a while for some divisions to come out and you had to do some of the calculations very exact with continued fractions, so I still remember -- I was a heavy smoker at that time -- and when one of these long divisions had to be ground out by the machine, I had time enough to light a cigarette. Today, of course, you would program the whole thing obviously, but I spent lots of happy hours just slogging away.

P: Incidentally, coming back a moment, you mentioned earlier that one of your aims was to try to take into account more realistically the stratification of the atmosphere. Margules had considered an isothermal atmosphere with isothermal changes of state, and you considered a more general autobarotropic atmosphere.

H: Yes, which of course turns out -- in fact that has been shown really by Vilhelm Bjerknes -- it turns out that for the purposes that Margules and I had in mind, an isothermal atmosphere with isothermal changes of state is just as good or as bad or as unrealistic as an adiabatic atmosphere with adiabatic changes of state.

P: Yes ... I was going to put it this way, that in the tidal theory -- in the theory of the Laplace tidal equations -- the realistic thermodynamics is adiabatic. That means that the autobarotropic atmosphere is one with an adiabatic lapse rate. And so in that sense you are not taking into account the stability of the atmosphere, except insofar as the double layer provides that.

H: Yes. I think actually this -- as far as I was concerned -- this idea I had, largely because Vilhelm Bjerknes had said so, that it is much easier to deal with autobarotropic layers -- he was quite general -- and bring in the baroclinicity, if that's the right word, bring in the baroclinicity by stratification, by subdividing in different layers.

P: Yeah, well, sure. Okay. Paper 46, now, is the first of a famous pair of papers that you published in the Journal of Marine Research in 1940, the first one called "The motion of atmospheric disturbances," where you considered planetary waves on a beta plane, and the second

called "The motion of atmospheric disturbances on the spherical earth." This is, in the, yes, in the first of these papers, you discuss some of the questions that had been considered by Rossby in his paper in the Journal of Marine Research in 1939, such as the stationary wavelength, but in addition you go on to consider the effect of friction, and in a more complete way, the effect of forcing in the vorticity equations.

H: Yes, especially some sort of an idealized crossing of such a wave from the ocean to the land through a solenoidal field.

P: I, of course, re-read your paper having read it many times in the past, and one thing that I noticed that I perhaps don't recall I noticed before, you also considered the initial value problem for the vorticity equation; you gave a formal solution to that, and you discuss the consequences of the dispersion associated with planetary waves, and I recalled that T.C. Yeh -- I don't know whether you know him ...

H: I don't know if I know him; I know of him anyway ...

P: ... in 1948 or -9 published a thesis dealing precisely with this sort of question, the effect of beta on the dispersion of planetary waves. Of course, that was about 8 years later. Incidentally, in the list of references to this paper I found a reference to Exner's Dynamische Meteorologie, but I couldn't locate where in the text you actually referred to it. But maybe we can go into that later. I was curious to know what aspect of Exner you were mentioning.

H: I haven't any idea ... you know ...

[Postscript 4-7 by B.H.: The reference to Exner's "Dynamic Meteorology" in paper 46, about which G.P. is asking, is found on p. 44 of the paper and deals with the magnitude of the "Rayleigh" friction coefficient.]

P: We can do that later. Then in the same year, in the same Journal of Marine Research, you published "The motion of atmospheric disturbances on the spherical earth," and here you mention the connection between this work and Margules' oscillations of the second class, and you re-derive the planetary wave formula on the sphere, and this is where you also note that the P-2-5 wave has a structure that is realistic for the semi-permanent centers of action, and also for the prevailing westerlies is approximately stationary. Uh, and you go on to discuss the forcing of the barotropic vorticity equation and the possibility of resonance of stationary waves due to such effects as continentality.

H: Well, of course, I would say in a way this was something which should obviously be done, to assume that the earth is a sphere if you have such long waves.

P: Yes. One of the questions that always, I'm sure, interests anyone who has read anything about this subject is the continuity of ideas starting with, well, let's say chronologically and historically with Margules, going through Hough, then I think, so far as I know, the next major work along these lines was probably by Taylor, wouldn't you say?

H: Yes.

P: ... and then your work in 1937, and now this, and then Rossby's work of 1939 ...

H: Of course, if you mention Taylor, there is also Pekeris. Taylor, I think mainly was concerned with the first-class oscillations, with the tidal oscillations.

P: True, but he showed the equivalence of the atmosphere problem to the ocean problem, and I think that was an important step. But in 1940, or rather in 39 and 40, were you still in Canada, in 1939 and 40? At the time you did this work, published in the Marine Research?

H: Yes, I was in Canada, yes.

P: How much contact did you have with Rossby at that time?

H: Well, not too much, but I think I was at MIT, not at MIT, actually I think officially at Blue Hill, in 1940 probably, for a month in fall. It may have been 1940 or 1941, and I remember there was at one time a meeting, a few days' meeting, or just a one-day meeting while I was at Blue Hill, and of course at that time I also spent quite a lot of time over at MIT. Rossby told me about this meeting where people would come and talk about things related to the formula for the propagation of long waves in the westerlies, and well, we are here on the record, but even so I will tell, I still remember how I said to him: "Oh, I see, you got -- just like your great countryman Edgar Bergen -- you got your Charlie McCarthys -- together" (laughter). For those who don't know any more, Edgar Bergen was a well-known Swedish ventriloquist.

[Postscript 4-8 by B.H.: My visit from Toronto to Boston cannot have been in 1941 because in summer of 1941 I went back permanently to MIT. At that time Sverre Petterssen was chairman at MIT, and Rossby had left.]

P: Yes. I didn't know he was popular even in those days.

H: Well, I always listened to him ...

P: Well, did Rossby discuss his planetary wave work at that time?

H: He must have, yes. Well, other people were talking about planetary waves too.

P: And, in connection with your paper on the motion of ... on the spherical earth, you certainly, well, let me not presume anything -- is it fair to say that you were quite aware of the connection between that paper and the one in 1937?

H: Oh, yes.

P: I mean, that's almost a simple-minded question. But what is less obvious, perhaps, is the extent ... Oh, let me ask you another question. The earlier paper, at least was published slightly before that, although the work probably was done at about the same time, on the beta plane, did you also recognize the connection between Rossby's formula and your 1937 paper?

H: No, I don't think at that time ...

P: When did that come to mind?

H: Well, I think that it came to mind only ... let's see ... the connection between Rossby's formula and ...

P: And the Hough-Haurwitz 2-omega-K over n,n+1.

H: Well, that only came to my mind at the time when I wrote the paper on the Rossby waves on a spherical earth.

P: Now those two papers were published in the same volume, both 1940. But ...

H: Well, there must have been quite some time between them.

P: I see, okay. It couldn't have been too much, because Rossby's paper was published in 1939, but maybe a couple of months, you mean.

H: Yes. Of course, today, if I were to do such a thing I probably would not publish as two papers but as one paper. But that was simply the way I worked and published, at least at that time, partly because I had, of course, quite a lot of other things to do in addition to any purely scientific work, largely with teaching, instruction for the Canadian Met Service.

[Postscript 4-9 by B.H.: With regard to work other than research during the time when I worked on the papers 46 and 47: There was teaching in the regular graduate meteorology course, additional teaching to train more meteorological forecasters, and preparation of instruction booklets for the British Commonwealth Air Training Plan, and other war-connected work.]

P: Now, when Rossby did his work, I have always felt, although it's a question I never asked him, that he was not aware himself of the connection between that work and, let us say, Margules, or Hough. Do you have any impression about that?

H: Well, I only know that, to the best of my knowledge, there was a complete absence of any references to Margules or Hough, and I suspect Rossby simply didn't know about it, but he must have known about Margules, of course, but he may simply ...

P: Not in this connection, though.

H: ... not in this connection. With Hough ... he may even not have known about Hough. I'm sure I started to know about Hough through this paper by Bartels which I mentioned before. Bartels mentions Hough, and I even at that time bought the two reprints of Hough's, which I still have.

P: Did you ever, after your spherical paper came out, have an opportunity to talk to Rossby about that subject?

H: I must have, but I just don't remember what, if anything, we would have talked about at the time.

P: Let me see; I may want ... that's, well, if I can, at the risk of burning up tape needlessly, try to summarize the chronology and sequence and connection between these events, we have first of all Margules, in treatment of the atmospheric Laplace tidal equations, and of course Laplace himself had done that to some extent, and then we have Hough dealing with the oceanic problem, who comes out more explicitly with the planetary wave formula, but only in a very incidental way, showing that it's a limiting case. And, um, then there is a long interval with one exception that I want to come back to in a moment. There is the work you mentioned by Bartels in the late twenties.

[Postscript 4-10 by B.H.: I should point out that G.P. thinks here of the waves dealt with in my paper 40 which are a limiting case of Hough's waves of the second class. Hough actually does treat the second-class waves more generally. In the summation Lettau's work on the 36-day wave (1931) should be mentioned before mine.]

H: Of course, that is strictly on tides, not on the waves of the second class.

P: Okay. And then your paper of 1937, in which for the first time these waves of the second class are considered in their own right, so to speak. And then we have ... perhaps, it should be mentioned that Jack Bjerknes in the same year, 1937, published his well-known paper on the upper wave. Of course I am not implying that there was any connection between that and your study of the Laplace tidal equations, any obvious connection, but nevertheless that was the germinating point for the Rossby wave, which came along a couple of years later, and then only a year after that, in 1940, you showed how that Rossby wave was connected with the tidal equations. Okay. The one exception that I referred to is work by Lamb, that is often overlooked, in what is considered to be the second edition of Lamb's hydrodynamics published in 1895, actually slightly before Hough's work was done. He considers a rotating paraboloid. Now the parabolic shape of the bottom will give a potential vorticity gradient analogous to that of the planetary gradient, and as a result he does indeed get the two classes of waves, he discusses them explicitly, he even calculates some frequencies for the westward propagating waves, discusses their limiting properties. But then he drops it there, and doesn't really do ...

H: Does he have that in his later editions?

P: Yes, it continues in all the later editions.

H: I was going to say, I seem to remember vaguely having read a bit about it, and just very ...

P: Well, Lamb was at the time he did this in 1895, he was not aware of Margules' work. That was, however, later brought to his attention. I think he mentions that in a footnote ...

H: Yes, he says Chapman calls his attention to it.

P: Now. Let's proceed. I can see that this is going to a somewhat longer session than usual, or rather, it's conceivable, Bernhard, that we won't get on to much of atmospheric tides today, but let's not worry about it. We'll just carry on. Paper number 69, in 1949 concerns the instability of a vortex sheet. Its title is, "The instability of wind discontinuities and shear zones in planetary atmospheres." Your concern there was, or one of your concerns, was the question of how reliable is it to look at the markings on the Jovian planets and assume that they propagate with the mean flow of the atmosphere, and on the basis of that assumption then to draw inferences about the atmospheric circulation. How reliable is that

premise? And you point out that these markings might very well be instability phenomena on shear zones on those atmospheres, and if those features propagate at speeds significantly different from the zonal flow, then the interpretation is correspondingly unwarranted. So, I mean, the inference about the zonal flow becomes uncertain, then. You consider in detail the case of a discontinuity in the zonal flow, and the particular feature of this that makes it different from your previous studies of this type of problem is the inclusion of the beta effect. And that is quite intriguing for a number of reasons, but first of all, apropos the question that you set out to answer. What you find, in fact, is that the disturbances do indeed propagate at speeds slightly different from the mean flow, but the differences are not sufficiently great to be of concern for interpretation of markings on planetary atmospheres. That's what I understood your conclusion to be. Is that a fair ... ?

[Postscript 4-11 by B.H.: Rereading paper 69 I think it is fair to say, as G.P. suggests, that my opinion was that the differences between the motions of disturbances in planetary atmospheres and the speeds of mean motions are small enough to permit regarding them as mean atmospheric velocities. However, a somewhat casual rereading of this paper 69 makes me wonder if I did not underestimate the accuracy of astronomical measurements.]

H: Look, I really had completely forgotten about this paper, but it's probably a fair description. I don't know why I confined myself entirely to a discussion of what happens on Jupiter, but I'm sure I was also interested in possible implications for the terrestrial atmosphere, even though at this time I don't remember that I thought of anything in particular then.

P: You also go on to consider the case of a transition zone, which you had previously studied and we have already discussed this in the case with no beta. What I found intriguing about this paper is that in the same volume, I don't know whether it was the same issue, but certainly the same year, 1949, Kuo published his study of the barotropic stability problem, and these, this, that problem is very closely related to what you're discussing here. Uh, in a sense one could say that this work of yours foreshadows Kuo's treatment of the barotropic stability problem. You come up with a cubic frequency equation, you may recall that. And when there's no beta, that frequency equation is a quadratic equation that gives the usual Holmholtz instability. But beta adds a new twist to it, and we can come back to that later, if you like, but I just wanted to mention the coincidence of this coming out at the same time as Kuo's ...

H: Of course, that you get a cubic frequency equation means that you have three roots, that is you have three different types of

frequencies, and this is what you find in Hough's problems too, all the time, of course you have two frequencies for the tidal waves, one in the one direction, the other in the other direction; and the other is the planetary wave. Basically it's the same here in this simplified problem.

P: Numbers 78 and 9 are the next on your list of papers dealing with planetary waves, but unfortunately, I could not locate those, Bernhard, and I didn't take the time to look at them here, but I did want to ask you particularly ... Well, let's discuss them a little bit. I hope you can remember enough to comment on number 78, let's say.

H: Well, 78 is largely a discussion of again this problem of the resonance magnification of the solar semidiurnal pressure oscillation, and I discuss all sorts of difficulties about it which were well known, and in particular then come to the conclusion that despite all these difficulties there must still be something which gives just the right equivalent depth or equivalent height, or whatever you want to call it, in the atmosphere, so that the magnification can be as large as it seemed, still at that time in 1952 or so.

P: Did you do any theoretical analysis?

H: Very little. If you are really interested in the paper I can give it to you.

[Postscript 4-12 by B.H.: There are quite a few theoretical calculations in paper 78 on the resonance theory of the semidiurnal solar oscillation contrary to what I said to G.P.]

P: Okay, yes, I would like to see it. Now ...

H: I wanted to say something else. This paper was written, you might call it a little bit under stress or duress or something. Well, nothing serious, but my former professor Weickmann had his 70th birthday, which at that time, in 1952, seemed to me a very old age, and so I had to write a paper for him. That is incidentally also the reason why it was written in German. But that is the trouble very often with these jubilee volumes. You know that the authors are asked to write papers, to contribute, and they really don't have anything monumental just at the moment, but they cannot say no; that would be an insult.

P: Bernhard, number 79 is a very well-known work that is the starting point for a whole new way of dealing with atmospheric flow patterns by ...

H: And you say you have not seen this paper?

P: I have seen it in the past, but when I tried to find it in the NCAR library, I ...

H: Have you got a copy at home, because I know that I have still two copies, so I can give you one.

P: I don't know whether I have it at home. But in any case, thank you for the offer. Let's discuss this paper a little bit. This was a paper co-authored with Craig.

H: Yes ... well, the whole work originated or was a continuation anyway, of this work which started with the symmetry points -- I think we have discussed those -- and my project, when, after Pearl Harbor, my project at MIT to study symmetry points where Dick Craig as well as Ed Lorenz worked with me. Ed Lorenz left later, but then Dick Craig and I studied Kibel's method, and after that we started discussing this paper on "Rossby waves on a spherical earth" -- I like to refer to it that way, the paper which we discussed a few moments ago -- and thought now since there apparently is a physical justification, even though it requires simplification, since there is a justification just like in geomagnetism, one might as well try and represent the flow pattern, the pressure patterns by spherical harmonics ...

[End of side 1, tape 4, beginning of side 2]

H: Well, I think I have explained, I finished explaining why we were led to attempting to represent flow and pressure patterns by spherical harmonics, and so we decided to do that. Of course, for one thing, we didn't have enough data. That is, we had to take, say ... I remember maps published in Shaw's Manual of Meteorology, for instance, and read pressures off the intersections and things like that. And then, of course, we had to do everything by brute force, that is, the harmonic analysis along the latitude circles, and then the representation according to spherical harmonics.

P: How did you do the latitude part of it; do you recall?

H: Oh, yeah, I recall it. We just took, for instance, the wavenumber one, and took the harmonic coefficients for each latitude, and represented it by a series of the appropriate spherical harmonics.

[Postscript 4-13 by B.H.: Correction to my remark concerning spherical harmonic analysis: the latitude distribution of the Fourier coefficients from the Fourier analysis for each parallel of latitude was represented by associated Legendre functions (not by spherical harmonics).]

P: At equally spaced latitude.

H: Yes, at equally spaced latitudes. And later, I think we decided that since the Southern Hemisphere was ... well, we did that only for one or two distributions, because the work took a very long time, because one of the things of course was, you always had to do everything twice. And you do it twice, the first and second time usually does not agree with each other so you have to go back and do it again, and we had at that time five assistants, five women who worked on the computing machines actually solving determinants and all such things, but as I say there were all these mistakes which made it so long. In theory, of course it's very simple. And we didn't really do very many of these things. Then we also simply assumed that we had an ideal pressure pattern or flow -- yeah, I should also say that of course it's only the flow pattern, according to the simple theory, as produced in my paper, only the flow pattern which is represented by spherical harmonics. The pressure pattern is something a little different; well, of course you can express it by superposition of two spherical harmonics, so we had to distinguish that too. And then we also did attempt to find some regularity or some law or something in the behavior of these different pressure patterns, but we simply didn't have enough material to do very much with it. The whole paper, incidentally, came out much after the whole thing was done. It came out in 1952. That was simply because it was published in the -- what was it called -- geophysical research papers of the Geophysical Research Directorate, and they had a lot of things with more priority to publish first.

P: When was the work done?

H: The work certainly was done before the end of the war. The last one may have been 1948 -- no, it can't have been 1948; it must have been finished by 1947, or even earlier, because in 1948 I was at New York University.

P: So it was all done at MIT.

H: Yes, it was all done at MIT, yes.

P: Now. There is a gap of, well the next paper in this sequence, which I don't want to come to just yet, is 1975, paper 126. But that belongs with the subject of planetary waves, but not this atmospheric flow patterns with spherical harmonics, and I want to continue that if I may, continue that discussion for just a moment. In the early 50's when you were at NYU, you continued this work I believe with the help of students there, and in particular a student by the name of Silberman. And I wonder whether we can talk about that a little bit. Was there any work along these lines done between the time of your work on it at MIT with Craig and the others and the time you resumed it at NYU in the early 50's? Do you recall anything in that interval?

H: No, I don't, and I think it was really largely at Silberman's initiative that it was taken up again.

P: Oh? How did that come about?

H: Well, he was apparently just interested in it. He was a student at NYU at that time.

P: Was he there before you were?

H: No.

P: But then how did he get interested in it?

H: Well, he ... let me see ... I'm confused now. I don't really remember. When was Silberman at NYU? Was he there after 1952? In which case ...

P: I don't know, and that's part of the uncertainty in this chronology. His paper, published in the Journal of Meteorology, I think was 1954.

H: Ah, yes, well, he must have seen the paper either before -- if I remember correctly now he was at the Weather -- yes, I'm sure he was at the Weather Bureau and was sent to MIT by the Weather Bureau, which in this case means Harry Wexler.

P: To MIT or NYU?

H: To NYU. And he worked there on these problems.

P: But did you put him up to it?

H: I don't really know if I put him up to it or if he did it by himself. I would almost think he did it by himself.

P: I wouldn't know where he would get the impetus to do that.

H: Well, he must have heard about it before, or seen the paper by Craig and myself.

P: Well, sure, that's quite possible.

H: I just don't remember any more.

P: It's even possible that Harry, having seen your work with Craig, put him up to it and said, now you go and work with Dr. Haurwitz on this subject. Then did Craig himself do anything further with this? Or was there just this one ...

H: No, I don't really think he did anything with it. I'm not entirely sure now; he may have done something, because I remember at one time later when he was I think in Florida already he wrote to me -- you see, we had also calculated at that time when we did the work on this Geophysical Research Directorate paper, we had also computed the pressure functions, the combination of two spherical harmonics, for the appropriate flow pattern functions, and as I say we had calculated those in addition to our calculations of seminormalized spherical harmonics. And he wanted to get the tables from me, but I had lost them by that time already.

P: Didn't he write a thesis where he published a formula -- let me see whether I can remember this -- the formula for the planetary waves with zonal velocity -- angular velocity.

H: Yes, maybe it was a master's thesis. It certainly wasn't his doctor's thesis. Anyway he ...

P: He lost something ...

H: ... yeah, he lost one term, and Neamtan ...

P: ... and then Neamtan, that's right ...

H: He pronounces his name "neemten," incidentally. Neamtan is a Canadian, you see, and I met him first when we were both in the Canadian Meteorological Service. He later went out to one of the western provinces, to a university, and I think he died fairly young.

P: I think you're right. I was talking to Phil Thompson about this the other day and Phil said -- I hope I remember this correctly -- that he ran into Neamtan, not -- I'm not sure exactly when it was, but it was certainly after he did this work, quite a good number of years after Neamtan did that work. Neamtan, apparently, became totally out of contact with that whole subject and hadn't the slightest idea that his work aroused any interest whatever, apparently. He was much intrigued to hear that it had. Anyhow, getting back to your own work. The spherical harmonic representation -- do you recall whether anything further was done on that subject after Silberman left NYU?

H: No.

P: Did you not tell me, however, that he brought it back to the Weather Bureau and worked on it further there?

H: I don't think so.

P: You didn't say that. Okay, very good. Uh, paper number 126 entitled, "Long waves in the polar atmosphere."

H: Oh, yes, well, I know about that. That was really at that time my first attempt to make use of the fact that as you go high into the polar regions, you might be able to get away with approximating the earth again by a plane, except that it wouldn't be really a beta plane there because the Coriolis parameters there changes, rather more slowly, it changes quadratically.

P: So you considered there a polar cap bounded by some high latitude like 60 degrees -- bounded to the south, that is, by some latitude such as 60 degrees. And then considered a polar plane approximation, as you say, in cylindrical geometry, to Laplace's tidal equations. I noticed that in this paper in 1975, you were then apparently not aware of LeBlond's work on a similar problem.

H: No, I became aware of it ...

P: Later on, because you mention it in 19 ... in a later paper.

H: Yes, of course I didn't mention it here. That's right. I was a bit embarrassed, as a matter of fact, when I saw it. Embarrassed for not having mentioned it.

P: Paper 127 is an extension of the same subject, this time including the vertical structure with particular reference to heating. And you considered there both the positive and negative equivalent depths, and the consequences of that. Now, the kind of heating you used there was exponentially dependent, I mean decreased exponentially from the ground up. I don't recall, I didn't make a note here of what physical mechanism you had in mind.

H: I don't think I had any physical -- I don't really recall it either, but I was going to say I doubt that I had any particular mechanism in mind, except assuming that, except assuming some fairly simple function. Simple in the sense that it is easy to get integrals.

P: But I mean a la tidal theory, let's say, did you have insolational heating, or turbulent flux?

H: I probably had, if anything, in mind turbulent flux, and the idea then would be ... but I'm making that up now. I don't know if I thought about it.

P: That was my impression.

H: ... that if it's turbulent flux, of course the amplitude of the oscillation decreases exponentially and that's what I did.

P: These approximate solutions of the tidal equations incidentally can be expressed in their latitude dependence as Bessel functions. This is what you point out and develop. Then not much later, in 1978, paper 129, you -- co-authored with Allison Bridger -- discuss the eccentric circumpolar vortex. And here you use the nondivergent vorticity equation but with a forcing term included. This is again applying the same polar plane approximation but it's a slightly different context now. And finally, in 1980, paper 130, you more or less review the mathematical technique involved there and set about formally showing the equivalence between the ...

H: Yes, that was the general idea. I might add that the paper which we discussed before, the eccentric circumpolar vortex, again was one of these which was a paper written for a jubilee volume for a friend of mine, who incidentally died a few years later.

P: Who was that?

H: Dick Longley. I don't know if you have heard of him; he was in the Canadian Meteorological Service and later professor at Edmonton.

P: He's the co-author of a book.

H: Hewson and Longley. Yes.

P: Well, Bernhard, I skipped, I mean I went very quickly over these papers here, not really giving you an opening to say anything about them, but ...

H: I don't know that I have too much to say.

P: Well, say something!

H: Well, I just said something about this paper on the eccentric polar vortex -- the polar vortex work. Well, what intrigued me there was simply that you can by Bessel functions represent a vortex which is not centered at the pole, and so I made some calculations and then when the question came up, what do I write for the Longley Festschrift, jubilee volume, that seemed to be a good thing to do.

P: It's curious; it just occurred to me that in the latitude dependence, there is this situation that you pointed out where you can use Bessel functions. The same thing is true in the vertical structure provided you pick the temperature with height in a special way, and although ... I guess it's for a constant lapse rate, is that right, that you can get Bessel functions? I'm not sure, something like that.

H: Yes, I think so.

P: But coming back to this polar plane approximation, I'm reminded of a rather humorous comment that was made to me by Rossby -- not about this work, but you may recall that he had several papers around the late forties and early fifties on the motion of high pressure domes, and ... although it was more generally atmospheric vortices of a large scale. And this involves the beta effect. And he was considering such polar anticyclones that might originate near the pole. Well, he was well aware of the fact that the beta effect was very diminutive near the pole, and strictly speaking, it vanishes precisely at the pole, but when you're nevertheless close to the pole, you'll still get a beta effect, as you say in the cylindrical approximation it depends upon the square of the distance. Well, his comment to me was ... well, he formulated this theory, now I'm guessing here; I would have to look it up to recall the details, but I believe that he formulated this theory without explicitly talking about beta. He managed to do it in such a way that he didn't have to refer to beta, because -- and this was the humorous comment -- he said, people raised their eyebrows when I started to talk about the first derivative of the Coriolis parameter. What would they think if I had to talk about the second derivative? (laughter)

H: Well, of course your story about Rossby not mentioning beta when he talks about the beta effect, reminds me, I one time saw a paper which has the word 'beta effect' in the title. Now I don't say there's anything wrong with it, because by that time everyone in meteorology, any reader of the Journal, would know what is meant by the beta effect, but the absurd thing was the beta, the greek letter beta, never appears in the whole paper. I don't know how he designated the parameter, the author.

P: Well, that's interesting. Let's see. Coming back to 1940 and the paper on the spherical earth -- no. Let's go back to 1937, this paper here, in Gerland's Beiträge. You give on page 220, we've already discussed that, the planetary wave formula for propagation speed of nondivergent waves. The context in which you discuss that is one of a limiting process for sufficiently large n. Do you recall that, Bernhard, right here?

H: Well, I don't really recall it offhand, no.

P: You say, "for sufficiently large n ... " and so on. Now that effectively brings you to the nondivergent limit, and you can get that limit in a number of ways formally; one way is by letting the equivalent depth become infinite ... isn't that so?

H: Yes.

P: And I think later on you point that out in some other work which I think we'll come to when we discuss ...

H: Of course you know, one thing in this paper if I remember now correctly, my ignorance in that case was that I never realized that if I wanted to put in the equivalent depth, I would just have to replace the R times T, the gas constant times temperature, by g times h. So I probably at that time -- not probably, I'm sure -- I had at that time a blind spot about the equivalent depth, and the relation to this paper. At least I would have to look through the paper much more carefully to find if I ever thought about the equivalent depth.

[Postscript 4-14 by B.H.: Relative to the equivalent depth and paper 36: looking through that paper again I find that I pointed out -- as V. Bjerknes had already done for plane horizontal autobarotropic layers -- that the height H of the homogeneous atmosphere may be introduced into the frequency formula by $RT_0 = gH$, where R is the gas constant for air, T_0 the air temperature at the ground, and g the acceleration of gravity. This fact is, near the end of paper 36, used to determine the frequencies for different values of T_0 from Hough's results for different depths of his homogeneous ocean.]

P: Well, I guess the question I was getting at, Bernhard, is when you came to the spherical earth paper in 1940, you started out with a nondivergent model. Did you know at that time how, formally, that model could be connected with the more general Laplace's tidal equations through a manipulation of the equivalent depth? That's more or less what I was getting at.

H: I don't think so. I would love to say yes, but I don't think so.

P: You do mention that later on, in subsequent work on the tidal problem.

H: Yes, but apparently I was quite slow to get myself into it.

P: Well, I think up to this point you had not yet tackled the vertical structure problem in a serious way, except for the double layer approximation that you used in 1937. And I suppose it isn't until one comes to that that this interplay is apparent. I don't know.

H: I imagine so.

P: It's hard to look at these things in hindsight and understand one's point of view that prevailed at that time. Bernhard, in looking at the clock, I realize that we are just about at the end of this tape, and that would suggest that we leave for the next occasion the atmospheric tides, because that's a very long subject in itself.

H: All right.

P: I mean, I am prepared to go into it now, but we've been at this for about an hour and a half, and I think maybe ...

H: Well, no, I mean it doesn't bother me.

P: We could start it, but we would have to take a new tape. We wouldn't get very far on this one.

H: Have you got a tape?

P: Yes, I have.

H: I would offer you mine, except I don't know how good that is.

P: No, no; I have, I come prepared with at least one or two extra tapes, just for this purpose.

H: The only thing, I would like to get myself some water.

P: However, this tape is still going, so ... I'm just wondering whether we -- there are some other points about planetary waves that we ought to talk about before we get into tides. At an earlier point, we touched on Blinova's ... no, Kibel ... no, who was it, I always am confused about that.

H: Ertel?

P: No, the Soviet meteorologist who also did ...

H: Oh, Blinova. Mrs. Kibel. But she always wrote under the name Blinova.

P: Blinova. Now, I was trying to establish some chronology of ideas in the development of the planetary wave theory. Where does she fit into this? Can you talk about that?

H: Well, no I can't really talk about it. I only feel certain that she must have come to the same formula, the whatever you call it, the Rossby formula, for a spherical earth entirely independently. Her paper, which contains this, was a little bit later, I forgot, one or maybe even two years later than mine, but she certainly -- I'm sure she wouldn't have seen my paper. Besides, if she had seen it, she probably would have mentioned it. And also what she did later is, she brought the whole thing a little bit further by assuming that the fluid is not barotropic in the horizontal, but that you get a baroclinic term, and she expresses that by a temperature gradient. Well, this is sort of a bit simplified, and shows then that under certain conditions, such a temperature distribution might produce some

resonance -- not really resonance, but something very much like a resonance phenomenon, that is if you represent the temperature distribution by a spherical harmonic, then the resulting flow pattern, if I remember correctly it was not the pressure pattern, would also show some resonance, but in a slightly different harmonic. And Dick Craig and I in this paper which appeared in GRD, we tried to carry her work a little bit farther, because she neglected one term and we took that term along. But I don't know how she fits into the whole thing from a, shall we say, American viewpoint. She ...

[Postscript 4-15 by B.H.: Blinova's paper in which the formula for non-divergent waves on a spherical earth appears first, as far as I know, is dated 1943. However, the formula may very well have been published by her earlier, although there is no reference to such a publication in her 1943 paper. As a matter of fact, in the same paper the only reference is to my paper, contrary to what I said in the interview. The introduction of a forcing function by Blinova described by me at this point in the interview is also found in the 1943 paper. The GRD paper in which we carried Blinova's work a bit further is paper 79.]

P: You said that you think that she worked entirely independently ...

H: I mean independent of anything I had done. She ...

P: Was she aware of the older work on the tidal equation, for example?

H: I couldn't answer that question. I don't know if she mentioned it in her paper.

[Postscript 4-16 by B.H.: Blinova makes no reference to tidal theory in her 1943 paper. She starts out with Friedmann's equation (which we would probably call the vorticity equation). This is presumably the Friedmann who was for a while one of V. Bjerknes' assistants in Leipzig and later director of the Central Geophysical Observatory in Leningrad.]

P: I would have to look that up too; I don't recall. Let's see. Are there any other elements in this fabric that we're trying to weave here, any other major contributions that should be mentioned?

H: Of course, there are now quite a few studies which discuss observationally or statistically Rossby waves, though I am always a little bit skeptical about this, because, well, one wave looks like another one, so to speak. And I am always reminded of one remark which Rossby made one time in a seminar at MIT when he was still

professor at MIT. That must have been before my Canadian time; in fact, H.H. Clayton talked about solar periodicities and about sunspots and atmospheric periodicities, and Rossby afterwards got up and said that he was wondering, but it seemed to him if, with all the periodicities which have been claimed in the meteorological data, that you would really get a continuous spectrum if you plotted them. It didn't make Clayton very happy.

P: Might not have made Weickmann very happy either.

H: Well, he wasn't around. But I agree with that too.

P: So, um, let me see, are we overlooking any aspect of this ... Nothing comes to mind.

[End of side 2, tape 4]

TAPE 5
11 May 1983

Platzman: This is the fifth interview of Bernhard Haurwitz, being conducted today on Wednesday, May 11. Bernhard, today the subject is atmospheric tides and we have quite a lot of ground to cover, very interesting territory. The first paper in the sequence on this subject is number 65, 1947, that is on the "Harmonic analysis of diurnal variations of pressure and temperature aloft in the Eastern Caribbean". You may remember more about this paper than about some of the other early ones, because there is a companion paper that was published only last year ...

Haurwitz: Yes.

P: ... on the subject, so as a matter of fact I, at the end of our discussion, toward the end of our discussion on tides, I'll show you a little classification that I made of the way in which your papers on tides break down in terms of subject matter, and for example there are I think three of four papers, or five papers dealing primarily with tides aloft and ... or in the free atmosphere ... and this is one of them. What prompted you to get into this subject, Bernhard?

H: Well, as I had mentioned before I have been interested in the subject of tides and in particular the solar semidiurnal tide at first, ever since I have entered meteorology. As far as this particular paper was concerned, it was strictly I think in the military they call it a "target of opportunity" [laughter]. When I came to Puerto Rico on a three-month visit at the invitation of ... or the instigation and invitation of Herb Riehl, he showed me these data which he had there, and on which he had worked himself too, and I thought that was an excellent idea to find out something more about the structure of the solar semidiurnal and the solar diurnal pressure oscillation. In retrospect now and on the basis of my since then acquired knowledge and critique of such data, I wished I had made more of an attempt at that time to determine some of the probable errors involved, or whatever you want to call it in a statistical sense, but at that time the whole thing seemed quite logical and reasonable and so I published it. There is as a matter of fact a discussion of the same data, not from this viewpoint but from a more general synoptic viewpoint, by Herb Riehl which either proceeds or follows that paper in the issue of the Bulletin in which this paper appeared.

P: Do you recall ... you spoke a moment ago about the statistics ... do you recall how ... what quantity of data you had available for this study which ... the study incidentally dealt with the estimates of the

S1 of p and S2 of p up to 16 km at ... was this all at San Juan, I believe?

H: It was not quite all at San Juan. There were some data put in with it from some other station ... there should really ...

P: But at least it was all Caribbean ...

H: It was essentially ... yeah, it was all Caribbean. It's just that there was some Air Force station nearby which Herb Riehl thought at that time it would be sensible to include.

P: You did ... I noticed that you did S1 of p and S1 of T as well as S2 for both p and T.

H: Yes.

[Postscript 5-1 by B.H.: With regard to paper 65: A harmonic analysis was made not only of the pressure, but also of the temperature. In retrospect, and as is again emphasized in paper 131, especially the temperatures are doubtful. The other station was Antigua, approximately 300 miles east of San Juan. The data were taken in October and November 1944, and about three weeks of data are from Antigua.]

P: And you noted there the tendency of S2 of p to decrease with elevation and S1 of p to increase. I think we'll get into that in more detail in connection with the very last item on our list today on tides, so perhaps we can proceed to the next paper, number 83 in 1955, called "The lunar air tide", which was co-authored with Sawada. Incidentally, how did you come to work jointly with Sawada?

H: Oh, Sawada came to New York University as a student and ... to get his Ph.D. I think he was the first Japanese student whom we had after the war. I remember that in particular because we had a Chinese ... well at that time he wasn't even a student any more ... he had graduated ... Wan-Cheng Chiu ... he is now professor at Hawaii ... so I thought ...

P: Oh, yes, I have met him.

H: You probably have, he is quite well known. At any rate, I still remember before Sawada came I went to Chiu and asked him if he would in any way be ... well, I don't recall how I expressed it ... either annoyed or embarrassed or something, to have a Japanese here and he looked at me and said, "Oh, nonsense", or something like that ... so that's just a side remark. No, but it was simply that he came over to New York University as a student and I was his thesis advisor and he

wrote then a doctor's thesis which had been on the lunar atmospheric tide. He computed the ... what you would call the response function we probably call it today ... for the lunar atmospheric tide under different assumptions about the vertical temperature distribution in the atmosphere, and as far as this is concerned, this particular paper is really just a summary which we thought we might get on the record in this particular journal because that journal at that time published abstracts of presentations at some international meeting, probably some IUGG meeting or something like that ... I don't remember exactly what it was.

[Postscript 5-2 by B.H.: Paper 83 by Sawada and myself was in fact submitted to the IUGG meeting in Rome 1954, which neither of us attended.]

P: Did ... is that the ... I'm referring now to Sawada's thesis that you mentioned a moment ago ... this is a minor technical point and I may have mentioned it before ... I have the impression that by choosing ... if you divide the atmosphere into layers and in each layer you choose the temperature profile in a certain way ... I've forgotten exactly what that way is ... you can force the vertical structure equation to have exponential solutions.

H: Yes.

P: Is that what he did ... ?

H: No, I think ... I don't really remember now any more what sort of divisions he made. He didn't really divide the atmosphere into very many layers ... he had I think a lower layer, a troposphere with a decreasing temperature and then he made various assumptions about the magnitude of the temperature in the isothermal stratosphere and things like that and tried to get better agreement with the observed phase and amplitudes.

[Postscript 5-3 by B.H.: Sawada's thesis considered actually quite a few different temperature profiles with up to, and even more than, five layers, some of them isothermal, others with linear temperature increase or decrease, leading respectively to exponential or Bessel functions for the solutions.]

P: But did he ... did he do the vertical equation analytically in each of these space intervals?

H: Yes.

P: So he had to pick temperature distributions that had some special characteristic.

H: Oh yes, unfortunately. I think that was really before the time of the computers. Computers of course existed at that time ...

P: Now your conclusion there ... Well I think, is it not correct that his aim in his thesis work was to develop a catalog of L2's that corresponded each one to a different temperature structure and then to hope that the data on L2 might permit one to discriminate between these different structures?

H: Yes. And also I think there was some idea that one might find an explanation of the variation of the lunar tidal amplitude especially, but also phase angle, with the seasons.

P: Aha. Oh, I see, yes.

H: I shouldn't really say with the seasons because that seems to be the same way ... it's not inverse in the two hemispheres, the variation ... the amplitude is in both hemispheres, largest in summer and smallest in winter. Of course if you get more and more tidal data you see that this is really just a general rule, but it's quite often not satisfied.

P: Okay. This point incidentally about the question about whether the L2 data enable one to make such inferences particularly about the stratopause temperature: that has more much more recently been disputed and in fact there was a dispute, you might say, although that's too strong a word, in the literature, between Lindzen on the one hand and Geller and Hollingsworth, I believe, if I remember correctly, both of whom you may recall worked on the ...

H: I know, yes ...

P: ... on the dynamics of the lunar tide ... concerning this point, this sensitivity. I think Lindzen's point is that L2 is sensitive to the entire structure of T nought of z and I believe that Sawada's different profiles mainly concern changes in such things as the stratopause temperature.

H: Yes.

[Postscript 5-4 by B.H.: Sawada's thesis had about an equal number of profiles with different stratopause and mesopause temperatures, but it is difficult to characterize the twenty profiles in one sentence!]

P: ... but leaving the lower part of the atmosphere more or less the same in each case. Well, incidentally before we get farther on atmospheric tides can you explain to me something that's always mystified me. Why is the lunar symbol of the atmospheric tide "L" and

not the Darwinian symbol "M"? Do you know what the origin of that was?

H: No, I really have no idea. I think it may be but I'm now speculating ... if you had asked me how to justify it, I would say that certainly if you make a tidal analysis you ... from data ... you don't really expect only the effect of M2 ... of M ... to show up in L, but I don't think that has anything to do with the origin. I just don't know. It may simply be that whoever started that out first, I suspect it may very well have been ... no, it must have been somebody before Sydney Chapman ...

P: Well, I was ... my guess would have been that it was Sydney Chapman.

H: It could very well be, anyway ...

P: But it's only a guess.

H: Yeah, well, I would only guess ... it wouldn't be too difficult to find out, but I would argue that ... I would suggest that whoever thought about it, maybe it was even Chapman, just didn't know too much about the Darwinian tidal theory and all the different symbols for the different periods, so then I guess lunar and solar just calls for L and S.

[Postscript 5-5 by B.H.: The nomenclature L and S may very well have been first introduced by geomagneticians before it became known that the lunar and solar variations of the magnetic elements had anything to do with variations in the tidal potential, or in the case of S, mainly with variations in solar radiation intensity.]

P: The next is paper 84, 1955, which is a joint paper, a major work by you and Möller and there two things are done. First of all, you do what I gather is the first global analysis of S1 of T and S2 of T.

H: Yes, I don't know that we have S1 of T in here. I'm not quite sure now, I would have to ... I just see the 2T here ... yes, semidiurnal temperature ...

[Postscript 5-6 by B.H: The paper 84 by Möller and myself considers only the semidiurnal temperature and pressure wave. The diurnal temperature was analyzed in paper 96.]

P: I beg your pardon.

H: I think this is true and I think I don't know how that thing came about. I only remember that we had invited Fritz Möller, Professor

Möller, at that time to come to New York University and he had, if I remember correctly, already started making that analysis before he came over, and so when he came over and I found him doing that I really got into it, I mean we obviously started talking about it and then started working together, that's how the whole thing came about. The analysis of the semidiurnal temperature distribution, temperature variation over the globe, that was largely Fritz Möller's idea.

P: Well, now, isn't it so that he was ... his field of specialty was radiation theory. Is that a fair statement?

H: Yes, that was I think his main field of ... certainly his main field ... but he has done quite a lot of other work too. He has worked on the Ekman spiral, for instance, and well he has also made at one time a fairly large study of the diurnal and semidiurnal ... or let's say the daily variation of the wind in the layers near the surface. I really couldn't begin now to think of the various things he has done.

P: In both of those that you mention, the Ekman problem and the wind variation are ... have thermal influence implications.

H: Yes.

P: Well, so you say that he came to NYU, or wait a minute, was it NYU?

H: It was NYU.

P: On a visit.

H: Yes.

P: And he came with this idea of doing a global S2 of T.

H: Yes, and then ...

P: Yes, how did the tide element get into it.

H: Oh, well, I don't really ... I simply cannot remember now, whether he had already the idea that he wanted to use it to study tides, but when I came in I suddenly thought it would be an excellent idea to use these things even though of course the surface temperature variation is just ... is a fairly poor indicator of the total thermal forcing.

P: And in this paper you pay particular attention to splitting the temperature wave into a westward propagating and a standing wave.

H: Yes, of course, that was in order to get the two forcing components, presumably for the standing pressure oscillation and the migrating pressure oscillation.

P: I think this paper is interesting for a number of reasons, one of which is that it is still in 1955 within the epoch in which no satisfactory alternative to the resonance theory had been developed and you and Möller were left here to do what was possible in those days to defend the resonance theory. It's interesting, perhaps even somewhat paradoxical that you considered ... you and he considered the thermal response of the atmosphere in the ... after having analyzed S2 of T and discussed it. You then went on to discuss the implications for tidal theory and you tackled the vertical structure problem with thermal forcing of a ... I guess you might say of the kind where heat is propagated up from the ground. I say this is somewhat paradoxical because in view of Möller's specialty as a radiation man one wonders why he didn't propose to include radiation ...

H: Yeah, I don't know ... I mean, but I certainly at that time was still just at the idea that the heat propagation, the heating, is largely propagated from the ground upward.

P: Yes. Well, that was the prevailing conception all the way from ... I guess from the time that Kelvin talked about the subject. Paper number 85 published in the same year, 1955, is strictly a data analysis paper and incidentally data analysis in this subject of atmospheric tides is one might almost say more fundamental than the theory is and without that ... and no systematic survey ... no, I shouldn't say that. There were systematic attempts to deal with data on a global basis that had been made earlier for example, by Simpson, isn't that so?

H: Yes.

P: And also by Chapman, but under the very severe limitations of data availability, and I think what ... it's fair to say that what you attempted to do throughout a long series of papers was to create a much more accurate picture of what was going on on a global basis than had theretofore been available. At any rate this paper number 85 deals with S2 of p at Bermuda.

H: Yes, I was going to ask, I seem to remember that must be Bermuda.

P: And you were interested there in classifying the data according to the possible thermal influence on S2 of p, you classified it by the daily temperature range and by cloudiness and so on. And you drew some comparisons between a marine and a continental location. Do you have any reflections on that at the moment?

H: No, again that must have been of course a work of opportunity. I must just have found so that I could get my hands on a reasonably long data set so I started doing it. As I said before, I also liked to do numerical work from time to time, so ... Besides probably that was when I was at New York, yes I probably had quite a bit of help with the numerical work at that time too.

P: Then a year later paper number 86, this is the ... excuse me one second, I have a little chart here I want to refer to ... this is the first of your two major works on S2 of p, on the global distribution of S2 of p. This one was in 1956 and the second one, which is now considered to be almost the final authority on the subject, was in 1973. Did you at this time, 1956, do statistics? I don't have a note of that.

H: No, there is no statistics ...

P: That's this one?

H: That's the one, yes ... and the way that paper originated was that, reading Simpson's paper, which probably you have looked at too, it just seemed that after that many years -- let's see that was in the 50's, yes, and Simpson's was about 1918, I'd say -- that is after over more than 30 years and quite a few more data accumulated, it seemed logical to do the whole thing over again. Of course, it isn't done exactly the same as Simpson's anyway, but it was largely at first an attempt to do ... just to repeat Simpson's analysis.

P: Right. Do you use spherical harmonics here, do you recall?

H: No, not really.

[Postscript 5-7 by B.H.: Paper 86 on the geographical distribution of the solar semidiurnal pressure oscillation did include quite a few computations of new data, especially in order to fill gaps in the geographical distribution, as indicated in the paper. Although the analysis of the distribution of $S_2(p)$ was not a spherical harmonic analysis, spherical harmonics were used for the distribution of the migrating wave.]

P: That was done the next time around, yes. ...

H: Yes.

...

P: ... Then in 1957 paper 87 is ... deals again with S2 of p, but now specifically for high latitude stations, there were 15 high

latitude stations. You were interested in the seasonal variations and also in getting a more accurate representation of W2 and Z2, particularly Z2 I suppose which would be expected to be more prominent.

H: Yes, and Z2 has the advantage that theoretically it's much easier to deal with. I forgot now what it is, it's a trigonometric function of a trigonometric function of the latitude.

P: According to Solberg, if you take K2 as a good approximation to it, yes, that's true. In fact, it seems to me that you did there or somewhere else -- I think it was perhaps in the Möller, where you did S2 of T — you used those functions, did you not, to represent the data.

H: Yes, I think I did it there and I may ... I'm not quite sure anymore now in retrospect, but I think it may also have been in this other paper which we discussed already, this paper which is really a successor of Simpson's analysis of the semidiurnal pressure oscillation, I think I used this function there, this functional expression for the standing oscillation there too.

P: Paper 89, 1957, is a theoretical paper and the question you asked there is to what extent the meridional variation of the basic temperature field affects the response of the atmosphere.

H: Yes, I don't really get very far with that. I think I largely discussed here just again the standing, the polar oscillation because it's so much easier to deal with.

P: Yes, it was interesting how you approached that, namely to argue that the main effect of changing the latitudinal temperature gradient could be approximated by considering the corresponding change in the vertical structure that would exist at different latitudes, and the implication for [of] that for changes in the equivalent depth. And so you simply said why not look at the way in which the perturbations of the equivalent depth can perturb the eigensolutions of the horizontal structure.

H: Yeah, well, a very similar problem has later been dealt with by Sawada when Sawada was here -- not here but in Boulder at NCAR -- for a longer visit of a year and he at that time tried to take into account not only the effect of the temperature gradient, but if you have an original temperature gradient you will also have a change of the wind ... at any rate, he tried to take both into account, but the whole thing became very difficult and he really couldn't get very far. That was still at the time when one didn't too easily go to computing machines yet, in fact, I don't really know what the

situation was here at that time. Otherwise he might have gotten much farther. As a matter of fact, I did most of that work when I was on my first sabbatical in Mainz, in Germany, with Möller. After Möller had been in New York before, he invited me to come to Mainz and I went there for about four months in 1955-56. As a matter of fact, I was just reminiscing with my wife the other evening, what I did for about the first two months, I copied the differential equation wrong. I did quite interesting things, but they were all with the wrong differential equation.

P: You're sure that they weren't more interesting than the truth?

H: No, I didn't go to any physical results here.

P: Paper 90, in 1957, is one ... S_2 of p ... where is number 90, oh, Bernhard that's one that I didn't have or did I ... oh, that's this one ... yes I have it now ... that is S_2 of p at 136 stations.

H: Yes, I think that was the first attempt by anybody to represent the seasonal change of the semidiurnal pressure oscillation -- solar semidiurnal pressure oscillation -- on a geographical basis. I don't think that had been attempted before. Of course, what both Simpson and I later had done was taking annual mean values of the amplitude and phase angles and I thought by that we might get some characteristic changes in the distribution. There were minor changes, but again I don't recall offhand now what they are. An interesting sidelight was, since we occasionally have talked about using computers, that was still at New York University, and most of the work was done by Miss Sepulveda, now Mrs. Williamson, and when she started, one of the people in the research division of the College of Engineering pointed out to her how much easier it would be to do all that analysis on a computing machine, which was probably true, except then when she took the man up on it, well he never got going and finally she really got discouraged when he asked her one day what is harmonic analysis, and she did it then all by hand in about four weeks or so and she really beat the computing machine.

P: I can believe that. You ... in the abstract you say the data were grouped into latitude zones and also ... I'm trying to see how ... oh, seasons, four months ... you grouped it into four months, January, March, July, and September. Were these 136 stations ... now was that roughly the same material that was used for the main S_2 of p, this one here [paper 86]?

H: No, I think that must have been much more, because I think we were ...

P: Which was more, this one?

H: No, no, the older one ... the seasonal representation. I seem to recall there were about 25 ... for the annual means I had almost 300 stations, 296.

P: Is there a reason why you didn't use the same ones for this ...

H: Oh, yeah, there is a perfectly good reason, of course, because for many of the stations you see we didn't make the analysis ourselves in most cases, we took them largely from Hann, and we couldn't get ... so we didn't have available the seasonal data.

[Postscript 5-8 by B.H.: The attempt made in paper 90 to obtain a global distribution of the seasonal variation of $S_2(p)$ has since been repeated with the aid of computers in paper 122. Note that in paper 90 four individual months are considered, in paper 122 the three Lloyd seasons (three combinations of four months each).]

P: I understand, okay.

H: And I might also say that of course we took all stations, all the material which we could get when we didn't have any idea what the errors ... the statistical errors might be, we just omitted stations where Hann indicated that he had half a year of data or something like that.

P: Okay, I understand. Then in 1961, some years later, paper number 94 is the, I think, the first of several papers where you talk about the S2 at meteor trail levels, that is, at and above the mesopause, and you talk here particularly about meteor winds. In a subsequent paper you use the meteor winds ... and we'll get to that in a moment ... to make inferences about p from the momentum equations. But I noticed also in this paper of 1961 you comment on Siebert's theory of the thermal tide for S2 to the effect that it indicates that after all no resonance really was needed to account for that. Siebert ... I'd like to somewhat later on talk about the history of the subject and the way the ideas developed, but just for the record at this point, this paper of yours in 1961 is what I'm referring to now on the meteor winds was in the same year of Siebert's first quantitative treatment of the thermal forcing problem.

[Postscript 5-9 by B.H.: One of the main aims of paper 94 was to indicate to upper-atmosphere physicists that it is necessary to have some statistical measure of the reliability of tidal determinations. It did not do much good. The same idea was later stressed, again not very successfully, in paper 100 published by WMO.]

H: I don't remember.

P: Yes, it was, it was that year. He had previously made the suggestion of the plausibility of that mechanism in his thesis, I guess, some years earlier, but as I understand it ... I haven't seen his thesis ... as I understand it that was more of a qualitative discussion.

H: No, actually it wasn't. I have seen the thesis.

P: Please correct me on that.

H: I don't want to correct you, I just want to add things. We're talking now about a paper of mine which was published in 1961 and I had been, as I mentioned before, on a sabbatical in 1955 with Möller in Mainz, but since I was for a year in Göttingen at one time during my studies, of course I went to Göttingen too, and I knew Bartels quite well, he was at that time the professor of geophysics, so in 1955 I also went to Göttingen and that is when I first met Siebert who was at that time Bartels' student, and who worked on his doctor's thesis. He had it almost completed, as a matter of fact, at that time.

P: I see. How did he get into it?

H: Well, I don't know how he got into geophysics in general, but I think ... I'm pretty sure, as a matter of fact, that Bartels suggested the problem to him ...

P: Tides.

H: Tides, yes, and so he started work on it and as a matter of fact I also remember that during that period when I was in Germany there was a meeting of the American ... the German Meteorological Society, as a matter of fact, to give it the right name, at that time it was the Meteorological Society of Frankfurt, they didn't have a unified meteorological society in Germany yet ... where Siebert presented his paper, his thesis, but that was ... of course he didn't present the quantitative theory, that would have been too long, but it was very definitely quantitatively established.

P: I see.

H: And I know now what you ... you are referring to Siebert's longer expose or whatever you would call it ... essay on the atmospheric tides in the ... what is it called ... not Ergebnisse der kosmischen Physik ...

P: Isn't it the Handbuch der Physik? No that's Kertz.

H: No, it's ...

P: Where is that? I've forgotten. We can look that up.

H: Yes, we can look it up. Anyway, this book with articles which the Academic Press ...

P: Advances in Geophysics.

H: Advances in Geophysics, that's it. As a matter of fact I remember now. I had suggested to Siebert he should write an article for it and I had written ... I think I was at that time one of the not editors but the ... what do you call it ... associate editors of these Advances and I suggested to the editor, Helmut Landsberg I think it was, that Siebert would be a good man and Siebert wrote that article then and it appeared in 1961.

P: Well, now to go back just a little bit. His thesis was in 1954.

H: Was it 54?

P: Yes, I think so. We can check that, but I believe ...

H: Yes, maybe it was '54, then he must have finished it by the time I came to Germany in '55.

P: Then you ... he gave a presentation to the Meteorological Society in Frankfurt. In what year was that?

H: That would also be '55.

P: '55, okay. Now I want to ask whether there was any difference between the conclusion he came to in his thesis on the one hand and what he said in his survey article in 1961.

H: No.

P: You don't think there was. I had the impression that in his thesis work he got too small a value by virtue of his having taken a rather unrepresentative temperature distribution and that that was improved by the time he wrote this thing in 1961. But I could very well be mistaken.

H: Well, that may be. I wouldn't like to swear to that.

P: Let's look into that later. Now, back to paper number 96, in 1962. That is the second global analysis of S2 of T and here I think is also included S1 of T.

H: Yes, I was going to say that [number 96], I remember, is a paper which takes the various temperature waves into account. I think probably just 1 and 2.

P: I think it's 1 and 2, yes.

H: Yes.

P: Again, paying particular attention to seasonal effects and to W and Z. This is a ... you might say ... an updated version of what was done by you and Möller in ... some years before.

H: I'd like to make a correction here for that. I see that I have also the 8-hourly period.

P: For T?

H: For T, yes. As a matter of fact as far as this paper is concerned, the origin of it was that I had been invited the second time to Fritz Möller ... we had become very good friends at that time ... I knew him from before the war, but of course then I never saw him until he came after the war over here, so when he went to Munich as professor he invited me to come there. As a matter of fact the way that came about is because he had actually at that time an additional position for a full professor at the university, but in Germany, at least at that time, it took much longer really to get the man. He also knew who he wanted to get, so he had the money in his budget, but he didn't have any warm body, so he asked me to come over and also give a course, and so I went to Munich and I decided that this would be an excellent time to make such an analysis because I knew exactly what was to be done. I had done the same thing for the semidurnal temperature oscillation, so I decided to do it while I was in Munich and I managed to finish it quite on time. And that is the reason also why I wrote things in German, I just wanted to see if I could still write German. I think I made one mistake in it as Fritz Möller pointed out to me, but he corrected it before it was printed.

P: This one, in contrast to the previous one, I think was done in Legendre polynomials.

H: Yes, that's right.

P: Okay, well now moving on to ... did you have something more?

H: No.

P: Moving on to paper number 97 of 1962 ...

[Postscript by G.P.: There is a break of continuity here, some conversation having been unrecorded.]

[End of side 1, tape 5, beginning of side 2.]

P: ... paper number 99 on S2 of p over North America, dealing with the geographical and seasonal aspects. You were interrupted there by the end of the tape.

H: Well, I was going to say that one of the things which I thought might be interesting was ... is that the amphidromic point, do you call it that in English?

P: Right. Yes, exactly.

H: That is the amphidromic point which is there -- not at the North Pole, because of the superposition of the standing and the migrating wave -- this amphidromic point must be, or we know is somewhere over Canada or maybe Alaska, but at any rate in North America so I thought I might get that in a bit more detail. I don't know how much that hope was fulfilled, because the observations of course are still ... there still are not very many observations.

P: And amphidromic points being of course places where the amplitude by definition is zero, are very sensitive to any slight changes of data in that vicinity.

H: I might also say that if I recall correctly, I had in mind for quite a long time to do such work and then at that time we had some money. That was at High Altitude Observatory in Boulder. We had some money to pay students for summer work, students who would later come for their doctor's degree, so this chap Avery, who was a very nice chap came and worked with me. He did actually most of the work, I did most of the write-up. Unfortunately -- he was a very good man -- unfortunately he later lost interest and went into I think medicine.

P: You consider that unfortunate that he went into medicine?

H: Well, for meteorology.

P: Okay.

H: Or atmospheric science probably.

P: Number 100 is this well-known orange-colored publication of the WMO in which you gave what I've always felt to be an outstanding summary of the state of tidal theory ... particularly as concerns the

upper atmosphere and by upper atmosphere you here mean at mesosphere heights, and above, but the background, the introduction to the treatment of the data problems there for the upper atmosphere is a background dealing with fundamental tidal theory as it applies to the atmosphere and it's something that I think is the equal of any textbook discussion that I have seen. How did this come about, your writing of that WMO publication?

H: I'm not quite sure any more, but I was at some meeting and I was on the aerological committee, or whatever it was called at that time, and we discussed at that time who would write ... let's see, who would write what concerning the upper atmosphere and I said as far as tides are concerned, tides in the upper atmosphere are concerned ... oh yes, Murgatroyd was in charge of this committee if I remember correctly, at any rate he is mentioned in here, at any rate, I just said that as far as that is concerned I certainly can write on tidal phenomena in the upper atmosphere. And then I did. I came out way ahead of everybody else because I had really done two of these papers before which we discussed, "Comments on tidal winds in the high atmosphere" and the other paper which we discussed after that.

[Postscript 5-10 by B.H.: How I came to write paper 100, a WMO Technical Note on Upper Air Tides, is quite clearly set forth in the Foreword by the Secretary-General, Mr. Davies.]

P: This paper for the WMO must have gotten you in the mood to do reviews because the next paper on our list is number 101 in the same year, published in Science.

H: No, that is ... well that didn't get me in the mood, but what happened there is simply I got a letter from ... I think it was ... what's his name, Abelson ... I don't remember now, I think Ableson was the editor at that time already. He wrote to me, he said at the suggestion of Dr. Reichelderfer and would I write an article on atmospheric tides. Now, Science is a fairly prestigious journal so I said, yes, and there it is.

P: Excellent. One interesting feature of that is, this was about the time ... this paper in Science ... this is about the time when ... it was after the time that people were more or less satisfied with the state of the theory for S2, but the full explanation of S1 had not yet been apparent, but you do mention there that a reduction in the intensity of S1 at the ground could be expected by virtue of the small equivalent depth implied by the S1 frequency. Number 103, 1965, is six stations in North and Central America, and you do L2 of p and S ... I have written here ... S sub n of p. In fact, there are in classifying your papers I found that four beginning with this one in which you ... these were all regional studies or individual station

studies ... you do four p's L2 of p, S1, 2, 3 and maybe 4, yes that's five altogether, and let me see, is there any comment to make about this one here?

H: Well, there is some comment in general about these things I'd like to make.

P: Please.

H: When Chapman came to New York University at one time -- that must have been probably in the late 50's, not very long before I left NYU and went to Boulder -- and one time when we talked together he said he really planned no longer to continue his analysis of data to determine the lunar semidiurnal pressure oscillation and he suggested it would be a very good idea if I could do that, and I said yes I was interested in it for one thing and it was something which as long as you have some support can fairly easily be done, it takes a long time to do that, ... and this is how this whole series of papers, especially to begin with this one here, came about because I ... whenever I got then data or even deliberately went out of my way to get data and made these analyses. And of course if you make a lunar tidal analysis, at least with the Chapman-Miller method, as it's called, then it's almost automatic ... there is not much problem to determine the solar semidiurnal and also the solar diurnal and other periods and ... so that is why these papers, which really deal largely just with observations, why they contain both lunar and solar data. And in many cases as I say of course the solar semidiurnal, et cetera diurnal, 8-hourly and 6-hourly are just determined incidentally because it's easy to do it, anyway.

P: Paper number 104 in the same year is the first of your major global analyses of S1 of p. And there in that paper you not only do the analysis for S1 of p, but you also discuss the Hough functions that correspond to ... that would be needed to represent that S1 of p.

H: Well, there of course, I don't know if I should say I goofed, but the mistake in there, with respect to the Hough function is that of course I didn't realize at that time that there were Hough functions belonging to negative equivalent depths. I think the first one who pointed that out to me was Dick Lindzen, as a matter of fact.

P: This is a very interesting and important point about the development of the theory that I want to get into when we finish our discussion of the papers. But, yes, that's true; in this paper you point out that the first three symmetric Hough functions have very small, relatively small equivalent depths, ranging from about 700 down to about 50 meters. And that these small values of equivalent depth would imply a certain amount of reduction in the surface response to

S1 of p. Also the fact that these small equivalent depths give you Hough functions that are more or less equatorially trapped, and so they would be rather unfavorable for a global heating distribution. Number 109 in 1967 ... I don't mean to rush over this, but I do want to come back to this subject that you mentioned of the negative equivalent depths, and that would give us an opportunity to go into that aspect of it in more detail. Number 109 ...

H: That's the paper in Nature -- there.

P: ... with Sydney Chapman is a summary, really, for Nature, of the present state of the knowledge of L2.

H: The editor of Nature wrote a letter to me and out of the blue sky, as far as I was concerned, and of course again as in the case of Science, Nature is a prestigious journal, so I said yes. I only wrote back that I would appreciate it if I could co-author this paper with Prof. Chapman, provided Prof. Chapman would be willing to do so. So Sydney Chapman and I wrote it together.

P: I see.

H: That's the only paper Sydney Chapman and I wrote together.

P: You mention in this paper the possible importance for the atmospheric lunar tide of coupling with the ocean tide, and I believe by that time Sawada had made an attempt to deal with that.

H: Oh yes. That's right, yes. Sawada did that; that was the first paper which he wrote when he came to NCAR for his year-long visit. Then he went to work on the effects of winds and meridional temperature gradients on the resonance theory.

P: Right. And in this summary paper with Chapman you point out that one of the remaining problems of understanding is to explain the annual variation, the yearly variation which could possibly be, you suggest a zonal wind effect, or meridional temperature gradient, although you tended to favor the zonal wind, I think, over the temperature gradient. Or conceivably it could have something to do with the vertical structure, the temperature.

H: Yes, I suppose so. Or maybe it isn't really so clear-cut as it's always put in the literature. I mentioned very briefly before that there are quite a few stations where the rule about the change of the temperature, the change of the amplitude of the lunar tide, from one season to the other one, is not as much the same in the northern and southern hemisphere. In particular that seems to be true as you go farther south in the southern hemisphere, south of about I guess 40°S,

the only trouble is there are practically no observations. Somebody should sit, people should sit for about 50 years and just make pressure observations every two hours or every hour.

[Postscript 5-11 by B.H.: Another point should be mentioned in connection with the discussion of the annual variation of the lunar tide as carried out in connection with paper 109. Some more recent unpublished work on this annual variation shows that the statistical significance of the seasonal differences is often quite small. Thus the reality of the seasonal variation of $L_2(p)$ may be less well established than it seems.]

P: Now there must be ... it just occurred to me that there must by this time be a substantial amount of information from Antarctica.

H: Yes, there probably is. I have sometimes thought of that. But, the trouble is that in Antarctica now, the amplitude of the lunar tide should be very small to begin with.

P: Because cosine squared will come to get you.

H: Yes. Well, actually it's even -- there is no theoretical reason for that -- it's rather cosine to the third power.

P: Observationally.

H: Observationally, yes.

P: I'm glad you mentioned that, because that is a question I was going to ask you. Namely, whether there is any theoretical explanation as to why that cosine cube should ...

H: Well, there may really be; I have never thought about it much. The funny thing, the peculiar thing is that it's true not only for the lunar tide; it's also true for S2.

[Postscript 5-12 by B.H.: The simple empirical expression for the amplitude decrease with latitude ϕ, viz. $(\cos \phi)^3$, rather than the second power -- which holds for $S_1(p)$, $S_2(p)$, and $L_2(p)$ -- may be caused by an effect of the earth's geography which causes one or more higher modes to be present in all these oscillations.]

P: And it seems to be satisfied so almost exactly. It's amazing how the points fall on that curve. Nineteen sixty-seven, paper 111, is also a lunar paper.

H: Yeah, that again is something where we got quite a few data.

P: Fourteen additional stations, mostly in the southern hemisphere, actually.

H: Well, there I went out deliberately and among other things I managed to get the yearbooks which had been published by the German Seewarte. That is the old Seewarte where Köppen worked at one time ... which was part of the Imperial Marine, Imperial German Navy. Well, they, I wrote to them and asked if they had data for the Colonies. I knew they had very well taken the data, the observations were very good, and in addition to that they were all printed. And they wrote back oh yes, they still had some of the old publications there, from the German Empire, and they were sending a stack like this. They had saved that from the bombing. And so I got quite a few of the data from this. In addition to that they also had quite a few handwritten data still. The way I knew about these things, incidentally, was that Bartels had studied the lunar tide at one station, Dar es Salaam, which is in Tanzania, and he mentioned there that he was going to make other analyses. Apparently he never got around to it, unfortunately, with the war and God knows what else. So whatever he had done apparently was lost later, so I started that anew and, well this is it. And in addition to that I also there -- I should say we also did there -- some of the determinations when you have only two or three observations a day. For the lunar tide you can do that very nicely.

[Postscript 5-13 by B.H.: With regard to the data from formerly German Territories in paper 111, I found when I spent some time later in Göttingen at the Geophysical Institute, that Bartels had in fact given some data to somebody who had started to analyze them for $L_2(p)$; but the work was not continued after Bartels' premature death.]

P: Yes. In fact that ... we'll come to that in your very neat visual demonstration of the lunar tide. Then comes paper 113, 1968, where you look at L2 of the wind at 4 stations in North America, and incidentally you comment there that the direction of rotation of the wind vector is anomalous. And you make the same point at a later time, I forget which paper it is, you look again -- this was at Hong Kong and Uppsala. Has that difficulty ever ... I mean does it still persist?

H: Well, I don't know; there are no more data; there aren't any more data as far as I know since I made these analyses. The first analysis, the first determination of lunar tidal winds was from Mauritius by Sydney Chapman, and if I remember correctly now, I think there the tidal wind vector goes also the wrong way, goes opposite to what it should in the Northern Hemisphere, which was nice, but it's wrong in both hemispheres and as far as I know there is no explanation yet.

[Postscript 5-14 by B.H: The turning of the lunar tidal wind is in the direction opposite to that predicted by the theory at Mauritius as determined by Chapman and at other stations as determined in papers 113 and 117. Blamont has suggested an explanation based on the effect of higher modes. But this does not seem convincing considering the small magnitudes of such subharmonics. See also postscript 5-15.]

P: I think that's wonderful. I mean, in the sense that one should never be too overly enamoured of theory, and it's always nice to have the observations suddenly startle you.

H: Yes. There is of course one trouble about all this, that the tidal determination, even though the data are fairly known are still not quite as accurate as you would want, the error circles are still a bit large, and one should have more years of data. The only tidal wind determination which is satisfactory, I think, is either the south component or the west component, I forget which, in Hong Kong, I think, that comes out right. But of course from just one you can't tell very much, so you could say that maybe this misbehavior of the tidal wind vector is due to the large errors, but it would seem peculiar that it should be wrong in both hemispheres and all 8 stations. But it certainly is something which one should still look into ... if one could get more data.

[Postscript 5-15 by B.H.: It is the eastward component of the L_2 wind in Hong Kong for which the probable error is less than one third of the tidal wind amplitude, the usually accepted requirement for a determination considered statistically satisfactory. In paper 117 the determinations were made not only using all days with winds equal to or less than 10 m s^{-1}, but also if the upper limit is 5 m s^{-1}. In this case the L_2 vector turns counterclockwise at both stations as it should. But the determinations cannot be considered statistically satisfactory; worse luck!]

P: One fourteen and 116 both deal with L2 of p. The one at 10 new stations so to speak in Australia, and then 116, 1969 is the authoritative paper on the global L2 of p. And that still is the principal point of reference on L2 of p.

H: The other one incidentally, the first of these two papers, the one with the tides in Australia, well, I had at that time a colleague whom I had met in Alaska who went to Australia so that I felt confident if I had trouble there with the meteorological service, I think they call it the meteorological bureau, that -- I mean if they didn't understand what I wanted, I could write to him. But everything went very smoothly and they sent me the data. Of course, there is really nothing to be said to the other paper. The global distribution, the annual variation, that as you say ...

P: That's the principal point of reference to anyone who wants to know what L2 of p is in the atmosphere.

H: Of course it could probably be improved by doing ... if you had twice as many stations. I might say incidentally, that what I did in this -- what Mrs. Cowley and I did in this paper -- is we plotted the two harmonic coefficients for each station and drew lines of equal harmonic coefficient -- isopleths -- and then read values of the grid points, latitude and longitude grid points, and made the analysis from this because it was easy to program that.

P: Made the analysis of what?

H: Well the harmonic and ...

P: Oh, the harmonic analysis. ...

H: Yea, yea. No, but I was going to add, now you could argue that it might be better to go back to the original data, that is take these two hundred or whatever it was, 50 stations, and make the harmonic analysis for these 150 stations or so ...

P: Spherical-harmonic analysis.

H: Spherical-harmonic analysis. But well it seemed to us that we wanted some smoothing in there. Now, fortunately, there can really not be too much arguing which one is better because somebody else used my data exactly the same data and did make the spherical-harmonic analysis from the original stations.

P: Who did that?

H: His name was Stuart Malin, who is a geomagnetician, whom I knew quite well because he spent quite a while at NCAR, too, and he did that incidentally with a spherical-harmonic analysis of geomagnetic data, and he got very much the same results as we did.

P: Very good. Excellent. Paper 117, 1969, same time about, L2 of V, we were talking about that a moment ago actually at Hong Kong and Uppsala, and again this gives this anomalous rotation ...

H: Yes, and Hong Kong, one of the two components, wind components, at Hong Kong, is the only one which has really what Chapman, according to Chapman's definition, is an acceptably small error for the amplitude.

P: Okay. One nineteen is this very neat demonstration of the lunar tide, you might say a visual demonstration, and I took the opportunity

to read this more carefully than I previously had. And, I was going to ask you, who actually plotted all these points?

H: Oh, the computer.

P: Ah, that's a computer plot.

H: Yes, that is a computer plot -- and how that is done, you have to ask Ann Cowley.

...

H: ... Incidentally, I indicate here that Bartels has described that method of determination first, and also Bartels has discussed in some place the various methods to find periods and objects to some of them and he has actually done something similar for geomagnetic data at one time to demonstrate that if there is something in the data, some period or something, he can occassionally find it by very unsophisticated methods, provided you have enough data.

P: I'm not sure of this, but you use these "mu" numbers, which is ... is this what Bartels devised or is that somebody else who did that?

H: It's either Bartels or Chapman.

P: I think, I might be mistaken here, but I think that concept is very similar -- in which you divide the lunar month in a certain way -- is very similar to the Darwinian method of analyzing the lunar tide on the basis of solar data, where you make judicious jumps in the choice of solar hours in order to mimic the lunar period. Number 122 is the principal ... is the now authority on S1 and S2 of p, the global analysis of S1 and S2 of p.

H: Yes. One motivation of course for this paper was, as far as the solar diurnal tide is concerned, one motivation as I say was that the earlier one neglected, since I didn't know about it, the negative ... Hough functions with negative depths, and here, some of these are in here, I think.

P: Yes, I see. Very good. Then you had a review paper, number 125 in the McGraw Hill Encyclopedia of Science and Technology, it's a brief review, but you mention something that I find interesting there, namely that the lunar O1 and N2 tides have been detected ... atmospheric tides. That's something I wasn't aware of.

H: Oh, yes. They have. In fact, that I think is one thing which somebody ought to do, even going back to some of the data which we

used, which may still be available at NCAR on magnetic tape, and determine some of these. Though most of the data series are still a bit short to get good determinations.

P: Do you think one could do a global analysis? Or just some of the better stations, probably.

H: Yea, I think just some of the better stations. I don't imagine that global analysis would be ...

P: Now we have paper 128, which is another one of those where you did L2 together with four S's, this time at two Swiss stations, chosen to have quite different elevations, and you found that there was no significant change of amplitude of L2 with elevation.

H: No. No, in fact this is true at apparently most of the stations ... there is a summary I remember now, as I see here, and it just seems that at least in the low troposphere you can't detect any change. By the way, this paper was again to a certain extent a paper of opportunity because when I was in Switzerland with Hans Dütsch for a term -- a summer term -- I made contact with some of the people at the Swiss meteorological office and they kindly sent me all the data for the two stations.

P: Is Dütsch the ozone man?

H: Yes, that's the ozone Dütsch.

P: The last one in our sequence is very neatly closing the circle, so to speak because it deals with the same subject that the very first one did, namely tides in the free atmosphere in the Caribbean area, this done jointly with Herbert Riehl. This was an analysis of pressure height data available from the 3-hourly radiosonde ascents during GATE.

H: Yes.

P: And, do you have some comment you would like to make?

H: Yes I have a few comments. One of them is that unfortunately the GATE data really were not very good for these determinations.

P: Oh?

H: No, I wish they had had more regular data, say. At any rate, I would say also that altogether it extends I think over 2 or 3 months and that really isn't enough if you want to get a good grip on the errors. And, in addition to that, very often data were missing. What

we finally did -- both Herb Riehl and I -- we used some mean values which had been determined by ... let's see if it says in here ... I can't think of his name, it's awful that I don't remember names anymore ... in Seattle ...

P: Dick Reed

H: ... Dick Reed, yes, thank you ... Dick Reed, and of course I would imagine that if it's done by Dick Reed, it's as good as it can possibly be, but even so, I'm not too happy. And another thing is I wish they had taken the data not ... the observations ... not every 3 hours only, but maybe at least every 2 hours because I would dearly have loved to get some grip on the 8-hourly pressure oscillation and its distribution with altitude, but if you have just 3 observations -- observations every 3 hours -- an 8-hour period just barely ... well there are just 3 observations in it. Still I might try someday to speculate -- I still have the data -- to speculate about what the 8-hourly pressure wave does.

P: Yes. Well, one thing that certainly, in spite of the uncertainties, came out clearly is that S2 of p amplitude is relatively insensitive to height variations, but S1 of p increases markedly.

H: Yes. I don't know what S3 of p would do.

P: And also the phase of S2 is almost independent of height. Bernhard, in the time that remains, not very much, but it would give us enough time to talk a little bit about the ... to go back now, and take a historical view of the subject a little bit. Of course we're talking about the subject of atmospheric tides that really originated, well, I guess you might say almost 200 years ago, because Laplace's work ... well that's not quite true. Laplace's work on the tides did originate about 200 years ago, but his work on atmospheric tides was not until a bit later, I think until about 1825, something like that.

H: Of course, on the other hand, I think when the first person, I forget who it was, when he took his barometer into tropical latitudes, he must have discovered the semidiurnal pressure oscillation.

P: Yes, I suppose so. Then, of course, we come through a long sequence of people beginning with Kelvin, who tried to understand S2 and L2, particularly the S1 and S2 relationship, and there's no need to go through all the details of this evolution through Margules and Hough and Lamb, Chapman, Taylor, Pekeris, and so on. What I'm particularly interested in, however, is talking about the period beginning with Siebert's thesis and going for about another 10 or 12 years through the time when the negative equivalent depths were

recognized, and the implications of that. Now we've already discussed the Siebert thesis.

H: Yes.

P: I notice that at about the same time there was a paper by Sen and White. I guess I've never looked at that paper and I'm not sure what it was they said except that apparently they suggested the possible importance of insolation in the free atmosphere.

H: Yea, I think that's right.

P: Did they do anything quantitative there?

H: I don't remember any more either. It's quite a long time that I read this paper and I really found it very confused. I mean, not confusing, but it appeared to me as if the authors were very confused.

P: Then we have, as we previously discussed, Siebert's quantitative assessment both of H_2O and ozone, now we're talking here about S_2 and that more or less settled the problem of S_2 within reasonable limits. There are details, of course, that still interest people, but I think the ...

H: Well, of course about the same time there was also this Australian, the two Australian people, Butler and Small.

P: Small, yes. Yes, but there I don't think that they ...

H: They came a bit later, didn't they?

P: Well, I think what they did was to improve on Siebert's ozone estimate. They used a better temperature distribution than he had and got a better ozone result. They also pointed out, Butler and Small, the implications of the short vertical wavelengths for S1, and the small equivalent depths. But, the particular period in the few minutes that remain that I would like you to comment on is 1965 to 6 and 7. Now you had pointed out in 1965 that the small equivalent depths with their equatorial-trapping Hough functions were unfavorable for S1 ... for a global response of S1. And as you mention yourself, you just barely missed the negative equivalent depths in that paper. That was in 1965. Now in 1966, Kato pointed out the negative equivalent depths and their relationships to the problem and independently, I gather, in the same year, Lindzen did the same thing.

H: Yes, I think they were quite independent. As I mentioned before, I had, I probably had seen Kato's paper, but apparently I didn't read it because the first time that I heard about the negative equivalent depths was from Dick Lindzen.

P: But Kato was after your paper of 19 ... your paper was in 1965, Kato was 1966.

H: Yea, well I mean that I should have, after my paper came out, should probably have seen Kato's reference to negative depths, but I only found ... heard about it the first time verbally from the Dick Lindzen.

P: Now, Flattery was also working about the same time although he didn't actually publish his thesis until 1967. But he was in touch with you. It must have been in 1966 or maybe even 1965, and of course, Lindzen was in touch with you too. I think he ... wasn't he at NCAR at that time?

H: Yes.

P: Now, to what extent did you influence these people? I'm talking about Lindzen and Flattery, in their work on this subject of Hough functions?

H: Well, I don't know that I influenced them very much except there was a particular point, one of the equivalent depths for the semidiurnal oscillation was ... is about 690 meters, I think.

P: For S1.

H: For S1, yes. And I got that value and Flattery later told me he got this value too. On the other hand, Siebert got a value--not Siebert, Kertz, another former pupil of Bartels, got a value which I don't remember now any more, I think was 720 or something. It seems a trivial difference, but the difference actually is large enough in the calculations to make it ... to be substantial. And, well, I wrote to Siebert ... wrote to Kertz about it and Kertz wrote back and said well he had checked his calculation, but he still got that value. I checked my calculation and I wrote back to Kertz. I must say also, I knew Kertz quite well because he had been in New York for a while and we were good friends, so we could without any rancor or anything, correspond about that and then when I wrote him the second time, Kertz wrote back and said well, he finally found it. There was no mistake in his calculations, but he had somewhere, from one page to another only a copying error, and of course, that is a thing which you never think you make a mistake. I know that with my checkbook, when I copy from one side to another ...

P: What was his corrected value?

H: Well, it was 690 or whatever, both Flattery and I had gotten. Flattery was apparently quite perturbed about that too at one time

because he didn't get the value which Kertz mentions in ... or which is I think mentioned in Siebert's article in 1961 in ... but computed by Kertz.

P: Oh, yes. But coming back to your contacts with Lindzen and Flattery, what ... certainly you must have discussed the Hough function question with them, and I'm just wondering how did this negative equivalent depth pop up? You were there at the time, I suppose ...

H: Yea, well, I don't really know. I mean it just popped up as far as I remember.

P: Well, then of course, that event, which took place in 1966 enabled the final link so to speak to be established to explain the S1 response. Do you have any further reflections on the historical evolution of the subject down to the present time. I'm talking now about ...

H: Well, incidentally, speaking about the Hough function, there is of course this other business about the completeness of the system of Hough functions. You know ... you have a translation of this paper by Holl, another pupil of Bartels?

P: No.

H: Oh, I must give you a copy of that. It has only appeared in the proceedings of the Göttingen Academy of Sciences and I translated it into English because some people wanted it. I thought I must have ...

P: Recently?

H: Well, no it must have been, I don't know how long ago.

P: Ten years ago?

H: Not ten years, but within the last 10 years.

P: So, what did he do?

H: Well, he showed that the system in fact is complete.

P: With the negative depths.

H: With the negative depths, yes. And I think it was Dick Lindzen who said one has to take a sine function, in addition to that.

P: Yea, I vaguely remember that.

H: Well, I don't really remember the thing any more either, it just came back now, but I must get you ... I still have copies.

P: You mean that one of the solutions is not what we would call a Hough function, but is simply a sine.

H: Yea, well that is what Dick Lindzen claimed, but actually this particular sine function is a sine function only I think under a certain condition I think ... I just don't remember.

P: Well getting back ... do you have any further historical observations?

H: No.

P: Getting back, then to the subject of the tides themselves, what ... would you be able to say on the spur of the moment where you feel the remaining important questions lie in regard to -- let's talk about S1 of p, S2 of p, and L2 of p. Take those three. What would you like to see done.

H: Well, what I would like to see done is, one of the things is, what we discussed before, namely the peculiar behavior of the lunar tidal wind vector. I would well, probably one could just sit down and try to explain theoretically why this happens, one could however ... one should however also get more stations with long wind records so that one really can make good determination statistically ... Now, there are some ... there is an explanation or a paper by Blamont, I think he pronounces the name, a Frenchman, who describes under which conditions the wind vector can turn -- the tidal vector can turn the other way around. But that applies only under certain conditions which would not be true, certainly not for the largest waves or whatever they call the tidal modes. It's pretty hazy in my mind, I must admit now. But at any rate, that would be one of the things.

P: Okay.

H: Of course another very good thing would be to make more observations of the -- determinations of the tidal ...

[Postscript 5-16 by B.H.: Continuing the enumeration of things to be done in tidal studies, one would have to mention in the first place more upper air data so that something approaching global coverage can be attempted. Such data would permit a more precise check on tidal theory. As far as the lunar tide is concerned, direct measurements of the lunar tidal wind in the ionosphere would be useful for determinations of the electric state of these layers and would contribute to our knowledge of frictional effects.

Even the surface distribution of the various tidal oscillations is not as well known as might appear, because of the lack of sufficient data in the Southern Hemisphere.

For the solar tidal oscillations a study of the 8-hourly oscillation might be of special interest. It is presumably caused by the 8-hourly temperature oscillation which has a pronounced seasonal variation caused by the changing length of day and night. With sufficiently long series of pressure (or wind) and temperature data which enable the 8-hourly oscillations to be determined reliably for small fractions of a year, say one or two weeks, it would be possible to study empirically the atmospheric response to the thermal forcing.

In the study of the lunar atmospheric tide almost only the main term M_2 (in Darwin's notation) has so far been investigated. It would be interesting to determine also the oscillations corresponding to the next largest term in the tidal potential, namely N_2, produced by the ellipticity of the moon's orbit, and O_1, which is approximately lunar diurnal. Up to now these terms have been determined only for very few stations. Such determinations may give further clues to the response of the atmosphere to gravitational excitations.]

[End of side 2, tape 5]

TAPE 6
16 May 1983

Platzman: This is the sixth interview of Bernhard Haurwitz, being held today on Monday, May 16, 1983. Bernhard, the first topic for today is solar variability in the atmosphere. Paper 29 is one that I did not locate either at NCAR or in your collection, but I have a suspicion -- correct me if I am wrong -- that it may be a preview of the bigger paper that was published as one of the Harvard Blue Hill studies.

Haurwitz: That's right, it is exactly that. It's just a preview.

P: And that bigger paper was published a few years later, in 1941, and that's paper 50 which is the next on our agenda here.

H: Yes.

P: Now that's a very substantial study by Charles Brooks and a number of other collaborators. The title of it is "Eclipse meteorology with special reference to the total solar eclipse of August 31, 1932." And although I was aware of the existence of that work in a general way, I guess I did not until this opportunity look at it in detail, and I found it really quite fascinating. It raises all kinds of interesting questions in dynamics. Your contribution to that, I gather, was in dealing with observations of temperature ... surface observations of temperature, pressure, wind, and humidity. And of some of the inferences that can be drawn, dynamically, from that information.

H: Yes, except my recollection is that really nothing much came out of that. I had the impression -- well I still have the impression -- that the experiment which nature performs in this case is -- you can put it two ways -- either too short in time or it's extended over too small an area. The sun's shadow moves rather fast, and doesn't cover for a given moment a large area.

P: Do you recall the duration of totality in that eclipse of 32?

H: No, I don't really know how long it was. I came here of course after the eclipse. The eclipse was in the summer of 1932 and I came in fall. And so ... well, it probably says somewhere in the publication how long the totality lasted at different places, but I haven't any recollection anymore.

P: It's a matter of minutes, isn't it?

H: Yes, it certainly is only a matter of minutes.

P: In a way, I guess, one might say from a dynamical point of view it gives rise to a kind of an adjustment problem. That is to say, you are making very rapid changes in the temperature field; whether they can be adjusted hydrostatically so quickly, I'm not sure. In other words, whether that corresponds to a change in the mass field. But that ... did you subsequently get involved in any other studies of eclipse meteorology?

H: No, I never ... In fact, I wasn't very sanguine -- if that's the word -- even at that time about it, to be cynical. For one thing again, of course, if I remember correctly, if I am not very much mistaken, two of the co-authors of that particular paper were also Harry Wexler and Jerome Namias, who at the time when the investigation went on were still students at MIT, and one of the great merits of that particular work was that it contributed to their support as students. And perhaps also to my support here as a new arrival.

P: Was Brooks the principal impetus?

H: Yes. He was certainly the principal investigator.

P: Was that a special interest of his -- apparently it was -- or was it just a target of opportunity, so to speak?

H: I think it was a target of opportunity. I should also say -- now that we talk about it -- it may not have been just Brooks, who was the prime mover, but it may have been H. H. Clayton, even though I don't think Clayton officially was involved in it, but at that time I was simply too new over here that I felt I could ask if Clayton was interested or if it was Brooks.

P: Right.

H: Brooks certainly was interested in it, but he may have been put onto it by Clayton. I think Clayton at earlier times has discussed effects of solar eclipses, though at this time I wouldn't recall any more references.

P: Brooks, was he not director of the Blue Hill Observatory at that time, and they had an observation program going into which these eclipse studies seemed to fit rather well, I gather. Or is that not so?

H: Which observations program do you mean?

P: Well, I am speaking generally. But perhaps the eclipse data didn't come from Blue Hill.

H: No, in fact, the zone of totality I don't think ever went through Blue Hill. I'm open to corrections there too. My recollection now is that most of the observations were done in New Hampshire, and that people like Brooks went up there.

P: Yes, I recall now the maps. Actually, this subject fits quite naturally into the next paper, number 58, called "Relations between solar activity and the lower atmosphere," published in 1946, and there your interest was again in an aspect of solar activity, namely this time not an eclipse, but a solar flare and the possible effect that this could have on the thermal conditions in the ozonosphere. That is a theme that you continued for some years in further work.

H: Yes, it crept up intermittently. It started at that time because I was exposed especially to H. H. Clayton. When I say exposed I don't mean that in any derogatory way. I knew, of course, for one thing who Clayton was in the sense that I knew that he had made important contributions to meteorology apart from his later work on long-range forecasting and, in particular, on studying possible relations between solar variability and the atmosphere or weather in particular. But I quite often, when I encountered Clayton, who came up to Blue Hill Observatory -- and I remember also having gone to his house a number of times -- we talked about it and my objection always was to all this that there was no obvious -- or even not so obvious -- physical reason for such a relationship between variability in solar radiation and the atmosphere, and so I started thinking about it and I started making sort of a best case estimate which is really quantitatively described in this particular paper we are discussing at the moment, namely, that some absorption of solar radiation in the ultraviolet might take place in the ozonosphere and heat the ozonosphere and then lead therefore to some pressure variations there which then, even though the pressure variations in terms of surface pressure are very small, could be observed at the surface of the earth. Well, actually, what I proceeded to prove is -- prove in quotation marks -- that this would be quite impossible to perceive at the ground, but much to my surprise when I made all these most favorable estimates it turned out that there might in fact be some effect on the ground. And this is how that paper came about at that time. I also tried at that time then to find if there was any possibility to see in atmospheric behavior in the lower atmosphere -- I think I never mentioned that in this paper here or in another paper -- I tried to find any observations which might indicate that solar effects, solar flares or something, might produce tropospheric effects. I looked at that time in what was then known as the zonal index, and in fact I found that whenever there was noticeable change in the zonal index during the half year for which I had data readily available, I could find just before that either some solar flare or, more likely, some magnetic disturbance which of course can be interpreted as a solar effect, but the whole thing seemed too

vague to really pursue it, so I gave it up again and we might come then to the other paper which really is an extended version of this paper here, namely 67.

[Postscript 6-1 by B.H.: Correction: Paper 58 does not give a quantitative description of a mechanism for a connection between solar variability and the lower atmosphere, but only a qualitative outline. A quantitative account is given in paper 67, the next paper discussed.]

P: This one here. "Solar activity, the ozone layer, and the lower atmosphere."

H: Yes, there was a meeting at the Harvard Astronomical Observatory -- 50 or 100 year, probably was hundred year, 1848 -- a jubilee celebration. I could look it up because I have the volume myself actually, and I was asked to give a paper in one of their sessions, and this is what I gave then. I just pursued the idea which I had developed originally, pursued it a bit more and gave a bit more quantitative estimates. But I don't think there is any point in going into detail of that here.

[Postscript 6-2 by B.H.: Paper 67 was indeed presented at the Harvard Observatory Centennial.]

P: Well, I think in the first paper, number 58, your discussion was largely qualitative, although the mechanism was clearly mapped out.

H: Yes.

P: And then in this, number 67, published just a short time later, as you said a moment ago, you made this whole picture much more quantitative. The idea there was that you proposed to model the effect of the flare as an impulsive heating of the ozone layer and the consequences of this ... well it was impulsive in the sense at least that it was fast enough to prevent an adjustment of the mass field, but slow enough to provide ... I'm sorry, it was fast enough to prevent an adjustment of the wind field, but slow enough to provide an adjustment of the mass field so that the initial condition you assumed was that ...

H: I assumed, if I remember correctly now, that there was a ring of high pressure formed at some level in the ozone layer, around 30 km, I think it was, and then I made some very simple numerical, or dynamical estimates how that would affect the motion in the upper atmosphere and consequently with the redistribution of mass could make itself felt at the ground. I don't think anybody has ever pursued that very much, except Julie London who at some later time took some of my assumptions

about the effect of heating by a solar flare and showed that they are wildly exaggerated ... well, I shouldn't say wildly exaggerated, but that they are too high.

P: This is exactly the question I was going to ask you, about the photochemistry which is not covered in your paper, and I wondered whether anyone had looked at that.

H: Well, somebody had looked at it before, namely Maris and Hulburt. They had taken some figures and had written a paper in 1929 which is referred to in both of these papers of mine which we are discussing here. But at that time, of course, the data about the ozone layer were not very well known yet. I had just taken over their figures. And remember that my original idea was even with the very best estimates I wanted to show that there couldn't be anything at the ground and it turned out that there could. As a matter of fact, I might also mention as an aside, because that would never be known otherwise, when I talked about this whole idea about the way it is presented in this first paper, qualitatively, when I talked about that at Blue Hill Observatory one time at a semiformal presentation, Clayton was there and he really didn't like it at all. I was terribly disappointed because I thought I had given the first time a rational physical theory.

P: What was the reason for his ...

H: I don't really remember any more.

P: Maybe he didn't understand it.

H: No, that's too unkind. I don't think so. He was pretty smart.

P: Well, then would it be fair to say that if your estimate of the temperature response to the solar flare had held up, then there would have been a semblance of a possibility for an observable effect? But in fact as I understand you to say Julie London later showed the temperature effect which you assume was too large.

H: Yes. Well, as you say, if the estimate which I took had been reasonable then something ... one might presumably assume something could be observed at the ground. But it didn't turn out to be the case. As a matter of fact in late years with some of the strong solar flares there have been observational reports of large temperature, certainly numerical estimates, of equally high or even higher temperature variations with solar flares, not at layers of 30 km but at about 35 km. But I don't know, I have never bothered again to make an estimate to see whether that would make itself felt at the ground.

[Postscript 6-3 by B.H.: The mechanism leading from the heating in the upper stratosphere to changes at or near the ground would have to be worked out in considerably more detail than by me in paper 67. The estimates of temperature changes made by London were made quite a long time ago and would also have to be repeated on the basis of the much greater present knowledge of atmospheric photochemistry.]

P: That's very interesting. Paper number 74, Bernhard, I did not locate and I wonder what is in that symposium that's called "The influence of solar phenomena on the weather," published in the New York Academy of Sciences.

H: Well, I don't really have that anymore either. I have a "missing" here on my list.

P: Yes, it isn't in here and I wasn't able to locate it.

H: Yes, and I really don't remember anything much any more about this whole symposium.

P: Perhaps it's a summary of what you did in 1948.

H: It could be, but I have a suspicion that it was just probably a general introduction. Well, usually, when ... if and when I go to such a meeting discussing solar weather relationships I say that there is not much point in doing these statistical investigations ... well, probably I shouldn't talk here about my ideas.

P: Why not? This is just the place for it.

H: There shouldn't be just an attempt to find some possible correlation between some solar events and the lower atmosphere, but one should first have a physical idea of what to look for. I once saw that very well and concisely expressed by McNish, if you remember the magnetician. He was a pretty well known man in the department of terrestrial magnetism of the Carnegie Institution. McNish. He must still be alive because he certainly cannot be much older than I. At any rate, he talked in general about periodicities and he pointed out that in general when people look for periodicities they do not look for a given periodicity, but they just try all sorts of things. Now he finds out if the probability that there is one particular periodicity, say of about 20 days, if the probability of this is small, say P1, then the probability that you don't find anything is very large, 1 - P1. Now if you look for the next periodicity and it has a probability of existence P2, then the probability that you don't find this one is 1 - P2. And well, say if you have 100 then the probability that you won't find anything is the product -- that you won't find anything -- is (1 - P1) x (1 - P2), etc. Now, since P1 is

very small -- the P's are very small -- this is approximately 1 - P1 - P2, etc. So for instance, if each P is of the order one in a hundred and you look for a hundred periodicities in your work, it's highly likely that you will eventually find something. Of course it's just a mathematical statement of a triviality. But this is of course what people do in solar weather relationships. From time to time, just by accident, you find something, like in the case of sunspot variability and the lake level of Lake Victoria. I may have talked about something like that at this particular symposium. I was at that time of course at New York and I was a member of the New York Academy of Sciences.

P: Did you go to their meetings?

H: Well, yes, in fact for a while we had regular meetings of the meteorology and oceanography groups together, regular monthly meetings.

P: I know quite a number of things were published on both meteorology and oceanography in those days. I am not sure whether that practice continues today.

H: I don't know. I just didn't stay a member when I left New York.

P: Number 88, published in 1957, is a somewhat different tack dealing with ... the subject is "Solar activity and atmospheric tides" and the question there is whether any ... taking even the very best data available on S2 and L2, whether there is any information contained in that data that connects it with sunspots.

H: I remember that quite well. The people, the collaborators in this were Julius London and Gloria Maria Sepulveda, who was then ... she had I think at that time finished her Master's Degree, but she was working on a project still at NYU -- she is, of course, the present Mrs. Williamson at NCAR -- and Manfred Siebert, he was a German who had just gotten his Doctor's degree with Bartels at the University of Göttingen and who is now as a matter of fact the Professor for Geophysics in Göttingen. We were one time just talking, we were all interested in one way or another in atmospheric tides, and since apparently the upper atmosphere is greatly dependent, or rather the tides seem to be dependent in their resonance properties on the upper atmosphere, we thought it might be not entirely unfeasible that one could find some relation between solar variability and the high atmosphere, the upper atmosphere. By that we had in mind something above 40, 50 or 60 km. We had some data about the lunar tidal variation from year to year from a publication by Bartels which is the publication which was discussed before, where he publishes separate values for each of forty years, I think, and also data for Batavia for

S2 -- for the solar semidiurnal oscillation -- and well we just correlated that with the sunspot number, and I don't know how much you want to go into ...

P: This is wonderful.

H: Okay, so we sat down -- I'm pretty sure the way that worked since Mrs. Williamson, Gloria Sepulveda was the low woman, I guess I have to say, on the totem pole, she did the work -- it wasn't really too much work and she computed the correlation coefficient, especially for the lunar tide, I remember that in particular now, and it came out to be just on the borderline.

P: Excuse me, the correlation between let's say the L2 amplitude on a yearly basis with the sunspot number index, whatever that ...?

H: Yes. As I say, it came out just on the borderline if you compute the standard or probable error -- whatever you can compute -- of the correlation coefficient. Partly because you couldn't make full use of the data I felt a bit unsatisfied with it. We simply didn't have handy the sunspot numbers for all the 40 years. But then I remembered a friend of mine in Germany who had spent all his life -- an astronomer, Gleissberg -- spent all his life practically studying the frequency of sunspots and he had written a book of which he had given me a complimentary copy, of this book, which I had right in my office there, and sure enough, the three missing years, or whatever it was, of sunspot numbers were available. So I just computed it one morning myself, after all that wasn't much additional work, and well it turned out that the correlation became insignificant, statistically insignificant, with these additional data and then we decided to send this, even though it was of course a negative result, we sent that to the Journal of Geophysical Research. At that time Tuve, Merle Tuve, was the editor and I was very pleased -- I don't know, it probably does mean more to people who know Tuve somewhat well, Tuve could be quite irascible, I think is the word -- but I was very pleased to get a note back from Tuve that he was glad to accept this note of ours and he thought it was really excellent or something like that.

[Postscript 6-4 by B.H.: One of our reasons for publishing paper 88 was the strong conviction that especially in so controversial a subject as solar-weather relations it is important also to report negative results.]

P: Does that mean that he was a sceptic on this subject ... it fitted into his scheme of things?

H: Yes, I'm sure.

P: But, now, Bernhard, in looking at this paper that you've been talking about, I was struck by this figure.

H: Yes, that looks quite impressive.

P: It looks very impressive. Now that's a diagram showing the actual data. That is, I think that's L2, isn't it, at Batavia for the 40 year ... ?

H: No, I think that is S2.

P: Is that S2 ... beg your pardon. And with the sunspot number curve simultaneously plotted and I must say that looking at that casually one would get the impression that there is a strong correlation with a phase shift. Now I believe you did look at phase shifts as well, did you not?

H: Yes. We did look at them and the trouble is at that moment I don't recall ... I don't really know what we did to prove that it didn't really mean anything.

P: The trouble is that with a phase shift ... now the order of magnitude of that phase shift looks to me to be say 10 years.

H: Yes.

P: How one could devise any physical basis for such a phase shift is rather hard to see, but nevertheless a casual look at those curves would give one the impression that there is some correlation.

H: Yes, but I would have to read it more carefully to answer it. Now I could read it onto the tape.

[Postscript 6-5 by B.H.: Although Figure 1 of paper 88 shows an apparent lag relation between annual sun spot number and S_2 amplitude, with a lag of three years the correlation coefficient is only 0.036 ±0.11, totally insignificant.]

P: Yes, you could.

H: If anybody listens to it later and gets interested he can get the paper out.

[Postscript 6-6 by B.H.: Although I have published no more papers on solar-weather relationships, I have lately been working on this topic again. The results are some reports, book reviews, and talks. My negative opinion has not yet changed!]

P: Now, that's the last of the papers on this particular topic, solar variability in the atmosphere, and the next topic on our agenda today is oceanography, where you made quite a number of quite interesting contributions. Starting with number 68, "The effect of ocean currents on internal waves," and there your point, as I gather, was to establish that just as in the case of external waves there is a decided effect of the underlying flow being added to the propagation speed of the waves. The same thing is true for internal waves and what you considered there was essentially a double layer with shear. It's the internal wave Kelvin/Helmholtz problem, I guess you might say.

[Postscript 6-7 by B.H.: It may be useful to point out that here again in the discussion of paper 68 the "internal" waves are what now is called "interface" waves.]

H: Yes.

P: And ... well perhaps ... did you want to say something about that?

H: No. In a way I found, if I remember correctly, that this was really not very much of a new contribution, that is it's something which anybody interested in the subject can easily and without much work or much thought derive himself. I had, if I remember now correctly -- of course I may not remember correctly -- but I think I did that at one time when I was in the summer in Woods Hole during my twelve years at New York University and I showed it to somebody there and asked him what he thought about it and he thought, well -- that was somebody who was not a theoretician, by the way -- he thought it might be quite useful to have that somewhere. The reason why I asked whoever it was -- I forgot who -- but the reason was that I had been asked to write something for the Sverdrup Festschrift -- I think it was Sverdrup's 60th birthday -- and that was really the most readily available piece of research -- if you can call it research -- which I had and this is how it came about.

...

P: ... Now, number 71 continues the model of a double layer, this time with rotation, and there I think your motivation was a little bit different. The title of that ...

H: I have it right here, "Internal waves of tidal character." That was in the Transactions of the American Geophysical Union.

P: And there your concern was with internal tidal-type waves and you pointed out that it had often been said that since the periods of

interface waves or waves in a stratified medium were very much longer than external waves, the chances for any kind of resonant coupling with the tide were remote. But you pointed out that statement does not take into account the effect of the earth's rotation.

H: Yes, and as a matter of fact one of the people who had said that -- what you mentioned, what I pointed out to be incorrect -- one of the people, most prominent people was Defant, Albert Defant in some discussions of observations from the Meteor expedition, and shortly after I had mailed my paper in to the American Geophysical Union for publication and it had been accepted, Defant came to this country and in fact he also came to New York University -- in fact, I remember I even picked him up in Providence where he had visited Ray Montgomery and drove him to New York -- but anyway, he came to New York and we talked. I told him about this paper and he said ... and I told him that he had been wrong. After all I had now arrived and I could say that even to a central-European professor who was older than I, and he said, oh, yes, he had actually just discovered the very same thing a few months ago, I think, and he had also sent off a paper ... I forgot now where ... and at any rate so we were in full agreement. Perhaps as an interesting sideline I might mention here, he said he had in fact discovered that when he had done some work during the war ... that is the Second World War which was at that time of course already about five or more years back, and he had talked about that in fact in Sweden where he also had ... about internal, interface waves of tidal character, of tidal period, and after the war he had seen Deacon, the English oceanographer, who had also been in at the same meeting in Sweden and Deacon asked him how come that the Germans allowed him to talk about it because the interest at least in Germany and for that matter apparently in England too in these internal gravity waves was that at that time the German submarines when they went through the Straits of Gibraltar apparently from time to time encountered very severe interface waves when they went through the Straits, and some of them were what they call in German apparently "spurlos versenkt" -- that is, they disappeared without trace.

P: Good heavens, I didn't know that.

H: Yeah, I didn't know that until Defant told me about it either. A bit embarrassing was ... but Defant apparently never was mad at me. I had tried after I had talked to Defant ... I had tried to get the American Geophysical Union to put a footnote on my paper and Defant was going to the same thing with his. Now I noticed that his footnote was printed. The footnote which I wanted was never printed because apparently it simply was too late. Well, I don't think it really hurt Defant's reputation any!

P: As it turned out, however, and I think the discussion of the succeeding papers will deal with that, even though there is this inertia period limit for these interface waves, still does not help a great deal in exciting any significant amount of tidal energy apparently.

H: No.

P: Number 73 also published in 1950 is concerned with the meandering of the Gulf Stream and that's a joint work with Hans Panofsky in which you suggest that the instability might be modeled as a shearing, lateral shearing instability ... have you ... did you pursue that?

H: No, neither Hans Panofsky nor I ever pursued it any farther.

...

P: ... Number 76 is so far as I know your only venture into what might be called engineering type of hydrodynamics.

H: Yes. Well, that was simply an accident that I came to it. At that time we had some research projects on beach erosion and wave motions with the Beach Erosion Board, and so at that time they sent us ... us means New York University, the Department there ... they sent us most of their publications, and one of these publications dealt with lake level changes in Lake Okeechobee during the passage of a hurricane and I just glanced through that at one time and I saw there the lines of equal lake level and the wind direction and they looked fine to me. After all, I'm a meteorologist and the geostrophic wind blows parallel to the isobars, but then after I turned the page as a matter of fact it just occurred to me now wait a second, this is ridiculous. The wind of course should blow upslope to the lake, so then I started thinking about it ... I don't know how much I should go into this ...

P: Fine.

H: I started thinking about it and decided that what must happen is that as the wind blows it will of course start moving the lake level ... the lake level will adjust itself to the wind motion ... but as the wind turns fairly fast -- in a matter of a few hours -- the wind will change and the lake level ... the position of the "setup" they call it ... I don't know if you know that ... the "setup" cannot change as fast as the wind with the passage of the hurricane because it needs some water transport, so I sat down and wrote down the linearized equations for that and what the period should be and it came out quite reasonable. As a matter of fact, many years later -- this was in 1950, and one time at NCAR, not so very long ago so it

must have been about 20 years later -- somebody apparently took the same case of Lake Okeechobee and made a numerical calculation of that and it came out very well too, of course, but with more details than I had. I don't know ... I think I simply assumed that the lake is rectangular because I don't really know ... I could look it up, of course, but I assumed a simple lake shape.

[Postscript 6-8 by B.H.: In paper 76 the model lake assumed for the calculation was indeed rectangular.]

P: How did you come into contact with the Corps of Engineers?

H: Well, the Corps of Engineers does of course ... they do of course a lot of work in beach erosion -- or rather preventing beach erosion, I should say -- and it just so happens that the son of Dean Saville, the Dean of the College of Engineering under whom the Department of Meteorology and Oceanography worked, his son, also Thorndike Saville, was a member of the Corps of Engineers, and I don't know if our relation came that way or if we simply applied first to the Corps of Engineers for support of research, but at any rate then it turned out that the person with whom we had the most contact was naturally young Thorndike as he was referred to because his father was old ... well, not old Thorndike, but Dean.

P: Was this your only venture into what would you call this ... limnology, I guess or ...?

H: Yes, I guess I would call it ... yes that was my only venture into it.

P: In 1955, paper number 82, you gave a brief review of the ocean wave research then going on at NYU. I don't have the copy of that here.

H: I have a copy of it ...

P: Yes, in the other room.

H: Yes. But ... should I say something about ... it was called Report on Ocean Wave Research conducted at New York University.

P: Yes.

H: That is AIOP, that is, I don't really know, that is probably the French expression for the International Association of Physical Oceanography and it's been published in the Proceedings of the meeting which I imagine was in 1955. I should also say that this I'm sure was all work by my two colleagues, Pierson and Neumann who did the

research and they only said since I was the department chairman I should sign it. I said we should all sign it but somehow they didn't want it.

P: I see that chronologically I have passed over paper number 81, but I think that's just as well because that fits very closely with number 91 ... an analysis of ...

[End of side 1, tape 6, beginning of side 2]

P: Paper 81 is an analysis of ... let's see, this is it here ... "The occurrence of internal tides in the ocean" and paper 91 is to some extent also concerned with the same subject and this paper 81 was published in 1954 and paper 91 in 1959. In paper 81 you have a series of temperature data that resulted from four different cruises ... stations having been established in the North Atlantic in the interval February through June 1938 and you look at these data which were essentially depths of selected isotherms available from cross sections drawn from the individual stations ... and you look at these data with a view of trying to answer the question of whether there are any discernable periods, particularly tidal periods. Do you recall what prompted you to get into this, Bernhard?

H: No, I cannot really find definitely. I must certainly have seen the data and that must have started me. In a way, the way of trying to determine these periods, if any, and also to determine whether whatever you find by harmonic analysis -- whether that is really statistically reasonable -- is very similar, in principle at least, to what you do in the determination of tides, especially atmospheric tides. And it must have had something to do with it and well in a way I always felt that when I was at Woods Hole during the summer I should do something in oceanography.

P: I was going to ask you how often you went to Woods Hole in this period.

H: Well, I can tell you that fairly ... during this period when I was at New York University ... I came to New York University in 1947 and summer of 1947 I spent at Woods Hole and then I spent every summer at Woods Hole from 1947 to 1954, that is for eight years altogether, and at that time I also ... At first when I came there I worked what was known as the Wyman project, after Jeffrey Wyman who was I think originally a biologist from Harvard. At any rate he had during the war directed a project which had to do with convection in the Caribbean Sea and after the war he retreated from this project and I was officially put in charge. Of course, I was there only during the summer but at any rate Woods Hole kept me in charge most of the time. The other ... the person who stayed with this project the longest time

was Andrew Bunker -- Andrew F. Bunker, you may know his name -- and for a while Joanne Simpson ... well at that time let's see, she was Joanne Starr ... worked on it too.

[Postscript 6-9 by B.H.: My studies of internal gravity waves were not part of the project which I officially directed at Woods Hole. One of the reasons why I came to study this subject of internal gravity waves was doubtless simply the availability of observations, another one was that few, if any, papers included tests of the statistical reliability of the reported periods. Some of the results obtained in paper 81 had been described earlier in two Technical Reports issued by WHOI as mentioned in paper 81, and paper 81 itself is part of a Festschrift for A. Defant on the occasion of his 70th birthday.]

P: I think it's probably fair to say that the results from those ... that data series were distincly inconclusive. I think one of the interesting features of this study is the importance ... that you emphasize the importance of having a good statistical test of significance and of course you carried this over from your work in atmospheric tides, but the difference is that in the case of atmospheric tides there isn't any question ... there is no doubt whether the period exists in the data, the only question is to establish the error bounds for the estimates. And here the statistical test is of a different sort, namely to try to decide whether their is any significance at all.

[Postscript 6-10 by B.H.: With regard to the comment by G.P. about the same statistical tests being applicable in the present case and to tides in atmosphere and ocean, it is relevant to point out that my few earlier papers on atmospheric tides do not contain any statistical tests and that paper 81 is the first one containing such tests (apart from the WHOI reports on which this paper is partly based).]

H: Yeah, well, since you bring this up you know I had quite a discussion one time with Gerhard Neumann about it, who was of course a collegue for many years of mine at NYU, and I said that this ... these statistical procedures are to establish the reality of the lunar tide in the ocean. And he objected to that. He said, well it's perfectly certain that there must be a lunar tide in the data. And well after quite a bit of argument I said, yes, you are quite right there. What I really am doing is, I am trying to establish whether these data which we have there do actually indicate a lunar tide, and we could finally agree on that.

P: Okay. The problem with what was available to you in this paper number 81 is that the longest data run consisted of 21 tide periods. I assume that's M2 periods, so we are talking about essentially 10 days of ...

H: Yes.

P: And even analyses of surface elevation data for tides are somewhat chancey with that short a record, so it's not surprising that you would have a lot of trouble.

H: Yes, and of course ... one of the explanations ... I always told people, why don't they stay longer but they said, well for one thing of course, it costs quite a lot of money to have a ship out at sea. And secondly they also asked me, have you ever been in the tropics on a ship that is becalmed anyway and standing still? And of course I said no and people were suggesting, well maybe I should be on such a venture sometime and I wouldn't feel so strongly about long series of data.

P: I noticed in the acknowledgments to this particular paper you mentioned Carl Eckart. Was he at Woods Hole at that time?

H: He was at Woods Hole one year. That's the one time I got to know him. And it probably was at this time when I talked to him about the whole problem too.

P: The next paper, the one published in the Rossby volume in 1959, is called -- it has a title that has always appealed to me -- "On the thermal unrest in the ocean," and it's co-authored with Henry Stommel and Walter Munk, and in a sense it's an extension of the previous study you made of the temperature data, but what is new and different here is that here we finally have a long series of records ... of data ... because it comes from two recording thermometers placed on the ocean bottom offshore of Bermuda, one at 50 m and the other at 500 m depth.

H: Well, this was of course the time ... I don't know anymore when the Rossby volume ... you may know that better ... the Rossby volume was planned. It came out ... it was supposed to come out in 1959 ...

P: I think it was ... it did come out in 59, it was planned in '57.

H: Aha. Well, it probably was that either in '57 or '58 -- or maybe earlier -- I had worked on these things, on these data and then it turned out that Henry Stommel worked on something similar and I don't know if ... Walter Munk of course was at one time at Woods Hole too and I imagine must have been at that time where it turned out that he was also working on something similar ... so we just decided instead of writing three short articles we ... in this case where we are really talking about the same thing, if I remember correctly partly also about the same observations, it is perfectly logical if we get together and even though the contributions really are still fairly

separate they make a perfectly good, perfectly logical combined paper and that's how this came about. The title incidentally I think was due to Henry Stommel "The thermal unrest ... ".

[Postscript 6-11 by B.H.: With regard to paper 91 I am not sure now that I worked on it in 1957 or 1958, at least not at Woods Hole, because in 1957 I spent the summer at the Observatory on Sacramento Peak, New Mexico, and in 1958 at Boulder, Colorado. I may, of course, have been at Woods Hole during these years for a shorter time.]

P: You ... in the first part of the paper, you apply the same statistical ... you approach the problem from the side of harmonic analysis with the same kind of statistical testing that you used in the previous paper we talked about, and I think ... my impression is that in spite of the much greater volume of data and the longer record, the outcome was still very inconclusive.

H: I really don't remember now, I would ... I'm trying now going through here to find the table ... it may simply take too long ...

P: That's alright, take your time. I think that's probably it there. I think you did get some fairly small probabilities, but it seemed to me that you still did not find them acceptable.

H: I don't really know what I say. I mean looking at it I would say that they don't look very good.

P: I think there are some hints there of something like a 23-hour period that you suggest could be connected with the inertia period.

H: Yes, but the probability of that is quite ... that it appears in random data is still pretty large. I also point out here that it becomes smaller if I take just the first six days, but that statistically of course is ... that is just not cricket.

[Postscript 6-12 by B.H.: As G.P. says, I did not find the statistical results to be acceptable evidence of the periods discussed, as shown by a more leisurely reading of the paper after the interview.]

P: Then I guess through Walter Munk's interest in time series the second part of the paper deals with the spectral analysis of the same data.

H: Yeah, I think this is his contribution, really.

P: And the surprising thing there is that even from that standpoint -- looking at the whole spectrum without any preconception

as to what you are going to find -- there still is no conclusive evidence, or at least the indications are very slight for, for instance the M2 period. And this underscores the difficulty of the problem that still persists today as far as I know, of trying to understand to what extent tidal energy does find its way into the baroclinic motions of the ocean. An interesting aspect of the spectral analysis is that it comes out with the first indication -- I think it's the first indication as far as I know -- of the spectrum of internal waves at high frequencies. But was there any other comment that you had about that, Bernhard?

H: No.

P: Papers ... that is the last of the papers on oceanographic topics. The next topic is ... comes under the umbrella of "Miscellaneous" and some of these papers I did not find, but let's just go through them in chronological order. Number 6 in 1930 is one ... you previously mentioned to me having done this little study about the variation of gravity in the earth's interior. What was the outcome of that?

H: The outcome, well, I have first to say what the problem was. I just happened to read in -- I guess it was Gerland's Beiträge -- yes, it must have been Gerland's Beiträge -- in a paper by Beno Gutenberg, where he computed the distribution of the acceleration of gravity as a function of the distance from the center of the earth under different assumptions about the density, and I was struck by the fact that in all these cases the maximum of the acceleration of gravity was, I think it was 900 or so km, so I thought it should be possible to start out not with a particular preconceived assumption about the density but see what you would get if you make just a very general assumption. If I remember, yes my assumption was that the density as you go from the surface to the interior of the earth never in any place becomes smaller as you go farther, closer to the interior. Of course, I also assumed of course that -- for obvious reasons -- that the density is uniform ... that the density is only a function of the distance from the center of the earth and then ... now I don't know exactly anymore what I found there ... it was something about the ratio of I think the surface density to the mean density, and that the position of the maximum of acceleration of gravity should be at around 900 km depth if the ratio of the ... I guess it was the surface density to the average density ... if that is, well whatever it actually is ... I forgot now ...

P: It's about 3 to 5, yes. Well, is it correct that in any situation where the density increases with depth you are going to get a maximum gravity at some place other than at the surface, or does it have to increase at a sufficiently large rate?

H: I couldn't answer that now. Probably would be, I would say offhand now, it always has to be a little bit below the surface of the earth, but I would really have to look at the paper again. This was my only excursion into solid earth physics.

[Postscript 6-13 by B.H.: My recollection with respect to Gutenberg's paper -- which prompted my paper 6 on the distribution of gravity in the earth's interior -- is faulty (not surprising after more than 50 years!). Although I don't have Gutenberg's paper available, it is clear from my paper 6 that I was interested in Gutenberg's findings that the acceleration of gravity in the uppermost 300 km of the earth is nearly constant under different assumptions about the density, provided the ratio of the density in these surface layers to the mean density of the earth is as 2:3. (I don't know why I referred to a maximum gravity at 900 km depth in the interview.) What I actually showed was that with a spherically symmetrical, but otherwise arbitrary density distribution the acceleration of gravity has a maximum at the surface when the ratio of the density at the surface to the mean density of the earth is 2:3. Since the condition for gravity maximum is that the rate of change of gravity with depth is zero at the maximum, Gutenberg's result is explained. However, since the earth's density is greatest in the core, the gravity will remain constant or even increase slightly with depth in the mantle and decrease towards the center in the core. With constant density the gravity would decrease linearly toward the center.]

P: In 1932 ... that was in 1930, and just a couple of years later you had this note on the use of vectors in meteorology, and tensors, particularly from the standpoint of notation. Do you recall that?

H: Oh yes.

P: What prompted you to comment on that?

H: Now, you notice that this is with Baur ...

P: It's number 13, yes.

H: With Baur and Stüve.

P: Oh no, I didn't notice that.

H: Oh, if you look at the list it's Baur and Stüve.

P: Aha, okay.

H: What happened there was that at that time the German Physical Society I think started, formed a committee for the unification of

vector notation ... well, I mean, there are the notation for the cross product for instance, and in Germany in particular some people would use in order to denote vectors simply what we used to call in Germany "German letters" which is just the old Gothic form, and I was elected as a member of that committee just before I went to the United States. But ... oh, well, no I have that wrong ... I think it must simply have been that Baur, Stüve and I got together at one time, either corresponding or we were at a meeting together ... I certainly had been at meetings with Stüve and also visited Bauer at that time ... and so we decided to make some suggestions for meteorologists to ... what to use in vector notation. I have forgotten now, I still have the paper or a reprint of the paper, but I have forgotten what our suggestions were. I might even have suggested at that time using "German" letters, and later when the German Physical Society decided to have a committee they elected me of the three people who wrote that paper for the meteorologists. And they took me because I was closest to the two other people, but in the meantime I had gone to the United States.

[Postscript 6-14 by B.H.: Paper 13, jointly with Stüve and Baur, was written after the three of us were appointed by the German Meteorological Society as a committee to establish a uniform vector notation.]

P: Well, you had another paper on the use of mathematics in meteorology in 1943, that's number 52, and this was apparently a special paper ... I mean a paper specially directed toward mathematicians.

H: Yes, well, that was during the time of the war courses ... the meteorological war courses ... there were also introductory courses to be given by mathematicians ... and so I got a letter from whatever the organization was ... the American Mathematical Society ... asking me if I would give a talk to indicate what kind of things people would use in meteorology, what kind of mathematics ... of course at that time, that was all pretty elementary ...

P: Yes. Two papers, and I take them somewhat out of order here, one in 1943, and the other one in 1966, number 54 and number 107, deal with the Coriolis force, and I wasn't able to locate either one of those. I think number 54 concerns a simplified derivation of the Coriolis accceleration ...

H: Yes, well I think somebody had published one in the Bulletin ... I haven't got this paper anymore either and I don't even have the Bulletin anymore, because I left that at NYU when I went away ... no but this paper ... somebody published the paper, a note in the Bulletin ... a simple derivation of the horizontal component of the Coriolis force ... and when ... I don't really ... .

P: Was that Horace Byers by any chance? He did something ...

H: I don't think so. It may have been. I just absolutely forgot. But at any rate by reading it I ... it seemed to me a good idea to derive it that way, but thinking about it a bit more I pointed out that one could also derive the vertical component, not only the two but also vertical component of the Coriolis force by the same physical considerations. That's really all.

[Postscript 6-15 by B.H.: Paper 54, on a simplified derivation of the Coriolis force was motivated by a paper by Leaver of the Canadian Meteorological Service, in which such a simplified derivation was given, but only for horizontal motion. The other paper, 107, deals with the name "Coriolis force" and was prompted by an article by Jordan.]

P: Then we have -- to round out this miscellaneous category -- two papers dealing with Antarctica, the first one, number 92 in 1961 and the second, number 105 in 1966. The first one is a very brief overview of some of the outstanding problems of the Antarctic. The second is a very substantial paper, I think, which was the Wexler memorial lecture. Was that the first of the Wexler memorials?

H: No, the second. It was the second one. The first one was by Sydney Chapman. As far as the first paper is concerned, number 92 -- that is chapter 1 of "Science in Antarctica, General introduction to Section 1, Heat and water budget of Antarctica" -- that came about in this way. I had been a member of the ... at that time it was, I think, Committee for the Polar Year ... not the Polar Year, what was it ... the International Geophysical Year, which later became the Polar Committee, but at any rate at that time I think it was still the ... I always call it Polar Year ... the International Geophysical Year, IGY, and I had been the chairman or was the chairman of the panel on Heat and Water, or Heat and Water Budget, which really meant meteorology and oceanography. I think glaciology was separate, but at any rate this committee thought ... or was even told that it should ... at any rate the committee thought that it was a good idea, it would be a good idea to write some somewhat lengthy publication mainly directed at Congress to explain what is being done in Antarctica, why all these expenses. It was of course meant for the general public, therefore, but specifically directed at Antarctica, and various people contributed to this chapter 1 on Heat and Water, but it also needed a general introduction and I wrote this general introduction. Now this general introduction, which as you mentioned, was only about 2 or 3 pages, really concerned largely sort of what you might call "astounding facts" or interesting facts about Antarctica, how ... not really how small it was, but how relatively small it was I mentioned, and then what would happen if the whole continent of the ice would

melt, how the water level would rise and all these things, just to make it interesting for the general reader and especially for a Congressman. So it was ... I wouldn't really call it a scientific paper.

P: However, number 105 ...

H: Oh, yes. Well one of the moving spirits as a matter of fact ... this was 105, that was the second Wexler Memorial Lecture, yes, which I called "Antarctic Exploration". I was invited to give the, this lecture by the AMS and it seemed to me a good idea to talk about something which Harry Wexler had presumably worked on, but which especially meteorologists in general wouldn't be so familiar with. Now Harry Wexler was one of the members on the Heat and Water Budget Panel and later also on the corresponding panel of the Polar Committee. He was in fact one of the moving spirits and I was the chairman. He was really not the chairman, he should have been the chairman perhaps, except for political reasons. It would have been bad because the Weather Bureau of course naturally in the nature of things had to do much of the observing in Antarctica in particular, and in polar regions perhaps even in general. Well at any rate so he had done quite a bit of work on polar meteorology and even in glaciology and things like that. I mean you may know some of these things. So I spent a considerable time reading those papers of his. Some of them of course I had been quite familiar from before. There were papers on meteorology dealing with the arctic, like the kernlose -- he was very fond of that word -- winter in Antarctica, but others in glaciology I really had to learn at that time. And that's how I came to give the lecture ...

P: It's an in-depth assessment not only of Harry's particular contributions to Antarctic meteorology -- or to polar meteorology I guess it would be more accurate to say -- but also of the state of some of the basic problems in Antarctic meteorology at the time.

H: Yes. That's quite true ...

P: And you clearly put a great deal into this lecture.

H: Oh, yes, I did an enormous amount of reading for it.

P: This ... these two papers as I recall are the only two that specifically deal with surface problems. No they are not limited to surface problems, but I would say the only two that specifically deal with the Antarctic. Isn't that so?

H: Yes, that's right.

P: Can you think of any other comment about that Antarctic paper?

H: No.

P: This brings us then to what may be the last of our general topics, and that's the very loose designation "Books and contributions to books". Now here I ran into a very severe problem of availability and I did not take the time today to peruse your own collection of reprints, so I wonder whether we could take just those items in this category that are not the books, such as number 17 which is a contribution to the Physical-Chemical Handbook ... Pocketbook ... and did you want to comment on that?

[Postscript 6-16 by B.H.: The correct title of the book in which my article 17 on atmospheric humidity appears is "Physikalisch-Chemisches Taschenbuch."]

H: Well, that was really something quite incidentally. A colleague of mine who was a physical chemist and at that time still a student as a matter of fact in Leipzig but also associated with a publisher, he was asked to get together a book on the ... a pocketbook on chemistry, and well the idea behind such a book is that it gives you a ready reference to all sorts of different topics in science, this case in chemistry, and he needed somebody to write something about the atmospheric humidity, especially what he wanted obviously was just the different ways in which a meteorologist or a physicist in general would express atmospheric humidity. It wasn't a book which would tell you how to measure atmospheric humidity ... and he asked me to write that. Essentially these whole 2 or 3 pages ... I think it was about three pages ... just deal with the various expressions such as absolute humidity, relative humidity, etcetera.

P: Then the very next paper in 1929, number 18, is co-authored with Weickmann and published in the Textbook of Geophysics on the Mechanics and Thermodynamics of the Atmosphere.

H: Yes, well, this book, this textbook was a combination of the various branches of geophysics. Have you ever seen it? It's about that thick. I have it in my room back there ... and one of the authors who was asked to contribute to the meteorology ... was Weickmann. He was asked to write in particular the section on Mechanics and Thermodynamics of the Atmosphere. Now Weickmann was usually very busy and in this case, at that particular time he was particularly busy, as a matter of fact, because he took part in a flight with the Graf Zeppelin, the dirigible, across Siberia, with the Russians' permission incidentally I should say. So he came to me. I was at that time an assistant in Leipzig and had two years ago my Doctor's degree, and he asked me if I would help him with this particular chapter. He said he would get all the literature together, that is, tell me what to use, but I was going to ... he would do the

compilation and then I would sit down and write it. Well, of course first of all, even if I hadn't wanted to, you don't say no to your boss, at least in Germany, and secondly it seemed a good idea, so this is how that came about. I really wouldn't want to say that I was the author, I was just sort of an assistant. I mean it is indicated in this particular chapter by the way that it says in collaboration with B. Haurwitz.

P: Is that a very lengthy contribution, do you recall?

H: If I remember now correctly, it's about a hundred pages or so.

[Postscript 6-17 by B.H.: Article 18 in Gutenberg's textbook on geophysics is nearly 170 pages long.]

P: Oh?

H: Oh, it's by no means short. And one of the interesting things I might mention in there -- in the case of the recent flap about this book by Gisela Kutzbach and the forerunners of the Norwegian theory -- now in this particular section of the book, Weickmann and I with him, of course, made it quite clear that the Norwegian wave theory of cyclones or polar-front theory or whatever you wanted had indeed predecessors, people who had ... well I won't say anticipated it ... but certainly had some of the ideas which already were very closely akin to polar-front theory.

P: Interesting. Number 77 is a contribution to the old Compendium of Meteorology published in 1951, on "The perturbation equations in meteorology." That I could have looked up in the NCAR library, but I didn't have the time before coming down here.

H: Well I don't know that I have very much to say with respect to it. I was supposed to have this ready at a certain time and I had it ready just exactly one year later. I say later not late, because other people were quite a bit later than I was. Well, it's exactly what it was, the perturbation equations -- that was the equations of motion -- as linearized by Vilhelm Bjerknes.

P: Would you say that ... I was just going to say that it would be an update of Bjerknes' treatment of the subject ... when was that ... I think there was something in Geofysiske Publikasjoner in 1920s.

H: Yes, it must have been around 1929 or a bit earlier even. Well, I don't know if it really was much of an update. The only thing was I had a few things in there ... a few actual applications in there ... for instance I have in there an example of how to ... how the perturbation equations might look ... or the atmosphere might look on

the spherical and rotating earth and with the derivation of the whatever you want to call it ... formula for the frequency of long waves in the westerlies, Rossby waves, or waves of the second class, but ...

[Postscript 6-18 by B.H.: The atmospheric perturbation equation for a spherical, rotating earth had already been given by V. Bjerknes, but the derivation of the Rossby waves in this geometry was a new example of their application.]

P: Well, now the last of the items other than books proper in this category is number 112, your contribution to the Chapman volume, called "Scientific contributions of Sydney Chapman, a review."

H: Yes. Well there are just a few papers of Chapman's, meteorological papers which we mentioned, some of the atmospheric tide papers, but ... again this is really not a very deep paper. Of course, this whole book on Chapman ... "Sydney Chapman, Eighty" ... has been ... well the three editors have been Ben Fogle, Syun Akasofu and myself.

P: That was based I think wasn't it on talks that he gave at NCAR?

H: Yes, have you ... ?

P: I have a copy of it.

H: Oh, yes, I was going to say, we still have copies.

P: It's a very nice little book. I'm glad that we have a little time left, Bernhard, probably about eight minutes, to talk about the books and perhaps some more general subjects. There are three books, The Physical State of the Upper Atmosphere is the first and that was published in a revised edition somewhat later, first published in 1937, and then in 1941 was the revised version. The second book is the Dynamic Meteorology published in 1941, I believe, and then the co-authored book with Jim Austin in 1944 on Climatology. Would you like to just make some free-for-all comments on those books?

H: Well, as far as the first book is concerned, lets see, what was it called, the ... .

P: The Physical State of the Upper Atmosphere ...

H: The Physical State of the Upper Atmosphere. That came about because when I first came to Toronto in 1935 I was officially a member of the Physics Department there ... the money was given by Carnegie so I was called Carnegie Fellow ... and the Physics Department

suggested ... as a matter of fact, it probably was not the Physics Department at that time, but it was Andrew Thomson, who was at that time called the "Physicist" of the Meteorological Service of Canada and he was my immediate superior, so to speak. He probably suggested it originally to the Physics Department. At any rate, I was asked to give a series of lectures in the Physics Department and I selected as my topic the physical state of the upper atmosphere because that seemed to be something in which physicists might be more interested than the lower atmosphere. I approached the whole thing incidentally with quite a bit of trepidation because I felt I didn't know enough about quantum physics and things like that, spectroscopy or so, and I spent quite a bit of preparation in reading for it, but it turned out I needn't really have worried. Well, when I gave these lectures ... there were altogether ten, and if I remember, they were given once a week ... while I was in the process of giving them, one of my auditors who was Prof. Chant from ... the Director of the David Dunlap Observatory, the astronomical observatory of the University, approached me and said that he would like to publish them. He was at that time the editor of the Journal of the Royal Astronomical Society of Canada, which is a popular journal, but, a well, if I may put it this way, it's one of these British-type popular journals with serious amateurs and the papers, even though they are mostly quite elementary, are really of a very good quality. Well at any rate he suggested that I might want to write up my lectures and he would be happy to publish them in some installments in the Journal, and of course I said yes to that, and that's how they were first published there, and then the Royal Astronomical Society of Canada decided to put the lectures ... the whole thing itself into a book and that book then was for sale by the Society. I don't think really they sold them much at first except that when the war broke out and the United States ... the Universities needed a textbook for the air weather officers there was suddenly a great demand for this small book. That's how the second edition came about.

P: This was before Mitra, was it not?

H: Yes.

P: But now it would be interesting too to hear how the project for "Dynamic Meteorology" got going.

H: Well, this just started in a very relaxed way. When I came to Toronto that was the first time I gave the course in dynamic meteorology ... started out either in 1935 or 1936, and had what I thought was very good lecture notes, so I decided I might as well consider writing it down more in a book form and I started writing on it in book form ... that must have been around 1939 or so, I would guess, because the only English book at that time was Brunt, which

didn't seem too suited, at least not for Canadian conditions. It was for one thing simply too large. Well, at any rate, and then while I was working on that in 1940, I think, Sverre Petterssen came for a longer visit from MIT ... by longer I mean two or three weeks ... to Toronto and well of course he found out about the book and he said ... he talked a little bit and I think he read a few of the things at that time and he said, well, ... if I had a publisher and I said no, I really hadn't thought of that yet ... and so he said he would be glad to talk to McGraw-Hill about it ... his book of course was published by McGraw-Hill, and McGraw-Hill were interested in it and so that's how it came about.

P: And it was actually 1941, the year of publication. At that time were you not at MIT?

H: Yes, I came to MIT and the second proofs were edited while I was at MIT.

P: Why did you not think ... I mean not think ... but why did you not arrange for a second edition?

H: Well, the question about a second edition came up ... I'm not quite sure when I got a letter from McGraw-Hill ... it must have been either just before I went to NYU, New York University, or after I had gone there ... but at any rate at a time when I knew that I was going to change and I felt I would have too much to do to also be bothered about a revision of the book because at that time that would have been in 1947 ... six years later and after the war. It would have required I think quite a bit of revision. So I just said no I couldn't write anything at this time and then apparently they took the manuscript by Haltiner and Martin or I forgot, is it Haltiner and Martin or Martin and Haltiner ... ?

P: Well that was ... the 1941 book came at a superbly timed moment.

H: Yes, well that was pure accident, but I still remember when Petterssen talked to me about it I said well of course I will never make much money on it, but actually then during the war it really turned out to be financially quite rewarding.

P: But I think that there is perhaps a more fundamental reason than simply the wartime situation, although that certainly did account for the immediate demand for the book, but more fundamentally I think that it filled a gap that was obviously very keenly felt for a basic text, English language text in dynamics and also I think that it filled, it provided physicists -- in general, non-meteorologists -- with a convenient and understandable way of learning about the basics of meteorology.

[Postscript 6-19 by B.H.: With regard to "Dynamic Meteorology" I might append here that it was never reviewed in the Bulletin of the American Meteorological Society. However, it had a good review in Science by a physicist, namely Walter Elsasser.]

H: Yes, well, this is very complimentary. I hope it's true. Which reminds me that I think Chuck Leith once told me that he learned his dynamic meteorology out of that book without ever knowing anything about it before.

P: Well, that's an example. I think he was a physicist ...

H: Of course. Chuck Leith probably would have gotten it out of any type of book no matter how bad.

P: The book with Jim Austin, how did that come about? That was just a few years later.

H: Well, that came about somewhat in a similar way, but Petterssen at that time when he was in Toronto also realized from some of my talks that I had gotten itchy feet again or whichever way you want to call it ... well, I wasn't entirely happy at Toronto.

[Postscript 6-20 by B.H.: The book on climatology with Austin was written because I had to give a course on the subject at MIT. Originally it was planned that after my arrival at MIT, Petterssen and I jointly would give the course and write the book. But not very long after my coming to Boston, Petterssen left to go to England, and I had to give the climatology course alone. This was very good for me because I learned a lot, but I did not have any satisfactory text for the students. Therefore I prepared lecture notes for the students. These notes formed the basis for the book. Because of my abysmal ignorance of synoptic meteorology I invited Austin to co-author the book with me, and he accepted. It was a very pleasant collaboration.]

[End of side 2, tape 6]

[End of conversations]

APPENDIX A

BERNHARD HAURWITZ PUBLICATION LIST
(followed by a list of full names of journals cited)

1. Beziehungen zwischen Luftdruck- und Temperaturänderungen. Ein Beitrag zur Frage des Sitzes der Luftdruckschwankungen. Veröff. Geophys. Inst. Leipzig, 3, 1927, 266-335. (Doctor's Thesis)

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3. Zur Berechnung der Neigung von Diskontinuitätsflächen mittels der Margulesschen Formel. Meteorol. Zeitschr., 45, 1928, 338-341.

4. Die Arbeiten zur Dynamik der Atmosphäre von Diro Kitao. Gerlands Beitr. Geophys., 21, 1929, 81-102.

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16. Investigations of atmospheric periodicities at the Geophysical Institute, Leipzig, Germany. Mon. Weather Rev., 61, 1933, 219-221.

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20. (with H. Wexler) Trübungsfaktoren nordamerikanischer Luftmassen. Meteorol. Zeitschr., 51, 1934, 236-238.

21. Daytime radiation at Blue Hill Observatory in 1933 with application to turbidity in American air masses. Harvard Meteorol. Studies, No. 1., 1934, 31 pp.

22. A theoretical study of wind-velocity and wind-direction in curved air-currents. Trans. Amer. Geophys. Union, 16, 1935, 124-126.

23. The height of tropical cyclones and of the "eye" of the storm. Mon. Weather Rev., 63, 1935, 45-49. (See also item 42a.)

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(and corresponding numbers in the publication list)

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Association d'Oceanographie Physique (now International Association of Physical Oceanography), Procès-Verbaux (82)
Atmosphere (123)

Beiträge zur Physik der Atmosphäre (89, 98, 111)
Beiträge zur Physik der freien Atmosphäre (5, 15)
Berichte des Deutschen Wetterdienstes in der US-Zone (78)
Bonner Meteorologische Abhandlungen (121)
Bulletin of the American Meteorological Society (24, 37, 39, 41, 44, 54, 62, 65, 72, 80, 85, 105, 107)

Canadian Meteorological Memoirs (42)

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Journal of the Aeronautical Sciences (49)
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Veröffentlichungen des Geophysikalischen Instituts der Universität Leipzig (1, 10)

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APPENDIX B

TOPICAL OUTLINE (by B.H.)

A. Vertical distribution of pressure and temperature changes ("seat" of pressure changes)
Publications 1, 2, 11, 40, 42, 44, 45
Items 1, 42, 45 are longer papers dealing with data over Europe (1) and North America (42, 45).

B. Vortex motions and tropical cyclones
Publications 4, 7, 22, 23, 25, 26, 27, 32, 75
Items 4, 7, 75 deal with the relative motions of two vortices (cyclones) around each other. Items 22, 25, 26, 27 deal with the Ekman spiral when the balance of forces includes the centrifugal force. Items 23, 32 deal with the height of tropical cyclones and the funnel shape of the eye.

C. Atmospheric wave motions (short and synoptic scales, excluding waves in noctilucent clouds)
Publications 8, 9, 10, 12, 14, 15, 24, 37, 43, 53, 64, 72, 123, 124
Item 8 comments critically on a paper by F. M. Exner. Item 10 is a potpourri dealing mainly with interface waves. Items 12, 14, 15, 64 are about billow clouds. Item 37 is a non-technical account of the Norwegian wave theory and its results. Item 43 is an attempt, not particularly successful, to account for the observed pressure variations at different levels by means of interface waves. Item 53 has already been treated by Rayleigh, but without Coriolis force (which has no effect on stability).

D. Planetary waves
Publications 33, 35, 36, 46, 47, 69, 78, 79, 126, 127, 129, 130
Item 35 is a critique of a paper by Ertel. Items 46, 47 are extensions of Rossby's work on "Rossby" waves (see also 69, 79). Items 126, 127, 129 attempt to apply a cylindrical geometry to a part of the globe.

E. Atmospheric radiation
Publications 20, 21, 38, 57, 61, 66
All these papers deal with radiation measurements made at Blue Hill Observatory and Mount Washington which had never been looked at. Attempts are made to derive or confirm statistical relations for radiation-climatological purposes.

F. Turbulence, viscosity
Publications 28, 31, 70 (see also B 25, 26, 27 and G 95)

G. Upper atmosphere (tides excluded)
Publications 41, 49, 93, 95, 102, 108, 115, 118, 120, 121
Item 102 is just a theoretical discussion of wave motions, intended for the noctilucent cloud observers at the University of Alaska. Item 108 is a general review of the subject, now very much dated.

H. Atmospheric tides
Publications 65, 83, 84, 85, 86, 87, 89, 90, 94, 96, 97, 99, 100, 101, 103, 104, 106, 109, 110, 111, 113, 114, 116, 117, 119, 122, 125, 128, 131
Most of these items are concerned mainly with data analysis. Item 94 bemoans the usual omission of statistical significance tests. Items 100, 101, 109 are review articles.

I. Weather analysis and forecasting
Publications 3, 19, 39, 56, 62, 80
Item 62 reports on tests of a forecasting method by Kibel which gives apparently very poor results (Kibel strongly dissenting).

J. Solar variability and the atmosphere
Publications 29, 50, 58, 67, 74, 88
Items 29, 50 deal with solar-eclipse effects on the low atmosphere. Item 67 is an attempt at a physical explanation of hypothesized solar influences by way of the ozone layer.

K. Miscellaneous dynamic meteorology
Publications 59, 60, 63, 98

L. Oceanography
Publications 68, 71, 73, 76, 81, 82, 91

M. Miscellaneous
Publications 6, 13, 52, 54, 92, 105, 107

N. Symmetry points
Publications 5, 16, 30

O. Books and contributions to books
Publications 17, 18, 34, 48, 51, 55, 77, 112
Books are underlined. Item 48 is the second edition of 34.