Conversations with Jule Charney

George W. Platzman,
University of Chicago
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In the Spring of 1980 Jule wrote to me of his wish to undertake a "tape-recorded oral biography." (His letter is reproduced in Appendix D to this transcript.) The publishers Harper and Row had asked him to write a biography, with financial support from the Sloan Foundation, but he felt that an oral interview would be "the best first approximation." I replied enthusiastically.

A few weeks later I had second thoughts and wrote to Jule that on reflection, I had become sobered by the subtleties of the art of interviewing, and suggested we engage a professional for the "basics", which could then be supplemented by a more idiosyncratic sequel such as he and I could do. Jule was firm, however, in his preference for working with someone with whom he felt he could communicate easily as a colleague.

Early in August 1980 we scheduled the interview for the week of the 25th. I then began to feel more keenly my obligation to use this occasion in the most productive way as an opportunity to illuminate the historical record through the mind of one of the leading figures of twentieth-century meteorology. I turned to three colleagues whose long personal and scientific association with Jule made them especially able to share this responsibility with me to some degree, namely Norman Phillips, Joseph Pedlosky, and Joseph Smagorinsky. From each, by telephone, I elicited extemporaneous suggestions for topics to include in the interview, and a few days later received from Norman some more deliberately worded questions. These suggestions helped to diminish my uneasiness in undertaking to be an interlocutor, a role for which I had absolutely no experience. I believe almost all of them found their way into the interview.

As a location most likely to protect us from interruption, Jule and I agreed to conduct the interview in my room at the Holiday Inn (Blossom Street, Boston), within walking distance of his apartment on Lewis Wharf. (This proved a good choice, except for occasional intrusion of sounds emanating from both inside and outside the hotel, as auditors of the tapes will find.) The interviews were recorded in four days beginning Monday August 25. On each of the first three days we started in the early afternoon and made one 90-minute tape. On Thursday we made one tape in the morning, had lunch in the room, and made one and one-half tapes in the afternoon. A total of about eight hours was recorded. The first three tapes concern Jule's education, his doctoral dissertation, and his postdoctoral years at the University of Chicago and the University of Oslo. They span the first 31 years of his life, to 1948. Tape four deals with the Princeton years

After the morning session of the last day my effectiveness declined, and when in the late afternoon we began the last tape, Jule completed the interview in an almost uninterrupted monologue, with little assistance from me. Not only was I fatigued, but in retrospect I realize that after 1956 when Jule left Princeton, my contacts with him and my knowledge of his work had diminished.

Although there is little doubt that Jule's long and almost indomitable battle with cancer made grave inroads on his stamina (he reclined throughout the sessions), those who listen to the tapes and know him personally will detect no sign of this and will hear his familiar conversational style, vigorously combative, intensely groping for intellectual clarity and accuracy, displaying no false modesty but never disdainful of others, finding humor and a hearty chuckle at every seemly opportunity. His conversation, firmly controlled by his wide-ranging mind, was never glib and often did not flow smoothly. Those not accustomed to it may be disconcerted by his frequent interruptions of himself (as well as of others!), a tendency clearly revealed by the transcript. (Perhaps we are all more disjointed in conversation than we think we are.) I am reminded of Sylvanus Thompson's description of Lord Kelvin as a lecturer: "His imagination was vivid: in his intense enthusiasm he seemed to be driven, rather than to drive himself. The man was lost in his subject, becoming as truly inspired as is the artist in the act of creation." (Life of Lord Kelvin, vol. I, p. 444).

Jule came to these sessions without notes. (My admonition that he not use notes was unnecessary, as he well understood the advantage of spontaneity, and indeed that was his natural style.) However, he certainly had an outline in mind of the topics he wanted to include and, moreover, it was on his initiative that the interview was conducted. Both of these circumstances, and the fact that Jule is an engaging conversationalist, made my task as interlocutor easy and pleasant. The only topic that he was at first reluctant to include was his childhood and family background -- not, I am sure, because the subject in any way embarrassed him, but simply because he felt it was not relevant to his life as a scientist. After reflection he yielded, however, and came to the first session without inhibitions about this topic, as the reader will find.

My own preparation, apart from the solicited advice previously mentioned, was regrettably limited to the brief intervals available after my arrival in Boston. At the start of each of the four days I wrote an outline of the topics I thought should be covered that day, and some specific questions about them. I had asked and received from Jule a copy of his vitae and list of publications, and I also brought reprints of some of his papers. However, for the most part my outline was useful only when the conversation lagged, and this happened infrequently.

Jule and I probably were a little self-conscious at first,
being accustomed neither to the roles we were playing nor to the unforgiving monitor of our performance. I believe this feeling rapidly receded for the good reason that we both became absorbed in the subject matter of the discussion. Indeed, time passed quickly, and at the end of a session although we had to stop the tape, we could not stop the conversation. I regret that these lively epilogues are entrusted only to my errant memory.

Before going to Boston I suggested that we agree in advance on what would be done with the recorded interview. Jule was not receptive to this suggestion. He regarded the interview not as an end in itself but as a first step toward a more finished "intellectual autobiography" (quoting his letter reproduced in Appendix D). As a second step his intention was to edit a transcript but Fate, the arbiter of human aspirations, did not permit that step to be taken. Subsequently Nora Charney, Jule's daughter and Executor, agreed to share with me responsibility for disposition and use of the interview. Our desire is to promote serious historical scholarship and to preserve the original tapes under conditions of safety and controlled access. We therefore deposited them, with the transcript, in the MIT Institute Archives, where they will be subject to normal conditions of use. This arrangement was made possible and convenient for us by the Institute Archivist, Helen Samuels.

Jane McNabb, administrative officer in the Department of Meteorology and Physical Oceanography at MIT, obtained the tape recorder and blank tapes and put them at my disposal. I am also grateful to Jule's secretary Joel Sloman whose meticulous transcription is a remarkable achievement in its fidelity to the spoken word, and in capturing the nuances of Jule's conversation. As our mutual aim has been to produce a verbatim transcript, my editorial contribution consisted mainly of rendering some technical terms and attending to the spelling of proper names. Edward Lorenz and Morton Wurtele assisted in the latter task. I believe that only unimportant mumblings of words or phrases remain undeciphered. To Marilyn Bowie goes the credit for converting the transcript to a formattable file on the University of Chicago's computer.

Occasionally Jule and I were uncertain about names, dates, or events, or we clearly mis-spoke. I have taken the liberty to insert corrections (perhaps I should say presumed corrections) within brackets in the text of the transcript. In addition, I placed into an "Interviewer's commentary" some remarks on a few doubtful points, and into an Appendix some documents and letters cited in the transcript or commentary. Arnt Eliassen and Norman Phillips contributed to the commentary. Philip Thompson provided two early Charney letters (Appendices B and C).

The University of Chicago
December 1982

George W. Platzman
Transcriber's foreword

A transcription is like a photograph. It gives the illusion of truth and is therefore often misleading. In transcribing, one tries to reproduce all the intelligible language on a tape and trust -- or hope -- that, in conjunction with the deviations from continuous discourse, the falterings, the pauses, the repetitions, and so on, the meaning of someone's real speech will be communicated. This does not always happen. I would therefore remind readers that the tapes themselves are available to help settle questions of tone and meaning.

Punctuation is often a question of interpretation. Another transcriber might have used fewer, or more, periods and commas, or placed them differently. It was often not clear to me when a sentence ended because so many thoughts begin with the word "and." Very little emphasis is indicated. I could easily have underscored fifty words or phrases that I left as is. "Um"s and "ah"s are generally left in.

Few editorial devices are used. A question mark in parentheses indicates any sort of questionable interpretation. Ellipses indicate syntactical breaks and moments when the two speakers talk simultaneously. They don't indicate pauses in time.

I would, finally, like to thank George Platzman, as well as many people in the MIT Department of Meteorology and Physical Oceanography, for being patient with me over the long period of time it took to complete this transcription. During that time Jule Charney was, to me, still living.

Massachusetts Institute of Technology        Joel Sloman
August 1982
FOREWORD

Jule Charney (1 January 1917 - 16 June 1981) was an internationally recognized leader in meteorological research for more than three decades. After earning undergraduate and graduate degrees from the University of California, Los Angeles, Charney began his long and distinguished career at the University of Oslo, Norway, in 1947. From 1948 to 1956 he was at the Institute for Advanced Study in Princeton, New Jersey, where he was director of the Theoretical Meteorology Project. From 1956 until his death, he was a faculty member at the Massachusetts Institute of Technology, where he headed the Department of Meteorology from 1974 to 1977.

When Charney, the recipient of numerous awards and honors, received the American Geophysical Union's William Bowie Medal in 1976, it was observed that "... more than any other living figure, [he] has guided the postwar evolution of modern meteorology." His contribution to the modern understanding of atmospheric dynamics was enormous.

Jule Charney's conversations with George Platzman, transcribed in this volume, took place only a few months before his death and cover his entire scientific career. The idea of publishing this interview as an NCAR Technical Note originated with NCAR archivist Nancy Gauss. NCAR gratefully acknowledges the cooperation of George Platzman and of Helen Samuels, archivist at the Massachusetts Institute of Technology, in the arrangements for this publication.

NCAR Warren Washington
September 1987 Director, Climate and Global Dynamics Division
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Note: In many places the topics overlap, so this outline is only a rough guide. For example, there is further discussion of the Princeton period at the beginning of tape 5-1.
George Platzman: This is an interview with Jule G. Charney. Mr. Charney has been a meteorologist since his student days in the early 1940s at the University of California at Los Angeles. He is presently Professor of Meteorology at the Massachusetts Institute of Technology. This first session of the interview is being conducted on Monday, the 25th of August, 1980, in Boston at the Holiday Inn. The interviewer is George W. Platzman, a friend of Mr. Charney from the University of Chicago. Here is an outline chronology of Mr. Charney's career: our conversations probably will follow this chronology to some extent. Mr. Charney was born in 1917 and received a bachelor's degree in 1938 from UCLA. From 1938 to 1941 he was a graduate student in mathematics and physics at UCLA and from 1942 to 1946 a graduate student there in meteorology. From 1946 to 1948 he was a visitor at the University of Chicago and at the University of Oslo. From 1948 to 1956 he was a member of the Institute for Advanced Study at Princeton. Since 1956 he has been at MIT.

[End of leader]

P: Incidentally, following the how-to-do-it book, I have pre-recorded about a one-minute strip of an introduction, in which I say this is an interview of Jule Charney, and give the date and my name and things like that. This is what I picked up from the how-to-do-it book . . .

Jule Charney: Okay.

P: . . . as a kind of a leader to the tape. I also gave there a thumbnail outline of the chronology that I think we may roughly follow, but we're not bound to follow anything. We can jump back and forth if it seems convenient at the moment. But, um . . . there is the tapping.

C: Yeah. But I don't think that would interfere . . . (?)

P: You would be amazed, you'll be amazed, and you . . . We'll play this thing back again, you will hear that in the background, but it doesn't make it, it doesn't make it obscure what we're talking about, it's just a little annoying. Well? let's begin.

C: Well, I mean . . . you want to tell me what your chronology is or should we just start with my . . .
P: Chronology starts from $t = 0$ and it goes forward in time.

C: But from $t = 0$?

P: You were born in San Francisco.

C: Yeah, on January 1st, 1917.

P: That's rather an ideal place to be born.

C: Yeah.

P: I take it you didn't pick it yourself, but . . .

C: No.

P: . . . what were the circumstances?

C: All right, I've given a few minutes of thought to, you mentioned something about my family background. So I thought that we could perhaps start with that. I came from a Russian Jewish . . . both my father and mother were Russian Jews, and it wasn't a habit to trace one's family, to keep records of one's family background. I think . . . I did have an uncle who made it his business . . . who was interested in such things -- and he did have files on the family background; there was an old family bible, which he got a hold of somehow, and he was in touch with our relatives who remained in Russia. But he lived in New York in an apartment house, and he had it stored in a . . . downstairs in the basement, and the apartment house burned down after he had died. At least, so his wife says, and all these records were destroyed, so I can't . . . I have . . . I can't . . . you know, even if I were to do research I wouldn't be able to tell you much beyond my grand . . . anything much beyond my grandparents, you know. I could say I came from a long line of rabbis, but I don't know that. (laughter) On my father's side . . . he left Russia -- I don't know exactly at what age, but I think it must have been . . . It was at the point where he, if he had remained, he would have been drafted into the army. And Jews were very badly treated in the army, so many Jews left at that point -- Jewish men -- and he left. He lived in Bobruysk, White Russia. His father was a lumber merchant and he had a couple of older brothers, but he never kept in touch with any of his family in Russia.

P: Hm!

C: Nor with branches of his family in this country, for reasons that are a little bit hard to fathom, but this will come out maybe . . . So I know very little of his background. I think he
had more than the usual education for a person and a Jew in that family . . . he didn't go to a university, but I think he had some gymnasium. And he was . . . he came to this country, first, I think, to New York as they all . . . and then, somehow, went to St. Louis. He didn't have a trade particularly, and so, as many of them did, he became a garment worker. He didn't have to know much.

P: When was that?

C: Well, I think . . . He was born in about 1883 and died at the age of 73, so that was in 1956. He must have come over in his early twenties, probably. And he met my mother in St. Louis, and they fell in love and were married against her parents' wishes. They didn't think too much of him. I guess he was an unambitious person with not very much confidence in himself. They were afraid that he would be a ne'er-do-well and, what he turned out to be some, to a degree. He remained a garment worker, off and on, most of his active life, and most of his . . . He never tried to get in touch with, as I mentioned, other branches of his family, who were . . . some of whom were really quite distinguished.

P: Where were they located?

C: Well, he had one cousin -- Charneyvladich (?) -- who was the managing editor of the *Forward* in New York. And he had two other cousins -- one was Charniger (?) -- who was a famous Jewish writer, poet, lived in France and in the States. And another was Daniel Charney, who was a poet and critic, a writer and critic, also extremely well known, in the Jewish world, because they wrote in Yiddish. Several years ago, when I had organized UNAF [University Anti-War Fund (GWP)] . . . remember that? . . . I met Marty Peretz, who was then teaching government at Harvard and sort of in the anti-war movement, at Wiesner's house, and when I was introduced to him, he said that, do you come from the literary . . . Are you related to the literary Charneys? And I said, yes I was. And I said, are you related to the literary Peretz? He said, yes, he was. Of course, you've heard of Peretz, a great writer, in Yiddish. Of course, he's been translated. But my father never made any contacts with them as far as I know. My mother . . .

P: Before you get to your mother, let me ask, what is the Russian form of the name Charney?

C: That's an interesting point. The name Charney is the Polish pronunciation of black. And I think, names . . . last names . . . Jews didn't even have last names until fairly recently. And my father is dark skinned, as I am, and uh . . . but I don't know the exact reason . . . you know, it's like *schwartz* in
German. The family lived in Byelorussia, White Russia, and I think, at one point, that was part of Poland or, at any rate, it probably accounts for the pronunciation. The Russian pronunciation is Chorney.

P: So it's a rather direct transliteration of the Russian name?
C: Yeah.

P: Now, I think you were about to talk about your mother.

C: Yeah. Her family name was Sukharev, which is something like Sakharov, but "u, e" instead of "a, o." Um. Her father was a manager of a flour mill. And, very much aided by her mother, who was a very strong-minded woman with very little education, but a lot of horse sense. And her father I never knew, because he died when I was a baby. He was . . . they emigrated when my mother was about seventeen. She was born in 1889. So that would have made it about 1906 . . . 1907, I think maybe it was that she actually came over. With her father and mother. They too first went to New York and then, I think, they also went to St. Louis where an older brother of hers had established himself as a pharmacist. He was an interesting . . . she came from a large family. I think there were nine.

P: Mm.

C: And, uh, they were not . . . It's an interesting family. They . . . some of her . . . there were a number of them. Some of her older siblings showed signs of real, I mean, were interesting people. One, I think, actually was jailed for her socialist activities and may . . . and actually was involved with the 1905 revolution. The parents were religious Jews. The children tended -- almost all of them -- to be caught up in the intellectual ferment of those days.

P: Being jailed in those days was considered a badge of honor.

C: Uh, yeah. And another one was a gifted mathematician. She came to this country, but hated it; and she disappeared on the . . . In those days there was a ferry between New York and Boston, and she disappeared. They think she probably committed suicide. Um. Her brother in St. Louis was one of the organizers of the socialist party there. They . . . both my father . . . My father was a sort of a right-wing socialist and my mother was a left-wing socialist. My father read the Forward and my mother read Freiheit, and they constantly fought and argued. And I remember from my earliest recollections that there was . . . that politics was very much the topic of conversation in our family and my father was quite well read. I, uh . . . my mother was the stronger personality and the better worker. She always had a
job. She became a lady's tailoress. Also (picking it up the way my father did).

P: I remember your mother very well.

C: But she tended to work more on custom . . . I think . . . my father was a factory worker, my mother became more of a custom worker. And later on, she worked for the studios, and when the Metropolitan Museum had an exhibition of costumes -- Hollywood costumes -- from the golden days of film, she knew practically all the designers and had worked for them.

P: Interesting.

C: At any rate, and she was an excellent worker and, being the stronger personality, and my father being frequently unemployed, he had a very ungovernable temper. And, being a socialist, he was sort of in the . . . immediately joined the union and the workman's circle. And one reason why he was frequently unemployed was that he was an activist. He was often elected shop steward of the union, a strong union man, and when things were slow, he would be the first to be fired. (laughter) But he was also nervous and not a terribly good workman. For this reason, I think I always sort of . . . and, my mother loved my father very deeply, but they quarreled a great deal and I took my mother's side and grew up with . . . I think with a . . . with not a very good image of my father and a very strong image of my mother, and it was only, after I left home, that I began to appreciate my father's qualities. Turned out . . . He wrote infrequently, but I have some of his letters. They're beautifully written . . . and in excellent English. He always, both . . . he retained an accent all his life. My mother did not, strangely. I think, because she associated with American-speaking people more.

P: What were their respective inclinations toward music?

C: Ah . . . It wasn't highly developed. They had . . . But I remember, you know they would have some of the standard . . . Galli-Curci, Caruso . . . They tended to have operatic arias, Tchaikovsky's 1812 Overture. I remember very much . . . And I remember as a very small child, you know, from let's say, well, I can . . . from as early as I can remember, I loved music and would play those records over and over again . . .

P: Hm.

C: . . . for myself. But I never was given any musical lessons, which I think is a great pity. I don't think I have a good ear, but I love music, and it would have been a nice thing.
P: Well, now where does San Francisco come into the picture?

C: Well, after they . . . they left . . . they didn't stay for any length of time in St. Louis. And then they went to Denver, where an older sister of my mother's was married to an uncle, who had gone there because he had consumption. And he got over the consumption. And then established a very lucrative insurance business where he sold insurance to mine workers, Italians and Mexicans, and it's where he acquired a great taste for the vine. And he was an extremely interesting man who had quite an influence on my life because he was very convivial, utterly ruthless, chap, who used his relatives and was not very kind to my mother and her parents when they came to Denver. But was vastly entertaining, enormously well read. You know, later, this is where I first read Mencken's . . . what was it that he edited in those days? . . . where I first got to know Mencken, first got to know The New Yorker. I was then very young, ten, eleven, twelve. He used to take me out on some of his insurance rounds, and he was a gourmet; he would take me to some of the best places in Los Angeles, where one can get deep dip roast beef sandwiches, which were wonderful, as I recall in those days. I admired him greatly, but he was very unsupportive. (laughter) My grandfather died, I think, in Denver. He never really got back to work. He must have been in his sixties at the time. He was kind of a . . . oh, something like a sexton in a synagogue. And my grandmother, at first I think stayed with her . . . with my mother's older sister, but then eventually came to live with us in Los Angeles. My parents first went from Denver to Los Angeles in 1914, I think it was, and they stayed . . . I think, it's that my father couldn't find work. It was a little bit strange; it was the beginning of the war. So they moved to San Francisco, where I was born in 1917. And then in 1922, I think it was, they moved back to Los Angeles, where I was raised, so I only was five, approximately, and I remember rather little of . . . Oh, no, I was old enough to remember San Francisco, but it was mainly things like Golden Gate Park. We lived on a street with the same . . . with the Menuhins, and I was a playmate of Yehudi's when I was four years old or so, and I used to remember that he was already then . . . he had started the violin and . . . they had . . . houses were built on hills and he was on . . . Their house had a roof, and on the roof, we played, on his roof, where we had a windmill and sorts of things like that and then suddenly he would have to go down to practice. I would be left by myself. (laughter)

P: Have you seen him since those days?

C: I never really got . . . I only very recently, I was in Los Angeles, and he was playing at the L.A. Philharmonic. so I ventured to go backstage and just introduce myself. Of course, I didn't expect him to remember me. He did not. But he did
remember the windmill on the roof. (laughter) So that confirmed that I really had played with him.

P: Were your parents . . .?

C: But I think my parents and his parents were good friends.

P: Were your parents liberated Jews, or were you given an orthodox education?

C: No no, as you can gather from their political philosophy, they were liberated Jews. They were atheists or agnostics, I suppose you would say, and I was, despite the fact that my grandmother came to live with us when we moved to Los Angeles, um, and was, of course, herself religious, that made . . . you know, she was not able to have any influence on my beliefs. In fact, I remember asking her once whether . . . why she was praying. She really didn't know. She prayed in Hebrew and she read . . . and could read Hebrew, but she was unable to really explain to me what it was she was davening. So I had a totally secular upbringing.

P: Well, I think that Hebrew to a large extent . . .

C: I think, you know, political idealism replaced religion. And when much later in my life I felt that I had really been deprived of the religious experience . . . And I remember once on an oceanographic expedition, I read . . . I brought along William James's Varieties of Religious Experience and read it . . . I decided that the religious experience was merely a strong belief and I think my political beliefs were as strong as other people's religious beliefs, and as little founded. (laughter) They were inherited . . . (?)

P: Yes. Your primary schooling then was in Los Angeles?

C: Yeah.

P: And secondary?

C: And tertiary. (laughter)

P: And tertiary. Do you have any particular recollections of those days?

C: Yes, of course, but I don't know that we should spend very much time with that. I learned to read at a very early age. I think, I certainly was, I was four or less. I remember . . . and, as soon as I could . . . I suppose by the time I was six or so I had a library card, and we had a library close by, and I . . . That library was my sustenance. I mean, I took as many
books as I was allowed. I think in the beginning it was three. And it was a great day when I could even take five books.

P: What guided your choice of subject matter?

C: Nobody's. Just browsing in the library. I didn't ... Not exactly, because I mentioned, later, when I was twelve, thirteen ... no, even before then, because we still lived in Boyle Heights, I came to a degree under my uncle's influence and somehow he was a ... I became very fond of Mencken and another critic who was particularly interested in music and from whom I began to acquire a more sophisticated musical taste. He, for example, ... at that time wrote glowingly of Richard Strauss so I began ... I remember I saved up some money and I heard ... went ... I couldn't have been more than ten or eleven. I went downtown by myself and heard Elisabeth ... I think it was Elisabeth Rethberg ... 

P: Yeah?

C: ... singing Salome.

P: Uh hm.

C: Just the arias.

P: Uh hm.

C: "Dance of the Seven Veils." So I always, and then when I got a little older I would usher at the L.A. Philharmonic, go to concerts, things like that. So I always had strong musical interests, but no musical knowledge.

P: Were there any instruments at home?

C: No. Uh ... I never thought of myself as much of a student in grade school. And I remember one incident, when they put ... they had a kind of room which they called Opportunity Room -- I didn't really know what that meant -- where they gave me a battery of tests and, really, I thought it was ... I don't remember responding very much because of my bookishness. I tended to learn much more from books than I did from teachers. I remember a few incidents, but I would be hard pressed to say anything very much about any of my teachers.

P: Do you think they were good schools?

C: Yeah, I think they were. Boyle Heights is in East Los Angeles. At the time we moved there, there were parts of it which were very Chicano-Mexican, where the kids weren't very much interested in education. But the part we lived in was very ...
was mainly Jewish and Japanese and I . . . and the kids were very
interested in education. And my family, of course, always
assumed that education was essential.

P: How far did you go in mathematics in high school?

C: Well, let me get to that. I certainly don't remember any
mathematical . . . having any particular mathematical talent up
until the age of about fourteen. But as a consequence of this
battery of tests, they suddenly announced to me -- this was in
the 4th grade -- that they were skipping me to the 6th grade. So
it must have been some sort of IQ test or whatever. But at that
time the family moved to Hollywood, so I spent from kindergarten
through the 4th grade in . . . basically in Boyle Heights. Boyle
Heights was an extremely interesting place in those days. I
mean, there were a lot of immigrant Jews and first generation
families like my own. I didn't go to high school there because
we moved to Hollywood. Those who remained, the high school was
Roosevelt High School and it was a little bit like some of the
high schools in New York City, in Bronx and Brooklyn. I mean,
they turned out an awful lot of intellectuals. And I know a
number of them who remained went to Roosevelt High School, and
who came from Boyle Heights in those days. Then there was a
gradual movement of the Jewish population west. We went to
Hollywood and then later on to west . . . further west, and then
I ended up in Westwood, when I was going to college, which was a
typical migration for Jews in those days. Um . . . I remember
being a little bit embarrassed by my parents' Jewishness and the
fact that my father spoke with a fairly heavy accent.

P: Did you ever have any problems?

C: Not really. Not really, because I always fit (?), for one
thing, in a fairly Jewish neighborhood, and I don't really ever
recall having any . . . I remember having a fight with . . . when
I was six or seven, with an Irish kid. Seems to me that the
fight . . . that he picked on me because I was Jewish, but I
don't really . . . I just remember that I lost the fight. He was
a hell of a lot better. And it was in front of his house, not
mine, so he had his whole family out there egging him on.
(Laughter) You know, there was a certain amount of clannishness,
I think. But most of my friends were Jewish and so I didn't
. . . The Mexicans . . . there were certain neighborhoods where
one didn't go and we had some rock fights with the Mexican kids.
But I never . . . oh yes, and I learned a lot of Mexican words.
I guess there were Mexican kids in my school. I learned how to
swear in Mexican. But I don't remember forming any close
friendships with them. Most of my friends that I remember from
that period were Jewish. There was one little Japanese girl that
I admired enormously in kindergarten, I remember. I was sort of
secretly in love with her.
P: Must have made quite an impression.

C: It did. (laughter) But coming . . . Then we lived in Hollywood. When I was fourteen, my parents were separated. In fact, I think they actually became divorced. They came back together again afterwards. But at that point my mother sort of pulled up stakes, and she was invited by my uncle -- the New York uncle, who was . . . with whom she remained closest all her life, because he was only a couple of years younger . . . two, three, four years maybe younger than she, and she looked after him. In this large family, he was her charge. And he invited the two of us to New York. And I came there, but I think, I never got on with him. He felt, that I was then fourteen, that I should be doing more, I should be working, that he had never had a chance, that he had gone to work when he was ten, for a printer -- something like that. Let's see, would that work out? No, I, guess he was older than ten. He may have been thirteen or fourteen. He came over with my mother. And he found work immediately. He was very brilliant. Well, maybe that's an exaggeration, but he had a literary gift, which he expressed mainly . . . he was . . . he supported the Yiddish theater very much. Became . . . learned to be a bookkeeper, and eventually a public accountant, and worked for the baking industry and little bakers, and when the . . . and was very . . . probably the most radical member of our family. And you would have thought, when Roosevelt came to office, he was strongly supported by both my parents. But my uncle hated him with an implacable hatred because, with the NRA, and all these agencies he set up, it increased his . . . the amount of paperwork that he had to do for these bakers without increasing his income at all. And he wrote a barrage of letters to The New York Times, a number of which were printed, actually, pointing out some of the defects of the Roosevelt regime. Anyway, he felt, for whatever reason . . . I was a very shy, retiring kid, didn't have many friends, and I was sort of left to myself in New York, sent to . . . I went to Morris High School in the Bronx, where my uncle lived, for about three months, and was very unhappy. They had a different way of life. I would . . . They told me that I couldn't speak correct English, because I said sing-ging. I pronounced the g and in New York you didn't pronounce the g. (laughter) They had a testing system . . . I don't know it seems to me that it was practically sheer misery all the time. I remember going for long walks in the Bronx Zoo, and, it was in the fall of the year . . . Well, you know, that's a hard . . . that's a difficult time and it certainly wasn't made easier by the separation of my mother and father, and . . . I feeling obliged to take my mother's side. But my mother had a first cousin, who was somewhat older than she and who had some . . . and whose children were quite a bit older than I was. One was a violinist, one taught in a private school, and the other was an economist, who was already then teaching at NYU, I believe. And, at, I think it was the economist's house,
but I'm not sure, I came across a book on calculus. It was Osgood's Calculus.

P: Oh, yes, I remember it -- with a red cover.

C: Yeah, a red cover. And, just to while away the time, I remember reading the introductory part, and the first part of it, and finding that it was very comprehensible, and that I could do the problems. And, uh, it didn't work out in New York; my mother didn't like it, I didn't like it, so we went back after a short time. And, then I remember; suddenly becoming very fired up with mathematics, and bought . . . because I could do it! I suddenly discovered that I could do it. Up until then, in junior high school, I didn't think I had any particular talent for remembering (?) algebra; I don't even remember the grades I got. I remember being somewhat puzzled by, not . . . that algebra was easier for me than geometry, because I'm not visual; I tend to be more aural.

P: Did you run into a good teacher when you went back to L.A.?

C: In my senior year, but by then the die had been cast long before. I sort of . . . I breezed through. What happened was, I got another calculus book out of the library --. Granville, Smith, and Longley . . .

P: Oh, yes.

C: And I went through the whole damn thing, differential and integral calculus, doing all the examples, religiously. And then I got a book on differential equations, and did some of their . . . and I found that it was duck soup . . .

P: Was this your . . .

C: on my own . . .

P: . . . third year of high school?

C: This was . . . How many years are there in high school?

P: Four, usually.

C: Tenth, eleventh, and twelfth -- three. Cause we had junior high school, seventh, eighth, and ninth.

P: I see.

C: And, uh . . . This would have been when I was about fifteen. So I graduated when I was seventeen. So this would have been in my sophomore year, after I had taken geometry, and was taking
Charney interview

algebra. No. And then I began . . . I continued my reading. I don't remember exactly when this happened, but I began reading books on science, Jeans and Eddington, popular books. There was another guy with a kind of Polish- or Russian-sounding name, . . . . . . (?) First running into . . . well, Jeans and Eddington there was relativity already, you know, and I read them without thoroughly understanding them, but I was very fired up by cosmology, and relativity always, you know . . .

P: "The Mysterious Universe"?

C: Yeah. The . . . those things really very much attracted me. But the thing that I really worked at was not physics, but mathematics. In other words, abstract things, not . . . I didn't . . . never had any mechanical ability, never was encouraged, I mean, my father didn't . . . I see you're looking at the time. How's it going?

P: Fine.

C: So I think, in the eleventh and twelfth grades I blossomed out. And I remember such things as, you know, the period equal $2\pi$ times the square root of . . .

P: Really?

C: . . . of $g$ over $L$, or whatever . . . or $L$ over $g$. And running across that formula constantly, and eventually figuring out that this represented simple harmonic motion. I mean, that was, . . . and then running to my mathematics teacher in Belmont High School, I was going there then . . ., and showing him my great discovery. (laughter) You know, for me it was a tremendous discovery. You know, what was the thread that went through all these similar formulas.

P: Yeah.

C: And that was the first time that physics sort of (?) But I think the mathematics, so, in the eleventh and twelfth grades I was sort of the star (?)

END OF TAPE 1, SIDE 1
TAPE 1, SIDE 2

P: Now, let's see. You were on mathematics in high school. That was really quite interesting.

C: Yeah.

P: You went into that so early.

C: Then, what happened was, that in the senior year in high school, you could take solid geometry. I took all the mathematics there was: trigonometry, solid geometry, all the algebra they gave. There was no such thing in those days as advanced math or calculus.

P: Uh hm.

C: But the calculus I already knew. And then, suddenly, I discovered that I did have . . . after all, that geometry wasn't as much a mystery, and I sort of excelled in solid geometry, and suddenly, then, I got the point. I really began understanding what an axiomatic system was, and . . .

P: Was this your fourth year, your last year in high school?

C: That would be my last year of high school.

P: Was this when you had a good teacher?

C: And then I had . . . he was well known. That was in Hollywood High School. I spent my last year and, I think, a year and a half I think I was in Hollywood High School. I remember doing my chemistry and physics and . . . Now, wait a minute, now, wait a minute. That's interesting. Well, I can't remember where I had my chemistry and physics. I took a course . . . university oriented . . . (?)

P: Was there a so-called two-track system at that time?

C: A . . . Yes, I think so. I mean, you took the courses that you needed. Yeah, I took, you know, the foreign language, the . . . the mathematics . . .

P: Yeah. Did they teach calculus?

C: No, they did not teach calculus. I mean, in those days, they did not. But they would . . . This teacher gave special problems, and I always solved the problems right away, you know. Not necessarily right away. Sometimes they took some time, but it was a matter of honor with me that I solved all the problems.
P: Did you have solid geometry?

C: Yeah, I said it was that in solid geometry that, uh, . . .

P: Did you have the problem . . . ?

C: . . . that I first developed . . . that I felt I developed a geometric sense and some feeling of power.

P: . . . how you pass a plane through a cube to get a regular hexagon?

C: Uh, are you testing my . . . ?

P: No, no, that's a rhetorical question. That's one of the problems that I remember from our solid geometry course. Go ahead.

C: You remind me of a problem that Garrett Birkhoff was propounding: How do you cut a cube with a plane so as to produce two tetrahedra, or to produce . . . ? And he solves it by an inequality. A tetrahedron is defined by $x + y + z < 1$. I don't know. Anyway . . . I'm a little confused about which studies I had where. I didn't . . . don't feel that I excelled in physics as well as I did in mathematics. That came in my first year of university when, suddenly, I did excel in physics, which led me to undertake a joint major in both mathematics and physics. It was possible to do that. In other words, . . . I don't . . . and I was very good in mechanics, because that didn't require an awful lot of geometrical imagination, I think. Well, okay, should we now go to university? Or is there anything about high school . . . ?

P: No, I think you've . . .

C: I think I've covered, you know, . . .

P: You have covered it very well.

C: And so it was natural . . . My uncle visited us and, in those days, they had . . . dentists advertised. They had sort of cut-rate dentistry, and they were called mechanical dentists. I never could . . . I never could . . . in retrospect, I can't decide what the devil they really were. But I always thought of them as mechanics, not as . . . not as doctors. And my uncle tried to persuade me to forego university and study mechanical dentistry on the grounds that I could then go to work sooner and support my parents, which didn't endear him to me, and made me even more determined to go to university. (laughter) My parents, in the beginning, were not together, but they came back together I think in my first years at university. And I went to
the University of California at Los Angeles because it was the obvious thing to do; I could live at home. It never occurred to me in those days, and nobody ever advised me that it might have been better, for example, to go to Berkeley, where also there was no tuition, but one would have to . . . There was another circumstance which I'll only touch on, and that was that when I was about twelve or thirteen, I had the flu, and the doctor who attended me was a kind of a family friend, who was a chest specialist at the Duarte Sanitarium, in those days . . . since become the City of Hope, a huge facility for treatment of all sorts of diseases. In those days, it was a tuberculosis sanitarium. He came to the conclusion that, somehow, I had acquired a valvular heart lesion. And from that time until I was about eighteen years old I was under the impression that I had a weak heart, and that certain . . . it didn't prevent me from doing most athletic things, but it made me worry about them afterwards. And such things as . . . I had a friend in high school who proposed that we ship out on a cattle boat and spend a year abroad. And I was dying to do it . . . The friendship there was based on our literary interests. And I was dying to do it, but I didn't do it, not because of my . . . my parents were both, were against it, but I would have done it anyway, if I had felt sufficiently confident in my physical stamina. Even though, as it turned out in retrospect, the doctor was wrong. I never had anything wrong with my heart. It was a kind of a nervous condition that he had misdiagnosed.

P: How was it discovered that he was wrong?

C: Because when I was eighteen I had a girlfriend who persuaded me to see her uncle who was a well-known heart specialist. And he said . . . he found that I had no heart condition at all, and that what the other doctor had diagnosed could not have been possible because there would be signs of it. And then I went to see two or three other specialists immediately afterwards and they confirmed it. But it did affect my life, and my scope. Because, in retrospect, I see that you know, it would have made much more sense . . . some of my friends did go to Berkeley. You see, one of the things that happened at UCLA . . . UCLA, when I entered in 1934, was still a very provincial place, it had about five or six thousand students. It had recently moved . . . been a normal school, and the professors were rather provincial. They didn't have any really strong professors in the sciences. For example, my interest was always in theoretical physics, but there was not one theoretical physicist on the faculty. I learned all my physics from experimentalists, which meant, for example, that . . . I was always interested in relativity -- I learned relativity, such as it was, by taking a reading course from one of the German refugee mathematicians, Max Zorn, in Hermann Weyl's Space, Time and Matter.
P: A classic.

C: Yeah. And when I took ... in graduate school, when I took quantum mechanics, you know, you had a sort of standard course in quantum theory, where you learned the Sommerfeld quantization rules and the Bohr atom, but when it came to quantum mechanics, I had an experimentalist teaching the course, who was just one step ahead of his students. He would read Schrödinger .... We went through Schrödinger's paper; you can imagine ... This is the paper of which Einstein once said that, when he was asked what he thought of the paper, he said he skipped the novel, but the equations are very interesting. I mean, it's not the way to learn quantum mechanics and it's had a bad effect on me, because ... The consequence was really that I never did really learn quantum mechanics, except by browsing, you know, acquiring a speaking, but not a really .... a very strong understanding, acquaintance. And the mathematicians were the same; I mean, they were not .... the emphasis was on the education and not on the research. I had one professor with whom I became good friends, but who was not a strong research person. He wasn't able to guide me.

P: Are you talking mainly about undergraduate conditions, or did the same prevail ...?

C: Well, there was no graduate at that time. They later ... they wanted me to be their first graduate student, actually, I mean, first Ph.D. student.

P: In math?

C: Yeah, because they said that my thesis on baroclinic instability would be quite acceptable in math, and I had already passed the qualifying examinations in mathematics. In fact, I had gone so far that I had written .... I became under .... There was a genuine research mathematician who came later, and they immediately assigned me to him. His name was T. Y. Thomas.

P: Sure. He wrote on hydrodynamic stability.

C: Yes, he did.

P: He later went to Indiana.

C: That's right. But in those .... but he had been a student of Hermann Weyl's, but he wrote on the mathematical aspects of Riemannian and generalized Riemannian spaces, and their various invariance. He was really first rate. I went with him because I thought I'd learn something about relativity, but he never mentioned the physical motivation for all his mathematics. Never.
P: Are you talking about your graduate years?

C: That's graduate already. I'm skipping. But to come back. Even though . . . I had nobody to advise me in the early days. I mean, my parents were quite incapable and not interested . . . well, you know, I was their . . . And the funny thing is, I had a very . . . I had a very . . . possibly because I didn't have a strong father image, I didn't have any strong belief in my own ability. And so, when they got me to take college algebra, it was a joke. They allowed me to take . . . they wouldn't excuse me from college algebra, but they allowed me to take college algebra and differential calculus at the same time.

P: Oh yes.

C: Well, hell, I knew much more than the differential calculus, so . . . then I took differential calculus, and then I had to take integral calculus, and then elementary differential equations. I knew all that stuff. So basically the first two years of mathematics I knew already. And that inculcated a lot of really very bad habits. I didn't have to study at all. Nobody encouraged me, and, all right, what else could I study? They had one fellow who was an expert in advanced Euclidian geometry, save the mark! where you could make all kinds of difficult constructions.

P: Good lord!

C: You know, a circle tangent to three circles, by inversion techniques. You know, you would invert with respect to a given circle. And that was an upper division course, but I took it as a sophomore. And I thought I did well, but that was, I think, the only B I ever got in mathematics. All the rest were A's. But it kept me from graduating summa cum laude; I graduated magna cum laude.

P: I hope that your mathematics . . .

C: Then they had . . . it wasn't called projective geometry, but you learned, where you use homogeneous coordinates, um, . . . I took that also in lower division, did well, but it led to nothing. That led to nothing. The one professor who seemed to . . . with whom I had some kind of rapport was a man whom I admired greatly. He was one of the founders of the . . . you remember Upton Sinclair and the Cooperative Movement?

P: Um hm.

C: Well, he sort of led that in the San Fernando Valley. He had a farm. And, I think, at least two of his children became mathematicians. He himself was a number theorist, but . . . He
hired me as an assistant when I was still an undergraduate. But it was just to check elaborate calculations, where he was able to extend the smallest number that would satisfy Fermat's Last Theorem by a factor of a million, or whatever, but, you know, it was so high already that it added nothing theoretically to our understanding. He had been a student at Columbia. Titch, or Hitch, or something like that. Well, maybe this is sour grapes. If I had real mathematical ability, I may have risen above that and started to do mathematics on my own. But I really had nobody to inspire me mathematically. The head of the department had studied in Germany and his name was Earl Raymond Hedrick, but I never met him until I took his advanced calculus course, which was a sort of celebrated course. He was an excellent teacher. But there again, you know, advanced calculus doesn't by itself lead to very much. I think, in retrospect, that if I had gone to one of the Ivy League schools or to Berkeley, they would have recognized my mathematical talent such as it was, and would have given something that would have challenged me.

P: Yes. It certainly sounds like an object lesson in ossified pedagogy.

C: Yeah.

P: Well now, Jule . . .

C: You know, I never . . . and I didn't have the self-confidence to simply go ahead on my own, so I simply did what they . . . you know, I took the courses, got A's, took the physics courses, got A's. For example, I took electromagnetism -- advanced, if you please, electromagnetism. That was not . . . you took the standard physics sequence in the first two years: mechanics, heat, light, and electricity. And then, after that, you took quantum mechanics, you took electromagnetism. This guy never introduced Maxwell's equations, never. He did everything by neat little geometric tricks.

P: Perhaps Maxwell would have appreciated . . .

C: I took advanced optics from an experimentalist, who never -- never -- gave Kirchhoff's work on diffraction, or refraction, or diffraction. In other words, there was no connection with electromagnetic theory. There was no theory! And, you know, it didn't send me.

P: Mm hm.

C: None of that stuff inspired me. We learned . . . That was a good book, but it was an experimentalist's book -- this famous physicist at Johns Hopkins -- his Optics.
P: Nineteenth-century physicist?

C: No, twentieth, early, well... He was really a great optician.

P: Wood?

C: Wood! J. W. Wood [R. W. Wood (GWP)]. I think, for a person who felt physics in his bones, and who was experimentally inclined, it was a great book. Wood was a great experimentalist. But it didn't do anything for me because it wasn't theoretical. I was really interested in theoretical physics, and there were no theoreticians.

P: Interesting. But now, you wrote a master's thesis?

C: In mathematics.

P: In mathematics. What was the topic?

C: No, after mathematics... after my mast... they didn't require a thesis.

P: Oh.

C: But after that, T. Y. Thomas assigned me a topic on curved spaces, having to do with sort of generalizations to arbitrary spaces of Kelvin... things like Kelvin and Stokes theorems. And I worked that out, using what now probably would be called Banach spaces. Nobody told me what Banach spaces was. It's interesting that... well... And he said, I'll shut up and you can get your doctor's in mathematics. I had spent about a week or two. In those days, you know, I had a very... both lack of confidence, combined with a deep admiration for genuine research. And to me this was not genuine research. This was no important new idea. So I never took him seriously.

P: How fortunate.

C: And he didn't push me.

P: How fortunate.

C: I still have the thesis, or, now it wasn't a thesis, it was just a paper.

P: Well, now, you then at the same time, or shortly after that, became a university fellow in mathematics? Is that right?

C: Yeah, after...
P: What was the basis of that?

C: Well, they thought I was good. That was the highest thing they could award in those days.

P: Was this after your work for Thomas?

C: That was after my work for Thomas.

P: This was when, in late forty-one or 1942?

C: That was in forty-two, I believe.

P: Forty-two. Well, now . . .

C: You see, I graduated in thirty-nine. I took an extra semester. I think the reason was, I didn't want to strain myself, or something like that.

P: You graduated in thirty-eight, didn't you? It says here . . .

C: I entered . . . I entered in the winter semester in February of thirty-four, so I should have graduated in February of thirty-eight. I should have graduated in January, whatever, of thirty-eight. They had a semester system. But for reasons I don't quite remember, I think something having to do with I-don't-remember-what. Ah . . . no, wait a minute, I did graduate in thirty-eight, I did graduate in thirty-eight, but in June of thirty-eight. That's right.

P: Tell me . . . coming back to forty-one and forty-two, what effect did World War Two have on you?

C: Well, all right, let's immediately jump to that point. I was a graduate student for . . . in thirty-nine and forty . . . Seems to me I was a graduate student for two years, when I got this fellowship. So I held the fellowship in fort . . . no, I got the fellowship in 1940, I think.

P: Mm hm.

C: And I was . . . still working for Thomas, and I think I must have done this curved space paper in . . . I continued to take graduate courses in physics but by that time I was a mathematics major, not a physics major.

P: You referred to that as a "curved space paper." Did you actually write it?

C: I never published it. But I did write it, yes.
P: As a thesis?

C: No, I . . . he wanted me to submit it as a thesis, but I didn't feel that it was public . . . you know . . .

P: No, I meant as a master's thesis.

C: No no, we didn't have a master's thesis.

P: So, you were then . . . you were a teaching assistant in math?

C: Well, that's . . . yes, I was a teaching assistant in mathematics, that's right. Until I got the fellowship.

P: And that fellowship set you free, did it, essentially, . . .

C: Yes, it set me free from teaching.

P: But then you began teaching again, in 1941?

C: That's correct. What happened was that T. Y. Thomas conducted a kind of a seminar, and at that point he had become interested in turbulence. And he worked out a proof that all periodic pipe flows were stable for a sufficiently small Rayleigh number. I suspect that the proof was not using energetic . . . using the energy equations.

P: You say it was not using the energy equations?

C: Using the energy equations. But I think that Lorentz had probably done that long before, I seem to recall. But he used . . . Thomas had a feeling for physics, and this was really the first time he came out . . . I mentioned to you that he wouldn't talk about relativity. And I didn't take a relativity course, or reading, from him. It was with Max Zorn, the author of the famous Zorn's lemma, which is an alternative to the Zermelo postulate. He was a funny guy. He ran a seminar, which I contributed to later on. I once . . . In the early days, you couldn't get a job at UCLA in the sciences if you were Jewish. And I remember . . . Glenn James, this chap who founded the Cooperative Movement and who was a good friend of mine was a Quaker and he had no prejudice and was very down on the other members of the faculty. There was one chap whom I always suspected of being Jewish, but he never mentioned it. And Max Zorn was clearly a Jew, but he too was a completely emancipated Jew, and his wife was, you know, a German fräulein, er, hausfrau, with a vengeance. (laughter)

P: What teaching duties did you have?
C: Why was I talking about Zorn? Oh yes. I once ran into him and he had a score, and we started to chat and I noticed that it was one of Bach's fugues, and I said, "Oh, you read . . .," I said, "You're interested in . . .," I said, "Are you . . . do you play . . . Are you studying Bach?" He said, "No, I'm . . ." he said he was just fascinated by the geometrical arrangement of the notes.

P: Good lord!

C: He was that . . . he caught you off balance in this way constantly.

P: Yes.

C: And . . .

P: Was he pulling your leg?

C: No. I believed him. No, I already knew him well enough to know that.

P: Teaching.

C: Oh yah. Well, to come back. I had this fellowship when I took this course, and one of the people that was invited to lecture in the seminar was Holmboe, and Holmboe taught really very elementary stuff, elementary hydrodynamics; you know, the kind of stuff that you would find in his elementary textbook. and I wasn't terribly impressed by it, but maybe impressed enough to say, well here meteorology is a fairly serious science. Up until that point I knew zero about meteorology. I once in an introduction to this . . . to Bjerknes's . . . J. Bjerknes's selected works -- I was asked to write a little, as a quasi-former student of his; he and Mintz and Namias . . . he and Mintz and I were asked to write something, and I wrote that I came to meteorology not being fully convinced that wind was moving air. (laughter)

P: I believe the department was a very young department at that time.

C: Yeah, it had just started in 1940, the year before, with Bjerknes, and Kaplan had sort of brought Bjerknes there, and Holmboe, and . . .

P: No, that's not entirely so.

C: Rossby had a hand in it.

P: Yes, he had something to do with that.
C: I think you're right. That's right. Kaplan took credit for it. I guess he knew Holmboe; they were both running the war together. And Holmboe was a very amiable fellow, and, you know, we became acquainted, and . . . At that stage -- this was in the spring of 1941, before Pearl Harbor, but at a time when it seemed perfectly obvious that we would soon enter the war. One didn't know just when, but it was fairly reasonable, and some of my friends were simply joining the army, others were getting positions in defense industry, and what seemed to be open to me at that time, if I wished to work in defense industry would be aerodynamics, or aeronautics. They were taking people with a mathematical-physical background and putting them to work on things like stress analysis, which didn't require a tremendous amount of hydrodynamics, but, it meant that if one went into that you would eventually become some kind of an aerodynamicist, that would be the thing that I would have . . . Although at that time you know, I had had a course in theoretical mechanics, where they introduced the Navier-Stokes equations, then proceeded to do nothing with them, except maybe talk a little bit about potential flow. And a friend of mine had gone to work for Lockheed and that seemed to be an open possibility. And at that point Holmboe offered me an assistantship, a job that . . . I could take the wartime training course, which was about an eight-month course in the fall . . . Starting in the fall, the second course was given in the fall, maybe a nine-month . . .

P: A nine-month . . .

C: . . . and still help him as his assistant. Of course, I would have been more advanced in science than most of the students who applied for that course. At that point . . . I've forgotten how this occurred, but I once heard von Kármán lecture at Cal Tech. He gave some kind of a popular lecture on applied mathematics. And I guess I knew somebody out there; I don't remember the circumstances. But I saw him, and therefore knew something about him, must have heard something about him before that. And I don't remember how this happened, but . . . I might be able to look it up, but I doubt it. I decided that I would ask his advice. And at that time, he had kept an apartment in downtown Los Angeles because he was commuting so much between Cal Tech and the aircraft industries.

P: You talk about . . . von Kármán?

C: von Kármán, yeah. And so we made an appointment that I would see him in downtown Los Angeles in this little hotel room. And there was his sister who answered the door in her kimono, as I remember it. Must have been early in the morning. And I told him what my background was, and I said I seem to have . . . I have the problem that if I . . . that it looks like the war's coming and that if I wanted to do anything, the two things that
suggested themselves would either be to go into the aircraft industry or to . . . that I had this offer in meteorology. And he said that . . . he thought that, given my background, that meteorology would be a preferable field, on the basis that my interest in theory was such that . . . he said that aeronautics had become much more of an engineering science, and since I had no real interest in engineering, he said he thought that there were many more unsolved problems, that this was a new field . . . meteorology was a new field, and that it might be more interesting for me.

P: Very perceptive advice.

C: Yeah. Now, I don't . . . I think it would be perhaps an exaggeration to say that it was his advice that determined me to go into meteorology. It was probably the easiest thing to do; I already had an offer. And a number of my friends were doing the same thing.

P: But it certainly didn't detour you.

C: No. So, I signed up for the course. And I took the course, this wartime training course, and as I would . . . von Neumann [Holmboe (GWP)] would write up notes, I would. I wasn't a very good assistant, because von Neumann didn't . . . I mean, not von Neumann, Holmboe didn't send me. I mean, he was another pedant. He was very careful; he was very visual; he could only understand things geometrically. He had very little analytic ability. But, you know, he would give, you know, proofs of some of the standard theorems, and, you know, he had to see vorticity geometrically, and . . . and he would give me rough notes that I would subsequently work over. That was my job, and I don't think I did the job very well.

P: Worked them over with a view of mimeographing them?

C: Yeah.

P: For the students?

C: Yeah. And I think he gave a . . . I can't remember. He had this course in dynamic meteorology, which I took, but I was sort of . . . he assumed that I would get an A, so I helped him grade the papers. Which is true, I mean, it was very elementary. Uh . . . there were a couple of really very good students in that course. One was Francis Johnson, who I remember particularly. Another was a fellow named Sherman, who went into naval meteorology. He was killed in an automobile accident.

P: Yes, I remember him. He wrote something about tropical meteorology.
C: I think he did, yeah. Another was Harold Shniad, who went into the air force as a meteorological officer, and sort of disappeared from view, but he had been a mathematical fellow student, and I thought that he had great promise. But nothing much came of that. Another was . . . well, there were a number who subsequently . . . there was Gustin, who together with, wrote this book with Holmboe, but then went back into mathematics and, I think, is now at the University of Indiana, or someplace like that. You want to turn it off for a minute, so I could . . .

P: Well, Jule, um . . .

C: Or maybe we're just about, um . . . this might be a good place . . .

END OF TAPE 1, SIDE 2
TAPE 2, SIDE 1

P: This is a continuation of the interview with Jule G. Charney. This second session of the interview is being conducted on Tuesday, the 26th of August, 1980, in Boston at the Holiday Inn. We're off and running.

C: Okay. Well, George. Well, in thinking over yesterday's interview, I realized that I have a habit of starting a story, which is meant to illustrate a point, and then getting involved in the story and forgetting the point. And I started telling you about when I skipped a grade, a year, from the fourth to the fifth to the sixth.

P: Yes, I remember that.

C: But the point wasn't that, it was ... I mean, that fact in itself is not important, but it was meant to illustrate a characteristic of mine, namely that it was ... the following anecdote I'd forgotten to tell you, namely that, when my parents found out, my father presented me with a watch as a reward. And I never felt that I deserved that reward, because I had done nothing to skip that grade. I hadn't worked for it.

P: It all came naturally.

C: It just came. And that's really very characteristic, and when I told you the story about my ... this paper that I wrote for Thomas when I was in graduate school in mathematics and then he told me that if I polished it up it could be acceptable as a doctoral dissertation, I didn't regard that as ... I hadn't really worked hard enough. And, I think, in retrospect, I was right, that it wasn't of any note. And I'm very happy, in a way, that I didn't submit it. I would have gotten my degree in 1942 or 43, rather than in 1946. No, I take that back. I would have gotten it in 1940 or 41. It may have been of some value at that stage, but, as I say, in retrospect, it's worked out for the best, I think.

P: It would have been a disaster for meteorology had you ... 

C: Well, in all probability I would not have gone into meteorology because, as I mentioned, meteorology was something totally unknown ... field. And I think that that theme threads through my whole professional life. I mean, that ... well, that will come out in the sequel. That it has at least been difficult for me to accept things for myself -- accept things.

P: I trust that you didn't have the same pangs for the Symons Gold Medal or the ...
C: Well, maybe so. I'll come to this later, but the first thing that I felt I really had worked, not for, but... my paper on baroclinic waves, was something that I had worked very hard on, and I felt that whatever came to me because of that was deserved. And that really changed my whole attitude toward myself, in a way. It was the first thing that was -- I'm anticipating myself -- but it was the first example of truly what I thought was truly independent work. Nothing that had been assigned to me. And my first experience of genuine research, what I, in my own mind, considered to be at any rate genuine research. And what I had done before that was not.

P: Yes, I understand what you mean.

C: Well, it seems to me that... Okay, where were we?

P: Well, I think, it seemed... I feel that we've completed the story up to the time you began your doctoral research.

C: Well, I didn't begin my doctoral research when I began taking this wartime training course in meteorology.

P: True.

C: It came...

P: True. And I think you had...

C: Oh, but... forgive me. This is going to make things difficult, but there was another point that... it seems to me I was a little bit cavalier in dismissing the entire mathematics department as not research oriented, and simply oriented toward teaching at UCLA. That wasn't true. I mentioned that toward the latter part of my stay, in mathematics, there were... when it was decided to establish a graduate school, they brought... they did bring this one refugee, Max Zorn, and they also brought T. Y. Thomas, whom I've already mentioned. But even before that, there was a modern research field which was pursued, and that was, I think, mainly under the direction of William Whyburn.

P: William...?

C: Whyburn. W-h-y-b-u-r-n. He was the brother of George Whyburn of the University of Virginia, who was a very well-known topologist in those days. And he himself, I think, had studied with an even better-known point-set topologist at the University of Texas. I can't think of his name now, although I knew it well then, who used the kind of Socratic method of teaching -- that he would not present the subject matter of the course, but he would present the theorems and the students would work out the theorems -- and this is the method that Whyburn used. Got a lot of
experience in problem solving that way, but you never really . . . the problem was that you very often couldn't see the forest for the trees. You got a lot of specialized technique, and the one field that I knew somewhat and which was of absolutely no use to me in my later work was point-set topology.

P: Oh really?

C: Plus a little bit about generalized coordinate spaces.

P: Well now, there is a point that I would like to touch on, at least briefly. You had some teaching duties, partly in physics or mathematics, and then later in meteorology?

C: I only was a teaching assistant in meteorology . . . in mathematics, not physics, because as a graduate student I was only a mathematics major, not a physics major.

P: And did you lecture in that capacity, in mathematics?

C: Yeah. I lectured . . . they gave me college algebra to teach.

P: And what did you teach in meteorology?

C: In meteorology . . . well, that's slightly interesting. It was . . . As I mentioned to you, while I liked Holmboe as a person, and we later became friends, I don't think I performed extremely well for him as an assistant in the first . . . in this wartime training course, because my way of doing things was so very different from his. And so, at that time he had decided to write this book on introductory meteorology . . . dynamic meteorology. And at that -- well, it wasn't quite at that time, but it may have been, oh, possibly, a year later -- and he chose as his assistants and who . . . actually, they became co-authors: William Gustin and George Forsythe.

P: Yeah.

C: George Forsythe already had his doctor's degree in mathematics from Brown University. And Gustin was a contemporary of mine in mathematics; subsequently also left meteorology and became . . . took his degree in mathematics and . . . Forsythe took a position at Stanford and became head of their computer . . . their

P: Yes, I know of his subsequent . . . career. Did you ever have any contact with him . . . ?

C: Yes, I did. I invited him to Princeton when I was there. At that time we were concerned with efficient methods for solving
elliptic equations and he had worked on that problem. And so we discussed it and . . . we never actually used any of the methods that he had proposed. His methods were much more general. We ended up using a modified relaxation technique that (?)

P: How long was he there? Do you recall?

C: Oh, not very long, maybe a month or so.

P: Oh, uh hm.

C: Well, you said, well, to get back to my doctoral work . . . well, you asked me . . .

P: Teaching.

C: About teaching. I . . . After, the first . . . this introductory course, I was assigned the job of sort of substituting for Joseph Kaplan who taught the course in radiation. And I had taken his course in radiation, during this nine months. He offered a course in radiation, in which . . . but he was so busy with his wartime . . . military consulting activities, that he most of the time simply read Chandrasekhar's book on radiation to us. And we also went through Elsasser's Harvard monograph, which was by far the most useful.

P: Now you've answered a question I've had in mind to ask you later, namely, how did it come about that your first publication was the chapter on radiation in the . . .

C: It came about in the following way: After I had taken this course, in the second year, I was asked to substitute for Kaplan when he wasn't present. And it ended up that I took over the entire radiation course, and, you know, being one step ahead of the students, it wasn't altogether fun, in a way, and some of the students . . . by that time there were some really . . . some very good students, sent as naval officers. But they had just been commissioned from the universities. Many of them had their doctor's degrees and were just as smart or smarter than I was, and I had a hard time getting them to take me seriously. (laughter) But I learned some radiation, and then when McGraw-Hill decided to do this, well, when Gene Bollay and I think a couple of others decided to get out a handbook on meteorology to be used by naval meteorologists with McGraw-Hill as the publisher, they asked Kaplan to write a chapter on radiation, but Kaplan was too busy and he asked me to co-author the article, but he was too busy even to be a co-author, so it ended up that I wrote the article.

P: It's a pretty good one. (?)
C: It's very simple minded.

P: But you never returned to radiation.

C: No. No, I mean that wasn't my primary interest. I mean, right from the beginning . . .

P: Did you teach any other courses?

C: Well, yes, I mean, there was much of . . . I suppose if I had gone more deeply into radiation I might have been attracted by it, because there was much to be done in those days.

P: What other courses did you teach?

C: Well, that was . . . that, and the laboratory.

P: Uh huh.

C: And let me say now that I hated the laboratory when I took the course, because I couldn't see the sense in spending so much time drawing maps, and we have the same problem with our . . . with our current students at MIT and, I know, elsewhere, that students who are more theoretically inclined find it a great chore to go through the . . . and, fundamentally, I can't agree that the best way of learning even synoptic meteorology is by painstaking analysis of maps. The theory in those days was that only in this way would you familiarize yourself, sort of at first hand, with the meteorological situation. I know, we . . . I spent a lot of time plotting maps; that was part of my chore and, you know, to me that was a total waste of time. It was something that could have been done . . .

P: But you feel that this is not the way to learn synoptic meteorology?

C: Well, no, I have, I have, I'm of two minds about that, because I'm really . . . in the end, I'm very grateful for having been exposed at such . . . so very directly to actual atmospheric phenomena. In those days, of course, you only had observations. The maps that we analyzed were just from the United States or North America at most. And we never got very much of a picture . . . we did study some, and made one or two global analyses, but they were very incomplete, because then there was no such thing as a global network.

P: Well, would it be fair to say, as you look back, you put your reaction to weather maps in the category of a child's dislike of spinach?

C: Yeah.
P: Or of not wanting to practice the piano?

C: Exactly. But afterwards the child is very grateful for having been forced to learn how to play the piano. I always liked spinach. (laughter)

P: Jule, what led . . .

C: And, you know, well, I think, you know, it's part of the philosophy of training in meteorology. I still think that a method has to be found for indoctrinating students in the phenomenology of their science, and there's no . . . and the alternative to the physics laboratory in meteorology would have something to do with the study of weather maps, but it seems to me that the study could be . . . that they could be assigned problems or assigned experiments meteorologically, in the way in which you're assigned experiments in physics, and . . . and this rote . . . I mean some people are naturally talented at drawing isobars and some people aren't, but I don't think it matters that very much. Some of the very best students were the worst isobaric drawers.

P: Do you think the coming age of computerized instruction in synoptic meteorology . . . do you look . . . do you look upon that favorably? By this I mean the possibility of having instantaneous access to large data banks, even to analyze maps or at least maps that have . . .

C: Yes, I think if it's used constructively. I mean, after all, the computer not only can plot the data if you wanted it, but it can make objective analyses.

P: Right.

C: I think a person who has made subjective analyses of weather maps has a deeper appreciation of how inadequate many objective analysis schemes are, and if you simply accepted the machine product as reality, it would be very dangerous. And I think that . . . that the tendency to rely too heavily on the machine has occasioned a lot of errors in operational work, and, not only in operational work, but in research, synoptic research. I mean, some of the early work on the general circulation, on the various transport processes in the atmosphere, which relied naively on extrapolations between stations thousands of kilometers apart. What one can see easily now was naive. And the same thing exists today. In other words I think, it's more than a mystique that one must be exposed to the data, and precisely how to do that isn't clear. But at the same time I would agree very much that machines could be . . . can be used very constructively to produce various kinds of analyses and so you could look at the essentials of the atmosphere.
P: And one doesn't have to go as far as the analysis. I think the mere acquisition and display of the data is a huge step...

C: Of course, you must remember that in those days, in 1941, forecasting was a very subjective process. There were no... I mean, nowadays when they teach some of Petterssen's rules for extrapolating troughs and isobars and, you know, spend appreciable amounts of time on that. It seems to me that this is pedagogically... this is insulting to a graduate student.

P: Um hm. Jule, I want to talk about your doctoral research. What... It would be interesting to know what led you to select the problem of polar front stability and...

C: Not polar front.

P: Well, but it...

C: No, there's a difference.

P: Yes, of course there is. Of course there is. But in those days, it isn't... I mean it isn't obvious retrospectively that that difference was fully understood. In fact I think that was part of the problem.

C: I think it isn't fully understood to this day.

P: Well, why then did you not fall into the trap of thinking that Solberg and J. Bjerknes and Holmboe had already solved the problem that you were... (?)

C: No, I didn't... well, in the first place, it was very clear that they hadn't solved the problem. I didn't know Solberg's work that well, although I had begun to read in Physikalische Hydrodynamik. I learned more about it when I went to Norway later, but... well, to begin with, Bjerknes not only lectured on standard synoptic meteorology. He was very much then preoccupied with his isobaric channel approach to... with the long wave, and I had... during the course of which time he, in his lectures, he covered the material which is contained in his 1937 paper on long waves and cyclogenesis. I've forgotten the exact title. And he told us about his isobaric channel method in the training course, during the training course, so we knew that. But my interest came about in the following way: After I had taken the course, in the spring... I'm not even sure that I'd even finished the... this was in the spring of 1942.

P: Uh huh.

C: The course was still going on. It began in July of forty-one and finished sometime in the spring. It may have been just
after. I think it finished ... I'm not sure if it was nine months. It may have been only eight months. In any case, it was either just before or just after the end of wartime training. Neiburger had come as a lab instructor and Fletcher. Neiburger had been a student of Rossby's. So had Fletcher. And it was from them ... I'm quite sure it was from Neiburger that I heard of Rossby's 1939 paper on the quasi-permanent centers of action of the atmosphere and the Rossby wave so-called. And he suggested, or it may have been Holmboe, that I should review that paper in a rather ad hoc seminar. I don't think this was a regular seminar series. Or maybe it was a regular seminar series. So I read Rossby's paper and the first thing that occurred to me was that why shouldn't the Bjerknes channel method give -- since it was really the upper wave -- the wave that Rossby was discussing was the upper wave which had been identified by Bjerknes. In his 1937 paper, of course Bjerknes not really identified the upper wave but related it to the frontal wave. And so, what I did, simply, was to first of all calculate the velocity of the ... calculate the wavelength of the stationary wave by, you know, demanding ... by using Bjerknes's method demanding that the net transport in an isobaric channel between trough and ridge be zero. Using the gradient-wind equation for calculating the velocity of the trough and ridge, making use of the beta effect, and lo and behold I got exactly the same result that Rossby had gotten for a nondivergent atmosphere. And then I argued that ... that if you had an arbitrary wavelength -- not the stationary wavelength -- that the velocity defect between the zonal velocity and the stationary ... in other words, the critical velocity, beta L squared over 4 pi squared, the velocity of the wave should be c ... U minus U sub c. Now ... so, in a sense, I did not use Bjerknes's ... one of his principal ideas, and that was to use the isobaric channel as a means -- and the gradient-wind equation -- as a means of calculating the divergence. I later began to real ... later realized that that was basically falacious, that if you use ... well, that ... All right. Fundamentally -- I don't know whether I could work this out just at this minute -- but it seems to me that one can show that to calculate the divergence of the gradient wind is basically for small amplitudes, equivalent to taking the primitive equations of motion and evaluating the acceleration of a geostrophic wind. So that is a form of the geostrophic approximation. And it is ... and if you take the divergence of the equations ... if you solve the equations of motion for the Coriolis field, cross it with k so that you have the velocity, take the divergence of the velocity, and then with a few further approximations involving the density, you get the geostrophic vorticity equation.

P: That's right. In fact, that's the way Eliassen did it.

C: And so ... well, Eliassen simply ... no, Eliassen didn't
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do . . . didn't get the divergence, didn't get the vorticity equation. Eliassen simply substituted the geostrophic velocity for the velocity in the acceleration for one . . . When you take d by dt of v, he substituted the geostrophic velocity for v.

P: We can come back to . . .

C: If you simply . . . if you take the divergence of that you, of course, you get the geostrophic vorticity equation. You don't get the geostrophic vorticity equation without making some further . . . you would get the semi-geostrophic equations by Eliassen's technique. In any case, I thought that that was a neat derivation, but there was surprisingly little discussion of it at the seminar or subsequently . . . until, it seems to me, that Bjerknes and Holmboe made use of the basic idea in their 1944 paper. But it's perhaps a matter of personal opinion about whether they made use of my idea or rederived it. However, this did start me off on a more careful study of the long waves. It was absolutely clear that one could not hope to deal with baroclinic instabilities with the single-level model and so I began to look at a baroclinic model. And I was very influenced by Rossby's work and Bjerknes's work feeling that the beta effect was . . .

P: It seems to me that you made a little jump here. How did the idea of a stability problem come up at all? Where did that come from?

C: Bjerknes himself had not spoken in terms of stability or instability but in terms rather of the deepening frontal wave and deepening upper wave. And he did argue in terms of the tendency equation. He used the isobaric channels and the tilting of the trough to show that over the trough at the ground you have falling pressure.

P: Do you consider, uh, this is perhaps jumping ahead, but do you consider your solution of the problem of baroclinic instability to be in a sense a vindication of Bjerknes's subjective ideas?

[. . . long pause, about 40 seconds (GWP)]

C: I think properly interpreted. What he was calling the tendency equation . . . If one uses the gradient wind, not the geostrophic wind -- there's an approximation involved there, too -- to evaluate the mass divergence, the integrated mass divergence, you do indeed get an integrated form of the geostrophic vorticity equation, or what we call the . . . in terms of the quasi-geostrophic approximation one has to make some further approximations for small amplitude. But, in essence, you do. Ah . . . and, to that degree, and recognizing that the
tilting of the trough and the advection, uhm, was essential, I think that Bjerknes's subjective -- well, what did you use, the word qualitative -- ideas, about causes of deepening of the trough were essentially correct. Yes, I would regard this work as a vindication of his qualitative ideas.

P: Did he regard it so?

C: I believe so. Although ... Bjerknes was a very reticent man. I think that both he and Holmboe were very pleased by my work ... .

P: Do you think . . .?

C: . . . and did regard it as a mathematical . . . Vindication is a good term because, of course, their theory was not a three-dimensional theory.

P: Um hm.

C: It was a kind of a necessary condition for deepening. And, you know, with great pain they were able to derive the Rossby formula. And I remember Rossby, uh, Holmboe . . . I have letters . . . a letter from Holmboe in which he asks about how one applies the geostrophic approximation to the case of a free surface, to an ocean with a free surface, because they had difficulty already in dealing with that. In other words, their method wasn't . . . their method was not deductive, essentially; it was synthetic and not capable of solving the problem. Ahm. And, I . . . at the time, I first began looking at the baroclinic problem, uh, . . . I don't remember whether it was right at the start or shortly afterwards that I began looking upon it as a stability problem. I just wanted to see how the problem would . . . what would be the way to pose the problem mathematically as a three-dimensional problem.

P: The problem being what?

C: The problem of the motion of the long waves in a baroclinic atmosphere, that is, in a basic flow, as a perturbation of the basic flow in which there were horizontal temperature gradients and vertical shears of the zonal wind.

P: Are you saying then that what you started out to do was to develop a mathematical theory for Bjerknes's upper wave, and that that . . .

C: Well, for Bjerk . . ., for . . . for, uh. Well, yes, if you please.

P: And that that soon after turned into a stability problem?
C: That's right.

P: You mentioned, a moment ago, that you were not familiar with Solberg's work? what about the . . .

C: I had heard from Holmboe about it, but he didn't lecture on it and I read a little . . . and I tried . . . there were copies of Physikalische Hydrodynamik available and I read some of Solberg's and Oseen's contributions.

P: What about the work of the, uh, Norwegian . . . of the Bergen School in general, uh, as represented not only by Physikalische Hydrodynamik but numerous publications in the Geofysiske Publikasjoner?

C: Well, Bjerknes, of course, lectured on his contributions.

P: Yeah.

C: And Holmboe . . . I knew very little about the . . . I knew about Bjerknes's work, his synoptic work, and his work together with Solberg, but I knew . . . what I was able to find out about their theoretical work would . . . came from my reading of Physikalische.

P: That brings up the question of what guidance did you get, if any, from . . . directly from Bjerknes and Holmboe during the time you were . . .

C: None whatever.

P: . . . working with them?

C: None whatever.

P: Did you seek such guidance?

C: No. Because, in the first place, as I mentioned, uh, Holmboe's approach was geometric, not analytic, and, uh, . . . we just didn't look upon problems in the same way. And I would talk about . . . I would give sort of progress reports in which there was almost no discussion. I mean, uh, . . . you know, I stumbled around making various kinds of approximations to get something that was . . . that was feasible, amenable to attack, and, at first, I must have done a lot of foolish things. I know I did. Uh, but I got no assistance, nor, of course, I think it must be said that the problem was not suggested to me.

P: How long do you think . . .

C: Because, you see, I mean . . . it was . . . I would say if I
had an intellectual godfather at that stage, it was Rossby, not Bjerknes.

P: How so? What did you . . .

C: It was Rossby's papers . . .

P: Oh.

C: . . . that I had begun to read.

P: You say "papers," plural?

C: Well, yes, because I also read his pap . . . at that point I think, one on, well, no, his earlier paper on adjustment. but I think the seminal paper was his 1939 very simple-minded paper.

P: Yeah.

C: Uh, because it was an analytic approach that I could understand.

P: Um, how . . .

C: And, of course, no . . . I guess I did know about . . . well, you know, I knew . . . yes, I did know about stability problems, because in Thomas's seminar he discussed the Orr-Sommerfeld, the Orr, the . . .

P: Oh . . . Uh hm.

C: . . . the Orr-Sommerfeld equations, but he was determined . . . uh, but as a mathematician he couldn't accept their results because they were infinitesimal amplitude.

P: (laugh)

C: And he wanted to get criteria for stability which were finite amplitude, and he was able to do that, but only for sort of ridiculously small Rayleigh numbers. (?) Uh . . .

P: Reynolds numbers . . .

C: Reynolds numbers . . . uh . . .

P: Jule, we'll have to break momentarily. This might be a good point to do it because I have to turn over the tape.

C: Okay.

END OF TAPE 2, SIDE 1
TAPE 2, SIDE 2

P: Let's see, where were we? Uh . . . You were familiar, I gather, with the Bergen school in a general way; what about such things as Kotchin's work?

C: Had never heard of it.

P: Few people had I think in those days.

C: Is . . . is the recorder going?

P: Yes.

C: Okay.

P: (?) Um. Oh yes, I wanted to ask: How long an interval do you think there was between the time that the stability problem became more or less clearly set in your mind and the time -- not that you had finished the thesis, because there always are mechanical things that you have to do, and purely routine work -- but how long before you feel you had climbed up to the summit where you could see clearly that you had the solution and it was largely a question of turning the crank at that point? How long a time do you think that was?

C: Three years. See, I think that I . . . It was not accurate to say that I started on my thesis from the point that I derived the Rossby formula by the isobaric channel method. I had no notion of . . . that I would now write a thesis at all at that stage. I considered -- again -- that I, you know, that . . . that I was a novice in the field and that I simply didn't know enough to write any kind of a . . . acceptable thesis. And, it's also . . . it's not accurate to say that I immediately began looking for the baroclinic wave solutions. I, ah . . . there was a period of, oh, nearly two years, I think, in which I was mainly occupied in teaching and, uh, reading to a degree . . . and not working directly on the baroclinic wave problem.

P: It must have been in this interval that you wrote the paper on radiation?

C: Yes. Right. I think it must have been, oh, possibly sometime in 19 . . . maybe in early 19 . . . or middle 1944 that I began what could only be called really doodling with the equations of motion. Uh. In other words, I . . . at that stage I didn't have a clear concept of where I was going. I knew . . . I think that the formulation . . . I had . . . I had formulated the model . . . that is, uh, and I was a little concerned at that point with formulating an appropriate model which had a troposphere with a uniform horizontal temperature gradient. But
I didn't have enough intuition to be able to say that the whole atmosphere is a troposphere and so the original formulation was with a stratosphere with no temperature gradient. And then there was a question of fitting the appropriate boundary conditions between the troposphere and the stratosphere. I think I had, uh . . . well, I think, no, actually, that wasn't the first . . . no, the model wasn't even formulated that far. I began simply looking for a tropospheric solution, but not worried too much what the boundary conditions at infinity were. And, but I appre . . . I think, from the . . . from the earliest stage, I appreciated that . . . I think I was imbued with the notion that motions -- these large-scale motions -- were different -- had to be . . . that certain simplifying assumptions had to be made involving geostrophy or quasi-geostrophy. And, because I could see that if . . . in certain simple cases, that you would . . . if you tried to solve the equations of motion in generality, that these . . . that the equations would also permit gravity waves as well as long waves. And, ultimately, I derived the final equations by a systematic use of a set of inequalities which stated that certain . . . that quantities which were essentially either external or internal Froude number quantities were small. In other words, \( U^2 \over \rho gh \) or \( U^2 \over RT \), or \( U \times U - c \over RT \) or \( c^2 \over RT \) or phase velocity and the particle velocities would be comparable.

P: Do you feel that those procedures . . .

C: And, also, that the . . . I was aware that you could get inertial oscillations or that inertial effects, and that therefore \( f^2 \over k^2 RT \) would also be small. That I came to a little bit later, but, ultimately, the way . . . and what happened was that . . . it was simply a matter . . . simply an algebraic transformation or . . . of the equations until by systematically eliminating these quantities I got a set of . . . a derived set of equations which, to my enormous . . . great delight and amazement, turned out to be a very simple hypergeometric equation. And I can tell you, that . . . I mean, by that time I had begun to narrow down the problem. I knew it was a stability problem. I knew that you had to show that \( c \) was complex with a positive real part . . . with a positive imaginary part. And, I had lived with these equations for upwards of a year, and, uh, I remember going for a walk in the hills . . . in the Brentwood hills . . . or, in the Westwood hills, and just transforming them in my head and suddenly I saw the . . . in a sense, the solution. So I would say in late 1944 or early 1945.

P: In what sense do you mean you saw the solution?

C: I saw that . . . well . . .

P: You didn't solve the confluent hypergeometric equation in
your head . . .?

C: No, but I . . . I . . . but I said, you know, anything that would . . . the equation was so simple that it had to be right.

P: Would you not have been happier had the equation not been confluent?

C: Oh, it wouldn't make any difference to me. At that stage, I figured that any second-order equation could be handled one way or another.

P: Hm.

C: It turned out that to find solutions of the equation wasn't . . . you know, in those days, they were not all tabulated and you had, you know, there was one with a logarithmic singularity, and, um, to derive the solutions I had to adapt methods which had been used to handle that -- the confluent hypergeometric functions -- and it was only then that I had to go back and learn practical complex variables.

P: Um hm.

C: Because . . . I mentioned to you in preliminary discussions that I did have a course in complex variables, but it was on a completely abstract level; that is, we didn't evaluate very many contour integrals, if any. I remember we spent one whole semester in just deriving the theorem that the integral of a . . . of a complex function around a closed curve is zero. And I think Lebesgue proved that, you know, for a . . . for simple closed curves, it's not an easy s . . . I, I . . . In other words, and even then, I think one has to make some assumptions . . . to do it rigorously . . . about rectifiability and so forth . . . (sirens) And then the rest was on . . . on branches of the (?) . . . the monodromy theorem. Uh. So anyway, I had to study some complex variables so that I could, you know, evaluate some of the contour integrals. As it turns out, there were simpler methods that I could have used to derive these functions, but, uh . . .

P: Tell me, um . . .

C: But that, uh . . . wait, I . . . I don't know that it's . . . that's important. I think one can really . . . oh, what what was important was that C. C. Lin's first papers on the Orr-Sommerfeld problem began coming out in 1944 or 45 in the Journal of Applied Mathematics and I read them, and C. C. Lin was then at Cal Tech and I went and I discussed the problem with him.

P: Hm.
C: He didn't, uh, . . . in rather general terms. But, I think that was helpful.

P: Was that the only professional discussion you might say that you had about . . .

C: I think that was the only professional discussion. And it wasn't . . . in other words, he didn't suggest how I would go about solving my problem, but we discussed the Orr-Sommerfeld problem and he was, I think, interested then . . .

P: Hm. That's interesting.

C: . . . in learning, but I think . . . I don't know whether . . . Kuo subsequently carried the problem further and was able to derive the stability criterion analytically. Um. The way he write . . . he wrote his paper, either . . . clearly, he had been influenced by Lin's work. But I think he knew Lin personally and it may be that he actually discussed it with him. I don't know. Do you know?

P: No. I don't know for certain, uh, but I have the impression that he and Lin were classmates . . .

C: Yeah.

P: . . . in China.

C: But that doesn't necessarily mean that he would have known of Lin's work at, uh . . . I'm sure . . .

P: I'll ask him.

[See interviewer's commentary 1, page 151. (GWP)]

C: Yeah.

P: Jule, uh, as you look back at your thesis, uh . . . can you point to any mistakes that you made, perhaps later corrected by others who worked on the problem? Mistakes or oversights, call them what you will.

C: Well, I think, uh . . . let me jump a little interval in time. I wrote up my thesis, of course in forty-five and early forty-six, and uh, eventually found myself on a National Research Fellowship to Oslo. And it seems to me that the ship docked for a day or two in Bergen. It was the St . . . the Norwegian liner Stavangerfjord. And Eady was in Bergen.

P: Hm.
C: I'm not absolutely sure of this, but I could easily . . . well . . . And I spent either a day or a couple of days in Bergen and went to the Institute of Geophysics there and saw Eady and it was the first time I met him. This would be early 1947. And, uh, I had already sent away my paper for publication, but we . . . but we discussed our work, and it turned out that Eady had dealt with the same problem, uh, I feel in a much more physical and useful manner. But he had simplified the problem, uh, but in order to do so, he simplified the problem even more than I had. I had . . . In other words, I wasn't sure to what extent I could ignore . . . use what amounts to the Boussinesq approximation. I thought that the fact that the density changed by an order of magnitude would . . . would . . . but the upper boundary condition was very important, that a rigid lid was not accept . . . I thought of a rigid lid and this . . . and turned it down because it was unrealistic. As it turns out, you know, the Eady problem is much more exceptional and didn't involve beta. On the other hand, it was much more comprehensible and, I think, more physical. I think that in my own work the one part that was not dealt with adequately was the crossing substitution, which Kuo did properly. I think . . . I was able to show that there . . . that there had to be unstable roots and find the neutral stability curve but I wasn't able . . . but I didn't even attempt to calculate the unstable roots as Kuo did later. And I think, um, . . . Nor was I able, with the equation as I had it, to, um . . . I, uh, I I I was unnecessarily occupied with such things as, um, the dependence of the stability on the surface zonal velocity . . .

P: Um hm.

C: . . . because if you specify the temperature gradient that doesn't specify the zonal velocity. Well, that turned out to be a very small effect. Um. And so I spent a needless amount of time hunting for the stability . . . for the neutral stability curve, um, with variable of . . . finding the shape of the curve with . . . both with variable and zero surface zonal velocity, something like that. And also, I did solve the stratospheric . . . that is, the problem with and without the stratosphere, showing that the neutral curves didn't change very much.

P: Were all your calculations done on . . .

C: And . . . But I think the main . . . the main thing was that, uh, I didn't appreciate at that point . . . Well, for example, I didn't go into the energetics, although I knew intuitively that, you know, what the energetics were, I suppose.

P: How could you have known that?

C: Well . . .
P: That's actually rather a subtle question.

C: Well, no because, uh, I was quite clear that ... that it was the potential energy of the flow and not the kinetic energy.

P: But if you ... 

C: The nature of the geostrophic approximation is that you cannot convert ... I understood that much ... 

P: Uh hm.

C: That the kinetic energy of the flow did not contribute to the instability ... 

P: Uh hm.

C: ... of the mean flow.

P: Did you carry out all the calculations on a Marchant calculator?

C: Yeah.

P: That's impressive. Tell me ... Your paper eventually gave rise to a flood of papers on the baroclinic stability problem. Of those, whose contribution to the problem do you think is the most significant?

C: Well, I, of course, Eady's was done entirely independently.

P: Yes.

C: And he, uh, later on and ... much later. And I was very impressed by him when I first met him and later invited him and he spent a half year at the Institute for Advanced Study with me. And then I spent, uh, several months at Imperial College in 1957, and we both lectured and he was ... he was, uh ... we were ... he gave a talk in which he was at pains to point out ... no, he was very bad about publishing his results; a great pity, because I'm told by John Green, who was his student that ... that he had a lot of results that he never published. When he was at the Institute for Advanced Study he worked on ... on ... he wanted to understand the beta-effect and he introduced it as a perturbation. Never published that. Um. To my knowledge. But he was then, in fifty-seven, at pains to point out that, uh, that, although my paper appeared in forty-seven and his appeared in forty-nine, that we probably had ... that he had really started working on his, uh, during the war, in forty-five or so.
P: (laugh)

C: And I said, well, so had I. (laughter) But, there's no question that, uh, . . . And I don't know what motivated Eady. I think Eady had a much better background in dynamics . . . fluid dynamics than I did . . . when he began his work. I'm not clear about his own . . . Maybe somebody has written on that.

P: I don't know. I hadn't thought about it. Do you think there was a, uh, a, uh, lineage connected with Sutcliffe and that group of people on the synoptic side, who were interested in deepening and things of that kind. Or did that come later? I'm not clear . . .

C: I'm not clear myself.

P: All right, let me, uh, pursue this general line of . . .

C: Oh, but you asked . . . But wait a minute . . . uh, you asked me who had made the important contributions to our understanding of baroclinic instability. I think that, uh . . . uh . . . I hadn't considered this problem. I . . . I'm quite familiar with the literature. I think that Fjortoft also, when he saw the solution, he interpreted it and, in his own inimitable and completely original way. But I don't know that his work has had any subsequent influence. Um. I think I would have to jump some distance to, perhaps, um, the work that Stern and I did. You see, at . . . later I think we'll come to this, but at a certain stage . . . It may be at about, um . . . Kuo's work, which I think was very important. Nineteen fifty two, I think it was. Something like that. And that adumbrated . . . I mean Kuo made much more use of previous work on the Orr-Sommerfeld problem and on . . . the general . . . it made more use of the general theory of the Sturm-Liouville equations and, uh . . . Stern and I showed that the problem was very similar to the problem of Couette stability of two-dimensional or uniform shear flow, uh, and began to interpret the boundary condition as that of a free boundary and as of a . . . I actually, but didn't . . . I don't remember, you know, actually used in some lecture that I gave at MIT and Woods Hole, made use of the idea of, uh, surface potential vorticity, using a delta function, and in that paper with Stern generalized or made use of Lin's . . . of a . . . basically of Lin's derivation of von Kármán's interpretation of instability in terms of vortex forces. In the case . . . and could apply that to the case with temp . . . where the surface potential vorticity, that is to say, the horizontal temperature gradient vanishes at the ground. And then immediately realized that, uh, if you have, say, a uniform horizontal shear, but then suddenly bring the shear to zero at the ground, it doesn't change the problem, it simply changes the potential vorticity gradient, um . . . instead of having a delta function potential vorticity
at the ground. The one who... This appeared first, though, in print in Bretherton's paper in 1966, and I think that was an important paper, in the interpretation. And then, just very quickly, I think I would be overlooking a lot of other important work. I'm rather impressed by the work of Lindzen and his students on internal over-reflection. Basically, it is applying... And here... Here, of course, there is an enormous difference between my formulation of the stability problem and Eady's. My formulation, as I also rather soon became aware, permitted vertical propagation of... permitted the existence of internal waves, whereas Eady's approach didn't permit internal waves or vertical propagation. And his was therefore a highly special case, where, you know, any attempt at generalization immediately gives rise to the concept of vertical propagation of these waves. And then, in Lindzen's hands, led to the idea of overstability, in which he now... by means of WKB methods, can really derive crude estimates of the stability with almost arbitrary zonal velocity profiles. And I think that has deepened our understanding of the baroclinic... baroclinic instability.

P: What is your assessment of Burger's work?

C: Uh, well, I think that was important. It showed there were, uh,... Let's see, which came first, was it Burger's paper or Green's numerical result? Maybe Burger's was first, I'm not clear. I think Green in 1960 published a paper in which he found the secondary instabilities at values of the shear smaller than the critical value, when in fact, uh,... I'm not aware that these instabilities are of any major importance in the atmosphere or oceans, but I think it's an important point in, sort of, clearing up our understanding of baroclinic instability. I think that, uh, Green's results and the appreciation of the... what happens in critical layers is probably more important.

P: Do you think that your thesis is your most important publication?

C: Probably. In the sense that it has had the most influence. Yeah, I think it is, because, I think that the... because it was the first time the concept of... and this, of course, leads to the whole question of upper stability of long waves and stability of frontal waves. I mean, my present, uh... I mean, I think one could... At the time, I had not written up my thesis for publica... I didn't... I had written up... I lectured on my thesis in Chicago when I left UCLA and stopped in Chicago with the intention of staying only a week or two and ended up by staying nearly a year. Um. Rossby and Palmén were there at the time, and I lectured to them, and Rossby... This led to a number of discussions of the general problem of long waves and Rossby was at pains to point out that there was a difference between these long waves and these, uh, shorter waves.
that one also saw — Schnellläufer as he called them sometimes, secondary instabilities which form along the front.

P: Surface phenomena.

C: Which were more nearly surface phenomena . . . no, it can sometimes be seen in the upper air too. And, in my writeup for publication, I appreciated that point and pointed out that the long waves that I thought I was dealing with were different from the frontal waves, and that it was a problem to associate . . . the two. I think, since then, we've come closer to a solution to the problem with the work of Stone and Hoskins and Bretherton on frontogenesis. Well, and Eliassen. Um. In particular, Hoskins's work, who shows that an Eady wave, um, forms a frontal surface in finite amplitude, using the semi-geostrophic approximation, and that . . . and this was preceded by Phillips' numerical experiment, in which he too shows that the baroclinic waves develop sharp temperature gradients. And, that the, of course it was only a two-layer model and you couldn't deal with it very accurately, but it seemed to me then that the formation of the so-called major frontal . . . frontal wave was a concomitant of the formation of the front itself. It was not an instability on the front. But that the major frontal wave was part and parcel of the upper wave. However, once the front . . . once you got strong temperature gradients and vertical shears at the front, you can get secondary instabilities. And actually one of my students made some headway on that problem, looking at the instability of the . . . of the finite amplitude Eady wave.

P: M hm.

C: Duffy. But, yes, I think I would say, George, that my most . . . probably my most influential work was my thesis. And everything has been downhill ever since. (laughter)

P: Did you have any trouble getting it published?

C: No. Uh. I was very fortunate in having a first-rate editor, Ray Montgomery, who, while he didn't dispute the mathematics, uh, was . . . made . . . did make a lot of suggestions . . . suggested changes in the syntax, which I think vastly improved the paper.

P: Did he grumble about it's occupying a whole issue?

C: Yeah. Yeah, he did. Did it occupy a whole issue? I've forgotten now.

P: I don't know . . .

C: No, I don't think it did.
P: Pretty nearly.

C: But it was a longish . . . He said it was probably too long. And, as I . . . well, if I had to do it over again, I think I could shorten it by at least a factor of four.

P: Did you get requests for reprints?

C: Oh yeah. I ran out of reprints rather soon.

P: So it was recognized at once that . . .

C: Yeah, I think . . .

P: . . . we had something there.

C: Yeah, I mean, it certainly gave a big boost to my career and, what it did for me, personally, was to at last convince me that I could . . . that I could do research, and it solidified my love of meteorology. (?) . . . Here was a field where I thought I could accomplish something.

P: Uh hm.

C: And, uh . . . and I knew that, George, from the minute I found that confluent hypergeometric equation. (laughter)

P: Interesting.

C: Uh. I read Koestler's work on the sleepwalkers and, uh, I think that there . . . I don't agree with everything he has in it, but I think that the, and I don't identify with the great men that he was talking about, in particular Kepler, but I think that the, that that experience of . . . of . . . that the germination of ideas occurs in a very . . . that discovery is totally different from, um, the context of discovery is very different from the context of explanation or explication.

P: Uh hm.

C: But you don't discover things the way you write them down. That there's . . . there are subterranean processes, uh, which occur, and only at a certain point do you realize . . . suddenly do things begin to gel and you know where you're going.

P: Do you think this problem has been largely solved?

C: Oh, I think that the . . . that the general problem of . . . if you couple these, uh . . . by no means . . . I think that, if you talk about the stability of zonal flow, purely and simply, of the simple zonal flow, I think it has been largely solved by now,
although . . . but, if you take the finite amplitude problem, or the relationship of the upper wave and the frontal wave, these are . . . if you carry them to the realm where they can be observed so you must necessarily deal with the finite amplitude problem, I think that uh . . . they're far from having been solved. And it has always been a matter of puzzlement to me why the linear solutions do bear a resemblance to the observed motions.

P: Hm.

C: Especially if you filt . . . if you used some of the modern filtering techniques, let's say time filtering, so that you can manage to look at only motions with a certain frequency range.

P: Looking back . . .

C: But I don't think that anything I did in numerical weather prediction was comparable in originality (?)

P: Looking back now at the baroclinic stability problem in its historical context, that is to say, it is a big and crucial step in the evolution of ideas about the stability of systems, circulation systems in the atmosphere in general, can you link that problem and your treatment of it with earlier work? Is there any sense in which there are progenitors of what you did, or is this more of a discontinuous development?

C: Well, I've already named J. Bjerknes as a progenitor.

P: Uh hm.

C: I think his genius was that . . . to recognize the importance of the upper wave basically, and to simplify the mode . . . simplify the model in such a way that it became, uh, . . . he suggested very strong . . . I mean, his work suggested to me very strongly what the appropriate model should be. That . . . his and Rossby's, I should say, I think, uh . . . I think the earlier work of Solberg's and, as I later discovered, Kotchin's and a . . . and which was based, as V. Bjerknes writes in his sort of biographical discussion of the generation of some of the ideas of Physikalische Hydrodynamik going back to Helmholtz's ideas . . . of Kelvin's ideas of stability of shear flow. I think uh . . . there was an early recognition that the problem . . . I think that the . . . that the work on the Orr-Sommerfeld problem, particularly the work of C. C. Lin, uh . . . and the earlier work played a role, yes. In other words, if that hadn't existed, how would one have known what branch of the logarithm to use . . . or something like that. Um. In many ways, I suppose, the baroclinic problem is simpler than the problem . . . the Orr-Sommerfeld problem. But it (?)
P: Any further comments about this general area which we're . . . I think we're reaching the end of this phase, unless you have a lot more ideas about, um, what occurred up to 1946.

C: Well, there's another thing that came out of my thesis, that . . . that played, I think, an important role . . . maybe an equally important role to the baroclinic stability problem itself. And that was my realization that I could have derived the equations directly if I had simply made use of the geostrophic approximation.

P: Yes, I'm coming to that.

C: But I think maybe that would be the subject for our next session.

P: I think so.

C: Because that's what I worked on in Norway. I mean, I already had the idea and the idea of a filtering approximation at an early stage, but to . . . its proper generalization to arbitrary large-scale motions only came later.

P: What you're saying is that there is a natural bridge between your thesis and what came immediately after.

C: Yeah, in other words, I think, in Eady's case, he also made use of the geostrophic approximation. Um. And, in my work, I was highly conscious of it, and it seemed to me . . . and, I remember, at a point, sitting out in the sort of front garden at the Studenterhjemmet Hotel in Oslo working on the generalization of the geostrophic approximation and then suddenly discovering that it . . . what it was is that you deduce the . . . that you can't use it to evaluate divergence . . . you eliminate divergence, you get a vorticity equation, then use it, and that's it and . . . and, uh, I felt a little bit like the discovery liber (?) of the confluent hypergeometric equation.

P: Could you . . . could you date that moment?

C: I could date it almost . . . I probably could date it to the day, if I went back in history, but it was . . . it was in the late . . . it was in the early . . . it was in the summer of 1948, or . . .


C: Summer of forty-seven, that's right.

P: Yes. (Jule laughs) Jule, I think we'd better stop or we'll run out of tape.
C: Okay.

P: Okay?

END OF TAPE 2, SIDE 2
TAPE 3, SIDE 1

P: Today we will continue the interview with Jule G. Charney. This third session of the interview is being conducted on Wednesday, the 27th of August, 1980, in Boston, at the Holiday Inn.

C: All right. Go ahead.

P: Uh.

C: My mind is a perfect blank.

P: Well, okay. Then that'll give me scope for all of my . . .

C: I mean, we . . . we . . . we should have introduced this by saying it's totally unrehearsed. (laughs)

P: Well, I think . . .

C: And unpremeditated.

P: But to anyone who listens to it that'll be apparent. Uh. Nineteen forty six to forty eight, Chicago and Oslo. First question I'd like to ask is what led you to apply for the fellowship from the National Research Council?

C: Ah. I'd heard of it, and there had been two or three National Research fellows at UCLA. One of them was my, uh, instructor in . . . one was my instructor in . . . Richard Bolt . . . In physics lab. Uh. And he later went back to MIT and then left to find . . . found a company, uh, Bolt, Beranek and Newman.

P: Uh hm.

C: He came to study with Knudsen . . . to work with Knudsen. He was in acoustics. Another was . . . I've forgotten the name. Dick Wick Hall, a mathema . . . a topologist, whom I got to know. A third, I think . . . I'm not sure, was an assistant or . . . no, was an instructor in chemistry lab, in freshman chemistry, and who, uh . . . who, uh . . . It was from him that I first learned about quantum mechanics.

P: Oh.

C: That is, I didn't learn it, but I fi . . . heard about it.

P: Yeah.

C: Um. Anyway, so I knew of the existence of the National
Research Fellowship and it was, um. Originally, what happened was that, when I was about to graduate, Bjerknes and Holmboe thought that the natural thing for me to do was to, uh, go and study with Solberg in Norway, since he was the outstanding Norwegian theoretician. And this...

P: Was this after you had been awarded the fellowship?
C: No no no.

P: Oh.

C: And, there was. And they. They felt quite sure that they could get that their recommendations would get me in. a Scandinavian-American fellowship.

P: Uh huh.

C: And, uh. So I applied for that, but then I thought, well, what the heck, and, uh, I might not never have applied for the National Research Council fellowship if I didn't really feel pretty good about my thesis. And I thought well, you know, who knows, maybe I would get it, although I didn't really think I would. And then there was also a third fellowship that I'd heard about. Well, you know, you read about these things. And it was a Jewett fellowship.

P: Hm.

C: Given by the Bell Telephone Labs.

P: Hm.

C: I think Jewett was one of the early, um, directors of the Bell Telephone Lab, or at any rate he was high up in the company, and so I thought what the heck, I would apply for that one too. Um. So I applied for all three. And, uh, the first thing I heard was that I didn't get. Oh, I. They called me up, from Bell Telephone, and, uh, said that they I would probably get the fellowship...

P: Oh.

C: I wanted, and, uh, I stalled them off because it turned out I wasn't aware at the time, I think, that while the fellowship paid substantially more than either of the other two, uh, you had to take it in the United States.

P: I see.

C: And since my intention was to go to Norway...
P: What . . . what were the stipends for these three . . .?

C: Well, to my best recollection, I think the National Research Fellowship was something like two thousand or three thousand dollars . . .

P: (laughs)

C: And with travel allowance for myself. The Scandinavian-American was about a thousand or twelve hundred dollars with, I think, also a travel allowance. And the Jewett fellowship was maybe about four thousand dollars. I think it was more than . . .

P: Mm hm.

C: Or maybe even five. It was a much more lucrative fellowship. (laughs)

P: Mm hm.

C: But that wasn't the primary consideration. And then what happened was, that I think in quick succession I was notified that I'd been awarded the National Research Council fellowship. And been turned down for the Scandinavian-American. (laughter) But, which was all right with me because the National paid more, had more prestige, and did permit foreign travel. So I was all set to go to Norway. I didn't . . . It never occurred to me to do anything else, although at that time I felt that I . . . that my knowledge of hydrodynamics was woefully deficient and that it would make . . . it would have made good sense for me to go to Cambridge.

P: Hm. Well, did the application for the NRC fellowship ask you to specify your places of study?

C: Yeah. I told them that I wanted to go to Norway.

P: Did you mention any other places you wanted to go to?

C: I . . . At this point I don't recall. I don't think so.

P: Mm hm.

C: Um.

P: Was it a two-year . . .?

C: And then I . . . I trusted . . . Oh, no, one year, renewable.

P: And it was renewed?
C: No, it was not.

P: No?

C: For reasons that will become apparent. Um . . . possibly could have been. But under the circumstances there would have been no point.

P: Uh huh.

C: Um.

P: So when did you start out?

C: So. Well, uh, I applied for the fellowships in the . . . it probably was . . . I think the deadline must have been around the end of, uh, forty-five, beginning of forty-six, something like that. And that I was scheduled to start in on the fellowship in the fall of forty-six. And, uh, well, then, uh . . . Let me mention . . . I don't know at what point I ought to mention this, but, um, possibly in connection with Princeton, but just let me say that in 1945, uh, Phil Thompson had been assigned as a weather officer liaison with the department . . . the training course at UCLA. And we became good friends. And he, uh, he showed me . . . At one point he showed me a proposal, which I called "the modest proposal," of Vladimir Zworykin to the Air Force for analogue simulation of meteorological . . . analogue forecasting . . . meteorological forecasting, in which, I think, maybe . . . Let me just say, so I was aware of activity in Princeton already by 1945. But I . . . I . . . At the time I didn't take the proposal seriously, because he actually expected to reproduce the weather in a cathode ray tube.

P: I wonder whether Zworykin had already been in touch with von Neumann at that time?

C: Well, I think probably he had, but there was no mention, in my recollection, of von Neumann. I think we should try to get a hold of that report.

P: Yeah.

C: At any rate, uh, to come back, uh, I left, um, Los Angeles, I think it must have been in June or July of 1946. Um. Elinor wanted to stop . . . My wife, Elinor, wanted to stop by and visit her parents and her family in St. Paul, and I thought I would take the opportunity to visit Rossby in Chicago. And I . . . We must have written or telephoned . . . (?)

P: Had you previously met Rossby?
C: I think actually I had. I think he came to UCLA at some stage for a very brief visit and gave a lecture, and I think at that point I was introduced to him, but we...we had no conversation...no scientific conversation, to my recollection. Ah. Whether Rossby knew anything at all about me at that point or not I don't really know. He may have heard, uh...I think I...of course I told Bjerknes and Holmboe that I intended to stop and see... (?)

P: Did you send copies of your thesis to anyone before it was published?

C: No. It never occurred to me. Not to my recollection anyway. Well, so I left in June and, um, spent some time in St. Paul, but it was arranged that I would spend a couple of weeks in Chicago.

P: Uh hm.

C: Since we had time before we had to be in Norway. I think, we...we had (?)...I've forgotten exactly when the sailing was on the boat. Uh. Well, I came to Chicago and Rossby was, you know, extremely welcoming and very kind. And Palmén was there at the time.

P: Yeah.

C: They asked me to give a seminar on my thesis, which I did. And, in those days things were extremely informal and...

P: Informal.

C: Informal, and, you know, there... There may have been not more than half a dozen or a dozen people present.

P: Who do you remember besides...

C: I don't remember anybody besides Rossby and...and

P: Starr was there, wasn't he?

C: Well, Starr may very well have been there. There wasn't any extensive comment. But at that point, you know, my thesis in a way, was mathematical, and, uh, as so many...as I advise my own students not to do, that is, go into detail in the mathematics in giving a seminar or a lecture on their work...Um, I didn't have myself to advise me at that point...

P: (laughs)

C: ...so I went in...You know, I...I thought that the derivation of the equations was really the important thing. And,
you know, they sat patiently through the thing and . . .

P: Did you give just one lecture on it?

C: Yeah, but I think the lecture must have been two or three hours.

P: I see.

C: And they sat patiently through the thing. I mean, in those days, I mean, there were no (?) class schedules as (?). And, uh . . .

P: Do you recall Rossby's reaction?

C: Well, I think his, I . . . His reaction was mildly favorable. I think it was favorable. But . . . And I think he was too polite to . . . But later on, uh, he urged me, in my writeup, to have an introduction in which I say in words what I'd done. Uh, so obviously, he . . . At any rate, Rossby knew of my work right from the start.

P: He knew of it before you . . .?

C: Oh, no no, I mean, from the moment I got there, because I gave the seminar.

P: Oh oh, yes.

C: No no, I had no idea whether anybody had told him . . . It would have been Bjerknes or Holmboe who would have told him about it.

P: Um hm.

C: I don't remember the details, but I, you know, I . . . I think that they were certainly apprised of my going there, but whether they had . . . It was certainly . . . it . . . But I . . . But I'm quite sure that it was not their suggestion that I should stop in Chicago. Nor was it their suggestion that I shouldn't stop in Chicago.

P: Yeah. I understand. I understand.

C: So, um . . . But after that, uh, I, um, Rossby was very friendly and we had some very good scientific discussions on my thesis and related matters. I mean, you know, he wa . . . um, he emphasized very much the distinction between these long waves and the shorter waves that one sees on the surface weather map. And I . . . I was aware of that myself, and, uh, but . . . hadn't had it in my thesis, but I did put it into the intro-
duction to my pap... my published paper. You know, it...
Also, I think it was in specializing, uh, the, uh, analysis to
the case of zero temperature gradient and zero vertical shear you
could solve the equations easily, and it turned out that there
were a class of solutions with internal nodal surfaces. And
Rossby emphasized to me, you know, well, I... that this
sort of the importance of this, that these were internal
wave... internal long waves, and, later on, when I got back,
after having been in Norway and coming to Princeton, I began
looking at the question of, uh, vertical... it occurred to
... Oh, what had happened was that, while I was in France on my
way back from Norway to the States, I made the acquaintance of a
young astrophysicist named Evry Schatzman, through an
astrophysicist whom I knew at the Astrophysical Institute in Oslo
which was where I was stationed, and, uh, when I came... He
came to the Institute... Oh, he came to work with... with,
uh, Schwarzschild... Martin Schwarzschild on solar flare and
solar corona and, uh, we became really quite good friends,
Schatzman and I, and I knew Schwarzschild, and I knew what they
were doing, and they were working on the propagation of acoustic-
gravity waves from the convective layer of the sun into the outer
atmosphere of the sun. And that analogy somehow stuck in my
head, and, uh, it occurred to me that... that, uh... that
these internal waves that I had found must mean that you can have
vertical propagation of long waves.

P: Incidentally, that reminds me to ask you. Was Queney in
Chicago still at the time you were there?
C: Yes, I think he wa... Yes, I believe he was.
P: And, had his paper...
C: And actually Queney, then... And then Queney went to
Princeton and I think that when I first came to Princeton Queney
was there.
P: Well now, his... his work came out, I think at about that
... shortly before that time...
C: Oh yeah, he had this long paper on...
P: Yeah.
C: ... internal gravity waves in the... in the... And I
... Yes, indeed, that's right. Um. But in... When I became
occupied with the subject of numerical prediction... um, one
of the... there, uh... I became... one of the problems
that suggested itself was the domain of influence for, let's say,
a twenty-four-hour forecast. And I decided, you know, that...
that this was really determined by long-wave group velocities
and, uh, we already knew what the horizontal group velocity of a Rossby wave . . . So I set about finding what the group velocity . . . the vertical group velocity would be on the (?) . . . to know what a . . . how far into the atmosphere one had to have data before one could make a prediction for the ground, or for five hundred millibars. And, uh, so I . . . This, to my knowledge, probably was the first time anybody had looked at vertical propagation of Rossby waves. I found . . . But, of course, I did it for a resting atmosphere. And found . . . But I figured that the, you know, that the . . . So I didn't get into any of the refraction phenomena that later developed. But, uh . . .

P: Is this what you have in your paper in 1949 in the . . .

C: In 1949.

P: . . . in the Journal of Meteorology?

C: "On the physical basis for numerical prediction . . ."

P: Well now, I take it that (?) . . .

C: At any rate, uh . . . the reason I, uh . . . I'm getting ahead of myself, but, uh, my real work on that field was done in around 1960 with Drazin. But, uh, I just wanted to say that Rossby emphasized for me the importance of these things. And that's interesting because Rossby was thinking two-dimensionally in those days. Uh. But, uh . . . But this simply proves that he was . . . And, uh . . . Because I remember going . . . They had fascinating map discussions, with Cressman very often preparing the forecast, and . . . and presenting it to the group. And then Palmén and Rossby making comments. One time, in part . . . I must tell you this story. Oh . . . Victor Starr, who was a retiring sort of a fellow, working on his own things, and Rossby, you know . . . and refusing to be, uh, very much under Rossby's influence, but nevertheless maintaining his own independence . . . And he would often go up to the attic to work, because this was where . . . that he would be . . . he would be, uh, he thought, safe from Rossby . . . (laughter) and when the map discussion time came, Rossby would look around and say, "What! No Victor Starr?" and he'd send somebody in search of Victor, and he said, "Try the attic." And then Victor . . . then sometimes he would come down dragging Victor behind him. (laughter)

P: Oh dear, well . . .

C: Uh, but, uh . . . Once in particular, we were talking about the development of the trough and, in Rossby's company, and Palmén's too, it was very easy to participate in the discussion.
I mean, discussions were always amusing and exciting, and, uh ... Rossby had, like Bjerknes, had his own sense of dignity, but he didn't have any side. And so that, you know, you could argue with Rossby, you could challenge him, um, whereas I would never dream of doing that with Bjerknes. Altogether I, you know, I found Rossby a very congenial person and we became good friends. And when Elinor later came down, we became sort of good family friends. And I remained really close friends with Rossby the rest of my ... the rest of his life. Ah. But on this ... On this occasion, at the map discussion in connection with the deepening trough, um, I used some kind of a simile about the deepening pressure dragging the streamlines with it, or something like that, um, in other words, where the isobaric field was sort of the dominant field and the ... the stream field was secondary. I mean, the implication was that. Rossby became furious, and really almost apoplectic, that, you know, that that was not science, that this was just isobaric geometry and he thought that by now we ought of ... we should have gotten away from this kind of thing. Well, it seems to me that ... that he was venting his an ... his pent-up anger at the ... in a way, at the non-productive isobaric geometry that had ... meteorologists had used, but it may have been a left-handed swipe at Bjerknes. I'm not sure. Possibly was. I mean, I think, to him dynamical quantities, conservative quantities like vorticity, were predominant, and I think he was absolutely right. And when later he was actually the first to discover the theorem in somewhat restricted ... a restricted form of the theory of ... of potential vorticity ... again, uh, in his ... 
P: Mn, that was not there.
C: Uh.

P: That was in the late thirties.
C: (?) Was that in the late thirties? No.

P: I believe so. I believe ... 

C: No. I thought that was published in a supplement to the journal ... 

P: Well ... 

C: ... of the Q. J.

P: Right. That was in 1940, that supplement article.

C: In, uh, the supplement ... Was it in 1940?

P: Yeah. But then there had been an earlier version of it for a
for a stratified incompressible ocean in the Journal of Marine Research in 19. I think it was 1938 or thirty-seven.

C: Really?

P: Yeah.

C: I'm unaware of that. I'm only aware of the...

P: Let's look into that.

[See interviewer's commentary 2, page 151. (GWP)]

C: Yeah.

P: It might be interesting... I'm not... totally certain

C: Okay (?)... the record will show that. But any rate, I know it was before Ertel's paper.

P: Well... uh...

C: And I've always sort of objected to calling it Ertel's theorem because it... he had the essential theorem, it wasn't quite as general as Ertel's... before it.

P: I've often wanted to question that terminology too, uh, and it's come into vogue, but I've never troubled to delve into the historical matter except to carry around the notion that it wasn't quite accurate to use...

C: Yeah, well, I think, actually, of course, they had done it independently and both deserve credit and some people call it the Ertel-Rossby or the Rossby-Ertel theorem. And it seems to me that at some stage they wrote some kind of... Did they write a joint paper? They wrote some kind of a joint paper.

P: I believe they did, but I think it was on a different subject.

[Wrong: it was on the same subject. See interviewer's commentary 2, page 151. (GWP)]

C: Oh, was it? Okay. At any rate, uh, this was only one incident. There were... in which Rossby could express himself passionately. Rossby was a passionate man. But at this point he was more than passionate. He was almost, as I say, apoplectic. But, of course, it was not personal. And I never took it to be. And it didn't, uh, interfere at all with... On the contrary, it simply made him more human for me. It didn't interfere with the development of our friendship.
P: Did the . . .

C: And, well, then what happened was this: I got a letter from Holmboe saying that, uh, that Solberg was coming to the United States and would not be there when I arrived in the fall, that he . . . that, at any rate, that, um . . . and that he would be at the University of Chicago shortly. At that point, Rossby suggest . . . Rossby suggested that why didn't I stay as a Research Associate at the University of Chicago for the year. I've forgotten whether Chicago was on the quarter system then or not.

P: Oh yes.

C: It was on the quarter system? Well, then, at least until the following spring. And, as an inducement, he . . . he had told me about a meeting which was to take place at the end of August in Princeton to discuss the, uh, . . . that . . . called by von Neumann and . . . and Rossby was one of the organizers. And that Rossby would obtain an invitation for me to attend. And my interest having been already perked by this Zworykin's "Modest Proposal," um, it was . . . and . . . and my . . . and having been enormously attracted intellectually as well as personally by Rossby, it was an easy matter for me to accept his offer, thinking that I could . . . But, of course, I got in touch with the National Research Council and they said yes there would be no problem in postponing my fellowship.

P: I see.

C: Until the following spring, I think.

P: Um hm.

C: Um, later Solberg did come to Chicago while I was there, and it turned out that he was only coming for a short visit and that he would be in Oslo in the fall. But by this time I had already . . . Oh, but let me say . . . Wait a minute. When I first got the letter from . . . from Holmboe, Rossby had not made this offer, and I decided that . . . At this point, it sounded as if Solberg would be spending a long time in the States, and, um . . . And since I had the feeling that, uh, I would profit very much by studying with G. I. Taylor in Cambridge, I sent . . . I . . . I think I must have sent him a telegram, or, at any rate, a letter, asking if I could come and work with him.

P: Um.

C: And I received a reply, I think telegraphic, or maybe letter, saying that, yes, it would be fine, he would be glad to accept me, but that he had ceased working in meteorology sometime since, and, uh, that it might make more sense for me to work with
Jeffreys. Uh. I probably, . . . I . . . I'm not . . . My recollection isn't clear on the sequence of events, but, um, I'm not sure whether by then Solberg had come and said that he would be back in Oslo, and then Rossby made the offer, or vice versa. But, in any case, it seemed to me, um, nothing but, uh, beneficial to myself for me to spend this time with Rossby, and then go study at, uh . . . When I found out that Solberg would be there, I . . . I . . .

P: Would be where?
C: Would be back in Oslo.
P: Uh huh.
C: In the following spring, or, uh . . . I decided to continue to, uh, go on with my original plan after the Chicago . . .
P: Did you lecture while you were in Chicago?
C: Yes. Uh. Beginning that fall I lectured . . . I lectured in the hydrodynamics course, and remember the chief reference that I used was Prandtl.
P: Hm.
C: And I think I learned . . . I don't know if the . . . I can't vouch for the students, but I personally learned a lot of hydrodynamics. (laugh) I mean, this is the first time that I sort of speci . . . looked at hydrodynamics seriously. That was very kind of Rossby to allow me to do that. I think I told him that it would be very good for me if I could do that. So I lectured in hydrodynamics in the fall of 1946.
P: Do you think the whole Chicago experience was an important one?
C: Oh, I think it was a . . . I might even say the main formative experience of my whole professional life.
P: Is it possible to say in any . . . in any very explicit way what you gain . . . got from Rossby, either specifically or generally speaking, scientifically?
C: Well, I mentioned to you that I could, . . . that I had very little intellectual rapport with Holmboe, because he thought in geometric terms. Uh. Ro . . . I found, you know, that I . . . that Rossby and I were, in a way, kindred spirits. I mean, we thought very much the same way, and we had endless conversations about geostrophy, the origin of geostrophy, and . . . and the role of long waves, short waves, whatever (?) There was no
subject that we couldn't discuss and it was always intensely interesting to discuss things with him. And he always made himself available. And, it was not only that period in Chicago, because he had other duties, and, um . . . But, you know, we saw a great deal of each other socially. Uh. Erwin Biel came, and we would go on some of his conducted tours to look at . . . (laughter) Well, I went on some . . . But in the evenings, we would, you know, . . . we would go to the Hungarian section or whatever, and go to the . . . these Hungarian restaurants and dance the czardas and, uh, it was just enormous fun, and then Alf Nyberg came and . . . You know the Nybergs? . . .

P: Um hm.

C: . . . and he came along. It was, I think, one of the most delightful, the most exciting periods of my whole life. Uh. I mean, here, I . . . I discovered what it meant to have intellectual rapport with a man . . . with another person (?).

P: What do you think were Rossby's strengths and his weaknesses in science?

C: Well, he . . . he, too, was an in . . . intuitionist, and I think he was . . . I am. I think my intellectual bent . . . (?) He was analytical in his approach. Um. I already mentioned to you that his analytical approach was . . . No, my . . . my first encounter . . . I mean, I had read a little bit of, uh, Physikalische where too they used something of an analytical approach, but (?) But, well . . . But, you see, I always felt that, uh, in particular Solberg . . . I think this feeling was reinforced later on, but right from the beginning, that his most important work was done in conjunction with, uh, V. Bjerknes, and that V. Bjerknes was the physicist and Solberg the mathematician. Uh. And that Solberg's, uh, work was pret . . . rather formalistic. My own work was very definitely . . . was inspired by observations and, you know, that . . . that it didn't arise in abstraction. And, what . . . what I really felt, uh . . . Rossby, uh, had enough . . . had . . . had a . . . I think, fairly strong mathematical training in relevant mathematics. I once, when I visited his home in, uh, in O . . . In Stockholm, after he had gone there . . . I found a very well-thumbed copy of Riemann-Weber, and I think he had actually studied with Fredholm. And, you know, you could see that he could handle, uh, partial differential equations with a certain amount of ease, and, uh . . . but, that he used . . . but, that . . . that he used his analysis to explain observa . . . Right from the beginning, I think from when he first came to MIT in 1928 and worked . . . and worked . . . was a consultant to the Weather Bureau, and set up the long-range . . . uh, the . . . I forget what they call it . . . and this with . . .
P: Extended forecasting?

C: . . . with what became later the extended forecasting project at the Weather Bureau.

P: Five-day . . .

C: He was . . . he was . . . Five-day forecasting, but he was very very, uh, conscious . . . I mean, the . . . the fact that he participated so actively . . . in fact was the dominant personality in these map discussions . . . I mean, he was sort of the compleat meteorologist. I think he was both synoptic and dynamic . . . And that has always sort of remained my own approach. I think, uh . . . I don't know that I have the physical intuition that Rossby had. I think . . . I didn't have it to begin with, I know, but I think I've acquired some . . . somewhat, um . . . But I admired very much, and I could see the relevance of his approach. And I think that that was, uh . . . I think that, perhaps we're dwelling on this point too long . . .

P: No. Not at all.

C: If you were to ask me . . . The dominant characteristic of Rossby was (a) his in . . . his imagination, his intuition; and his . . . his, uh . . . He was a thoroughgoing physicist. That is, he didn't, um, allow his imagination to go too far beyond, uh, reality.

P: Could you put your finger on any . . . on any weaknesses?

C: (?) I think the weaknesses . . . his weaknesses were that he wasn't . . . I may u . . . This may be more of an identification thing, but, I would say that his weaknesses were that he wasn't rigorous. I mean, he would jump to conclusions. Ah. It remained for other people to come along afterwards and clean up some of his work. Occasionally, he could be wrong. Um. I think his weaknesses were those of any rather intuitive mind.

P: Uh hm.

C: I mean, he trusted his intuition a little bit too far, but, uh, to me that's . . . If your intuition is as . . . is as good as his, I kind of think that's a quality rather than a defect.

P: Uh hm. Well, I'd like to return to Rossby here and there as we go along, but could we get on now to what I think is a very important matter that falls in this . . . in this period, namely, the evolution of the filtered equations. And I have a couple of questions I'd like to ask as leading questions to induce you to talk about that.
C: Well, okay (?)

P: Can we get into that now?

C: Ah, all right. But, I think, for that, why don't we, uh, . . .

P: Let me pose this question.

C: Should we . . . should we jump to Norway? I mean, if we . . .

P: No, because I'm (?) . . .

C: Separate (?) geographic locale.

P: Well, I'm not quite ready for that. But this leads into Norway very quickly. Um. There's a . . . There are two really quite interesting letters dating from this general period that were exchanged between you and Phil Thompson.

C: That's right. You see, that was when I was in Norway.

P: No. Well, one . . .

C: Oh no no, wait a minute. No, one, no . . . no, no . . . (?)

P: One was in Chicago.

C: One was in Chicago.

P: All right, now, um, to begin with, let me say that Phil, at that time, in 1947, had already been assigned to Princeton. I think that was in the, uh, autumn of 1946, that he was assigned to the Princeton . . . I don't know what they called it at that time, but anyway, under von Neumann's . . .

C: Princeton project or . . . no no, the, well, uh . . . anyway, yeah.

P: And, um, then he wrote to you from Princeton in, um, on the 3rd of February, 1947, and here's what he said, after some preliminaries, and I'm quoting now: "Why don't perturbations, like say, the traveling cyclones, move at velocities comparable to that of sound, meaning, what new and essentially different physical mechanism limits how fast these disturbances are propagated?" Unquote. That's all I'll quote from his letter, but let me just, uh, add to that that in his . . . in the preliminary remarks in his letter, he talked a little bit about the problem of computing and the time and space increments, uh, having been made aware of those difficulties, undoubtedly, through . . . in
discussion with von Neumann.

C: Well, in particular, the Courant-Friedrichs . . .

P: Yes.

C: . . . Lewy condition.

P: Now, uh, you replied to that from Chicago. Your letter is dated February 12th, 1947, and it was a long letter, five pages, but, uh, skipping a great deal of it, uh, you first talk about fast and slow wave propagation in a barotropic atmosphere. And then, this is the part I want to quote now, and here I'm quoting, in your reply to Phil: "If you accept ("you" being Phil). . . . If you accept the consequences of the above reasoning, you will perhaps share my conviction that there is a general type of approximation or transformation or what-have-you that will eliminate the noise, and the problem is how to find it." Unquote from your letter to Phil. Now, here's what I'm getting at. Uh. That letter, your letter to him, uh, informs us about two things. First of all, in February 1947, at the University of Chicago, you certainly had a clear conception that transformation of the equations was needed in order to get weather prediction off the ground. That's near the end of this tape, but let's . . . let me just finish posing this question. Um, and second, at that time you had not yet found that transformation, in February 1947.

[Charney's letter of February 12, 1947 to Philip Thompson is reproduced in Appendix B, page 160. (GWP)]

C: In, uh, in . . . in, uh, I'd not found the general transformation that could be applied to numerical weather prediction.

P: Yah. So, the question is, uh . . . is, uh, . . . I'd like to pose it as a . . . as a two-part question. The first part is, when did you find that transformation? And secondly, to what extent, if any, did Rossby have any influence on . . . on what you did? Now, unfortunately, or fortunately perhaps, I'll give you time to think because I have to turn the tape over.

C: Okay.

P: Okay?

C: (?) munch a sandwich.

END OF TAPE 3, SIDE 1
P: Okay. Now, let's see. Hm. I had just posed a question to you about this letter that you wrote to Phil Thompson.

C: Yeah.

P: And the question had two parts: When did you find the, uh, filtering equations...filtered equations? And: Did Rossby play any role whatever in that process?

C: I think it's very difficult for me to say whether Rossby played any, uh, direct role. Uh, certainly he didn't suggest them to me. Because, for one thing, I already knew that for long waves of infinite lateral extent... I knew exactly, because in my thesis, I have... I derive the cubic equation and point out that um, for the cubic, um, dispersion relationship... and point out that two of the roots are gravity inertial oscillations, and one of the roots is the geostrophic...

P: Of course, that was already in Rossby's paper of 1939.

C: Yes, and which was wrong.

P: What?!

C: Oh that paper's incorrect. He made some... He made an unjustifiable assumption and his dispersion relationship is wrong.

P: I think... Wait a minute now... I think there's a misprint as to the place where... where the zonal current goes into that formula. Is that what you were talking about?

C: No, I think it's more... it's... The error was worse than that. I think, uh... in other words, it was not a misprint. I think... I won't swear to this. I would have to, uh...

P: Well, let's talk about that later.

C:... go back and look at it and refresh my memory, but my recollection is that, uh, in his derivation he made an unjustifiable assum... ad hoc assumption which he needn't have done and, uh, so that he gets a dispersion... a cubic dispersion relationship that's not correct.

P: Fundamentally wrong?

C: Fundamentally wrong. I mean, if you were to use it you'd get the wrong answer.
P: Well, let's talk about that.

C: I mean, you wouldn't get the . . . again, if you made . . . if you assume that the . . . the zonal phase speeds and the zonal velocity were small compared to the, uh, velocity of the gravity wave, you would get the right . . . you would get the Rossby wave formula, but you . . . the gravity wave formulas would be wrong.

P: But I believe he did point out that of the three roots, two correspond to . . .

C: Oh yes, I . . . I'm quite sure that he knew it.

P: That's (?) . . .

C: But, you see . . . But I went further than that, I think one could say. Mainly that I derived this cubic equation, pointing out that one root was the Rossby wave and the other two roots were gravity inertial oscillations. And then pointed out as well that, by making the north-south velocity component geostrophic, these were waves of infinite lateral extent. You could not make the east-west component geostrophic. Um. Because that was the divergent component. Um . . . that you filtered out the gravity waves. So I already knew how to filter out gravity waves. After all, that was the . . . that was one of the main, uh, results of my thesis. The problem at that . . . at the point when Phil Thompson wrote to me was to find a method which was generally applicable to finite amplitude motions, of finite lateral extent and that . . . But I discussed these . . . you know, the difference between gravity waves . . . and Rossby agreed with . . . you know, we . . . we were, Rossby and I were in thorough agreement that one needed to, uh, confine, you know, wanted to have a method to deal with the long waves and . . . and that the gravity waves were . . . were, uh . . . were unimportant noise, in some sense. I think, uh . . . So, I don't . . . I can't remember any leading idea that came from Rossby, but I . . . but I think the development of my general feeling about the atmosphere in these frequent discussions with him must have played a role in my thinking. I can't go further than that. It would be a pity . . . It's a pity that Rossby isn't here . . .

[See interviewer's commentary 3, page 151. (GWP)]

P: (laughs)

C: . . . so we could ask him. His memory might have been better. Well, anyway, the actual derivation of a form of quasi-geostrophic equations, um, I made in, um, Norway in, uh, the spring of . . . the late spring of 1947, or the summer of 1947, after I had already come to Norway, and began . . . and had the time and, uh, the tranquility to start really thinking about the
problem, uh, inspired at that time already by the thought that we
needed such a method, uh, both for theoretical reasons, because
that would give, as it had in my . . . the case of my thesis, a
vast simplification of the problem, but also for numerical
weather prediction. At that time, I . . . I had a dim view of
using the primitive equations. Somehow I felt that that, um
. . . but I very soon corrected . . . By the time I got to
Princeton I was already semi-aware . . . and . . . within a year
or so, I became completely aware that the . . . that . . . that
if you were willing to satisfy the Courant-Lewy-Friedrichs
condition, ah, that the error you would make in the initial
tendency field, because you couldn't measure the velocity in the
fields . . . of the pressure fields with sufficient accuracy
. . . that that error would just give a small embroidery on the,
uh, large . . . it would give you an embroidered tendency field,
which would be essentially correct. In other words, the
primitive equations would be quite possible. I published that
notion in 1951 in a Compendium, but I think I was aware of it
almost from the start.

P: But doesn't that depend upon how you treat the initial data?

C: No. Well, I mean, the whole question of initialization and
initial error seemed to me that . . . that was not a . . . a, uh,
fundamental issue, that, while it was true you would get the
tendency field wrong to start with, simply by . . . you would
probably be exaggerating the gravity waves, the gravity, if you
had the motion . . . the basic pressure velocity fields, or if
you had simply assumed that the velocity field was geostrophic,
ah, you would get the . . . and then use the primitive equations.
In fact, the objective analysis scheme that I first proposed
involved assuming that the velocity field was geostrophic.

P: But then you . . . then you're saying that, I think, that you
must, in some sense, balance the initial data.

C: Okay. But you would balance and . . . and this would give
rise to spurious . . .

P: Yes.

C: . . . oscillations, but I could . . . but I could show for a
simple case that the energy of these oscillations would remain
bounded (?) vicinity (?) by their initial value and . . . and so
this couldn't . . . But, of course, you were then required either
to satisfy the Courant-Friedrichs-Lewy conditions or else to use
implicit methods.

P: Do you think your initial wariness about the primitive equa-
tions stemmed in any way from a knowledge of Richardson's work or
were you not familiar with that work at this stage?
C: I was . . . Yes, I had . . . I had sort of skimmed through Richardson's book and, uh, I think erroneously, as you have pointed out to me, thought that his fundamental error was that he, uh, that his initial tendency field was completely wrong because he used the . . . he used the . . . because he was not able to evaluate the divergence. That he used the . . . that he couldn't have used anything better than the geostrophic wind so they would have given the false divergence. Yeah, I was . . . I was very aware of that; but, I mean, so aware, that I thought that maybe the whole method . . . that the primitive equations were just not appropriate. But I think . . . As I say, I think I became aware, despite the fact that we used barotropic models and geostrophic models in our work in Princeton, uh, when I wrote out a précis for the first, uh, primitive equation calculation, which was then made use of by Smagorinsky in, uh, Washington . . . That would have been in 1954 or fifty-five, something like that, maybe even . . . yeah, about then . . . I was fully aware that the primitive . . . that, in the long run, one had to use the primitive equations; but I think that my awareness of that came much earlier as, for example, in this Compendium 1951 article.

[See interviewer's commentary 4, page 152. (GWP)]

P: Now, getting back to 1947, uh, and just to underscore what you've already said, there is this letter -- you mentioned it earlier -- the second letter that you wrote to Phil. This one is, uh, in November of 1947, 4th of November. And there you say . . .

C: Did I say "Eureka!" there? (laughs)

P: Well, yes, essentially . . .

C: (laughs)

P: . . . but you said it in these words. Um. Let me find that here. I have it, I think. You say, quoting now from your letter of November 4th, 1947, that "I have been brooding about the problem of numerical computation ever since coming to Norway, and I think I've come up with an answer to at least one of the most vexing aspects, namely, the practical impossibility of determining the initial vertical velocity and acceleration fields with the necessary accuracy. The solution is so absurdly simple that I hesitate to mention it. It is expressed in the following principle: Assuming conservation of entropy and absence of friction in the free atmosphere, motion of large-scale systems is governed by the laws of conservation of potential temperature and potential vorticity, and by the condition that the field of motion is in hydrostatic and geostrophic balance. This is the required filter. It really does eliminate the small-scale noise. It is possible to justify the approximations used in deriving the
filtering principle by a method of scale analysis analogous to the type of reasoning used in justification of the boundary layer approximations of aerodynamics. The value of the filter for numerical computation lies in the fact that the equations of motion can now easily be reduced to a single equation in the pressure alone."

Unquote, from your letter to Phil. So ... and that was November 1947, so some ... as you've just said, sometime between February of ... your first letter to him, and November, these ... this whole thing crystallized.

[Charney's letter of November 4, 1947 to Philip Thompson is reproduced in Appendix C, page 165. (GWP)]

C: Well, I think, the ... the reason why it happened so quickly was, um, if I may say, implicit in my thesis, in other words, the thesis was a special case ... .

P: Yeah. Yeah.

C: ... but the generalization was easy.

P: Um hm. Okay. Fair enough. Uh. Let me go on to a question that ... to which this gives rise, I think, and it's one that has always puzzled me, about the history of this subject. Um. your paper in 1948, which is the formal statement of this work that we've just been ...

C: Yah.

P: ... uh, talking about, solved the problem of ... of removing the fast waves, uh, from the prediction equations. However, uh, it is also a fact that when the first numerical prediction was made in 1950 on the ENIAC, the model used was the barotropic nondivergent vorticity equation. Uh. There is no vortex tube ... tube stretching there, there's no need to eliminate the fast ... the fast waves from that model because there aren't any. Um. What puzzles me -- getting back to my question -- is that the ... the nondivergent ... two-dimensional nondivergent vorticity equation really had been known all along, uh, one might almost say from time immemorial. Lamb discusses it. Uh. And, uh, I ... it seems reasonable to assume that, uh, even people like Helmholtz or Kelvin, and Rayleigh, were aware of it. Of course, they knew the three-dimensional equations, but I mean ... specifically talking about ... .

C: Yeah, but (?) I think, certainly Helmholtz.

P: And, of course, Rossby adopted that equation as ... as his very special tool. Now, one could argue, I suppose, that the dependent variable of that equation is ... is the stream-
function rather than the pressure, but on the other hand even... even in those days -- we're talking now about 1947 -- it had long since been... the connection between the geostrophic streamfunction and the pressure had been known... was well known. In fact, when I was a student, uh, in meteorology in the early 1940s, I'm... I'm... think I'm correct in saying that we were drawing maps of geostrophically computed vorticity and, um... So. At any rate, the question is, we had the barotropic nondivergent vorticity equation; why didn't anyone think of using it prognostically?

C: I don't know. When I, um... I first... well. I don't know the answer to that question, George. Uh. Richardson didn't think in those terms. Richardson simply thought that the... that the most natural thing to do was to integrate the primitive equations. I believe... and, of course, Richardson's work was adumbrated by V. Bjerknes in 1905 when he... when he made I think, somewhere, um, in a paper or a lecture, at any rate, in a published work which was sort of rediscovered by Yale Mintz, um, that, you know, he... he defined the problem of weather prediction as that of the numerical... of course, he was very much involved with graphical methods in those days, but the idea was that, uh... it was simply the physical problem... the problem of somehow integrating the equations of motion by one numerical means or another. I mean, he... at that time he didn't have any clear suggestion as to how one might go about it. Um. In any case, um... But neither V. Bjerknes nor Richardson, uh... I... I think Rossby, you see... and here... here Rossby's approach, and this is why he and I had such great rapport... Rossby's approach would never have been to do that, because that was formalistic. Both Richardson and... and, uh...

P: To do what?

C: Just to take the equations of motion, which governed everything...

P: Oh. M hm.

C: ... and try to integrate them. I mean Rossby... what characterized his work was that he adapted his equations... I mean, what was... what was, I think, the stroke of genius in his use of the barotropic vorticity... nondivergent vorticity equation, was that he saw that it was adapted to what you could see in front of your eyes, in a way. He didn't have... It was intuitive, because he didn't have a good justification for applying it to the actual atmosphere, in the sense that, you know, in the early days they applied it to the 700 millibar... and obviously if he had tried to apply it to the surface he would have made very large errors, or the 300 millibar chart, maybe
less, so there, um . . . It turned out that it . . . that it gave reasonable results at 700 . . . I tried to give sort of a semi-
empirical justification for the appropriate level at which it . . . so-called level of nondivergence, which was made much use of by, uh, Holmboe and Bjerknes, sort of, in particular I think by Holmboe. Uh, not my paper, but, uh, . . . see Holmboe had been after all under Rossby's influence and . . . and, uh, was aware that . . . that the . . . of . . . that the, uh . . . of the usefulness of the barotropic vorticity equation, and, uh, trying to rationalize it . . . He spoke of a level of nondivergence. Well, for baroclinic waves, there is no such level . . . it's a surface of nondivergence, which tilts. In any case, uh, but if one wants to seek this level in some kind of a rational way, there is a method that I have proposed. Anyway, uh, that's not important. Uh. You asked the question, why hadn't that been integrated before? I think that there wasn't . . . that there hadn't been a Bjerknes and a Rossby who dealt with . . . who . . . whose ideas were based on what they had . . . saw in the atmosphere, and who didn't . . . perhaps that isn't, uh, fair to say it, because certainly I think, uh, V. Bjerknes was very influenced by what he saw in the atmosphere too, but, of course, his son was a member of that school . . .

P: But now Jule, you're speaking of the (?) . . .

C: Uh, but, on the other hand . . . but V. Bjerknes was a classical physicist and somehow it doesn't seem to me that it would have occurred to him to use a filtering approximation. Jack Bjerknes did, in a certain sense. And Rossby certainly did. And the reason they did is that, I think, they were directly in contact with the data, more so than any of the others.

P: All right, but now . . .

C: And so, now, there may have been no technological or engineering or, uh, practical requirement for integrating the two-dimensional Ross . . . vorticity equation. You see, Rossby, when I was in, uh, Norway and, uh, went to visit him in Stockholm, he urged . . . he thought that it was premature for me to join the Princeton project, uh, feeling, as he expressed in . . . in this August 1946 meeting that it was . . . that the wh . . . that the effort to integrate the equations of motion, -- uh, and I have . . . I took notes at that meeting -- that the effort to integrate the equations of motion numerically was perhaps premature because, uh, we didn't really know the equations of motion. And then he proceeded to write down the equations of motion with turbulent friction included as a divergence of a tensor quantity, and he said, you know, that the six components of this tendency . . . tensor were not known.

P: But Jule, this is really a curious and remarkable thing, uh,
and I can only feel that it is a case of, uh, myopia, scientific myopia . . .

C: On Rossby's part.

P: . . . in the extreme. On Rossby's part. Because here, von Neumann, we can only assume, came to Rossby, uh, uh, with the burning desire of having something to work with on numerical weather prediction. And there was a conference of 1946. Wouldn't you have thought . . . (?)

C: Nobody ever thought of integrating the barotropic vorticity equation.

P: But here is . . . here is the leading authority on the barotropic vorticity equation . . .

C: Yeah. But (?)

P: . . . whose advice was being sought . . .

C: Okay, but . . .

P: And here's a man who . . .

C: All right . . .

P: . . . himself in his work with long waves for ten . . . no, almost ten years (?) . . .

C: But Rossby didn't think in terms of numerical integration so it didn't occur to him. Pro . . . you know, uh, I once went . . . visited the MIT Faculty Club for dinner, and on my way out, uh, somebody came up to me and said "I want you to meet so-and-so." So-and-so was a chap, uh . . . There was a large group of people who were having cocktails and there had been a meeting on . . . on hydrodynamic computation, or aerodynamic computation, and the chap said that "You know that your name was mentioned very much during this meeting," he said, "because you were the first to integrate the vorticity equation." (laugh) Uh, appar . . . so that, if I can judge . . . if . . . you know, if I can take his word, nobody had done it before.

P: Uh huh.

C: That came as a considerable surprise to me.

P: (laughs)

C: I wasn't aware that nobody had done it before. I assumed that probably somebody had.
Charney interview

P: What year was that?

C: Oh, that was maybe two or three years ago.

P: Yeah.

C: But, I know exactly when, uh, if you please, the first begin-
ing integration was made. I did that partly in Norway and
partly in Stockholm by hand, using, uh, Richardson's relaxation
method, uh, as I integrated the, uh . . . calculated the Jacobian
. . .

P: How . . . hm . . .

C: . . . crudely and then proceeded to solve the Lapla . . . the
. . . the . . . not the Laplace, what do they call the . . .

P: Poisson?

C: . . . the Poisson equations by hand.

P: What year was that?

C: (?) That was still in forty-seven.

P: Hm.

C: The what . . . the end of forty-seven, the beginning of
forty-eight.

P: Of course, I guess you might say, uh, that, in a certain
sense, after . . .

C: Remember discussing it with Berson. Remember Berson?

P: Yes.

C: Berson was in Chicago. Or no, . . . no, did I meet him in
Chicago or did I meet him in Stockholm?

P: I don't know.

C: He was a South African, or Australian. Australian, perhaps.

P: In an intellectual sense, ha . . . having arrived at the
three-dimensional prediction equation as you did, one might say
that in 1947 -- published in forty-eight -- one might say that it
made the barotropic nondivergent vorticity equation in a sense
more palatable, because it was apparent how it was, uh, a direct
specialization of the more general equation.
C: That's right. And actually I think, you know, and in ... and in ... in my 1949 article, "On the physical basis for numerical weather prediction," I speak of integrating the hierarchy of models ... .

P: Yeah.

C: ... starting with the barotropic vorticity equation, and I ... I felt that the simplest equation was th ... was the barotropic vorticity equation, but there was still a remnant of the generalization, namely, that I ... that I, uh ... if you recall, the, uh ... the dependent variable was pressure, not the streamfunction. You know, we said that, uh ... in fact, I ... I don't remember whether we said the vorticity was del dot del p over \( f \), or del squared p over \( f \), whether we made that approximation. And I think ... But later on, as you remember, because you participated, uh, we used the streamfunction. And ... And we were posing methods of calculating the streamfunction ... .

P: Hm.

C: ... or (?) the geostrophic fl ... I mean, there is ... the streamfunction is not \( p \) over \( f \) or ... I mean, the ... there is an integrability problem there.

P: Yeah. Well, this might be a good point Jule to ask for your assessment of Arnt Eliassen's contribution in his paper of, uh, the same year. In fact, his ... I think ... or was it the following year? At any rate, almost simultaneous with your paper of 1948, that is to say, Eliassen's paper on isobaric coordinates. In connection with numerical weather prediction.

C: Well, uh, how shall I say, um ... I became aware of that slightly ... I think his paper actually came out after I left Oslo. But I think it must have been in seminars or in conversations that I became aware of it. But I somehow, uh, regarded it as perhaps a formalistic approach and, um, and I think that the, uh, ... and never took it very much ... ever made use of it or even took the trouble to compare the results that one might obtain with it, uh, with those that one obtained with the quasi-ge ... quasi-geostrophic equations. But later on, after having worked on the balance system of equations as a generalization, I thought that ... that if one ... put it this way, that if one wanted to generalize the geostrophic approximation and still filter out the gravity inertial oscillations, that the right way to do it was to ... was via the balance equations, and that uh, ... the Eliassen equations were an intermediate step in some sense. But later on, with the work on, uh, frontogenesis initiated by Stone, uh, and then gone into much more deeply by Hoskins and Bretherton, it became apparent to me that the
equations that Hoskins was using were Eliassen's equations, and I coined the term "semi-geostrophic," since we had been using the term "quasi-geostrophic" for my equation. But, I should also say, that I was aware that the geostrophic equations had been used dynamically before, by Ertel. I mean, Ertel has a paper in which he actually evaluates the acceleration geostrophically for a simple motion.

P: What year is that?

C: That was, uh, in about 1945. Before, uh . . . and . . .

P: But wait a minute. If you want . . . if you want to look at the record in that detail, isn't it true that the German Philipps . . . What was his first name? I forget . . . was already making geostrophic approximations of the acceleration in the thirties. In fact, I . . . I remember this only dimly, but I have the impression that he does something like an expansion. Do you recall any of that? Let's . . . let's look into that later.


C: Well, I think . . . You know, in a certain sense . . . In terms of a Rossby number?

P: No. Nothing as formal as that.

C: Um. But I regarded Ertel's work and, to a degree, I think, Eliassen's formulation as . . . as formalistic, not arising from any physical problem.

P: M hm.

C: And, uh . . . As I say, therefore I didn't take th . . . take th . . . their work . . . It played no role in my thinking, and, uh . . . but I think in Eliassen's case, um . . . and I think Fjortoft too, later on, used basically I think what were Eliassen's equations and showed that they are fundamentally much more accurate than, um, the quasi-geostrophic equations, especially when you have things like fronts.

P: M hm.

C: Ah. but also . . . in . . . for intense cyclones. I . . . Eliassen once remarked to me that he thought that there was a problem with those equations, that he himself hadn't used them because, um, he felt that they led to an invariant comp . . . something li . . . comparable to potential vorticity which didn't make any physical sense.
P: Oh really. Hm.

C: But that ... I ... but this ... this is vague recollection. I may be wrong. And furthermore, I had already shown ... well, I knew that they were energetically consistent, and probably Eliassen knew that too. Uh. So. I don't know if I've answered your question.

P: Yes, I think so. I think so.

C: But, uh ...

P: Before ... before proceeding ...

C: And I don't remember Eliassen and I actually discussing, uh, comparing the two formulations. As you say, they were both published in Physikalische Hydrodynamik [Geofysiske Publikasjoner (GWP)]. Mine actually came out in ...

P: Geofysiske Publikasjoner.

C: ... in the ... in the 1948 volume, his in the 1949, but that's immaterial because they were both derived ... 

P: Yeah.

C: ... totally independently.

[See interviewer's commentary 5, page 152. (GWP)]

P: Before we move on to Princeton, which is our next very interesting area ... era, do you have any other general comments about ... 

C: What about Norway? I thought I ... do we skip (?)?

P: Well, no, let's not skip Oslo, by any means. Uh. But I was going to ask you whether you have any other general comments about this period 1946 to forty-eight and, more specifically, your Oslo experience.

C: It turned out that when I came to Oslo I think that Solberg had become the rector. No, I think during the war he was the rector at the university, and that he was very much involved with university affairs. And he gave me an office next to his, but we had essentially no scientific discussions. Uh, I think Solberg by that time, um, whether it was a question of ... I don't think it was a question of personality. I think it was that he didn't encourage them and, uh, by this time I had ... had been introduced to Eliassen and Fjørtoft and Høiland and found them a very congenial active group and, uh ... uh, ... it just
came about that I had very little contact with Solberg. But I . . . I don't consider that a misfortune since, uh, the . . . these three people more than made up for anything I might have missed by . . . from a . . . more of a contact with Solberg. And, of course, V. Bjerknes was very much there. They . . . We had luncheon very often . . . In those days you came . . . not me, but most people came fairly early to work and . . . and had a snack around lunchtime, then left around two o'clock in the afternoon. Um . . .

P: For the rest of the day?

C: For the rest of the day, and when they would have their middags . . . But I remember very much at . . . at . . . at the luncheon snack, uh, meeting V. Bjerknes, discussing . . . talking to him, and what . . . we did not go into my work. Uh, he would reminisce, but I found . . . I found his reminiscences to be absolutely fascinating, because he had been, uh, an assistant to his father, uh, Christian Bjerknes . . .

P: M hm.

C: . . . at many of these international meetings where Christian Bjerknes was demonstrating his hydrody . . . hydrodynamische Fernkräfte . . .

P: Yes.

C: . . . and his rot . . . his oscillating sph . . . spheres. You know, at this point, you know, Kelvin, um, after . . . what was it, J. J. Thomson's discovery of the electron? Uh. Kelvin, who always thought in continuum terms, began likening the at . . . the atom . . . the electron in the nucleus to, uh, vortices and who . . . that could exert . . . that induced effects on each other. And considered a stability of a . . . of . . . I think it was he who first looked into the stability . . .

P: Yes, but I think that analogy, uh, came much earlier than the electron.

C: Yeah. Oh. Did it?

P: Yes, I believe so.

C: In any case, uh, you can see that . . . that Bjerknes's one over r squared attractive force . . .

P: M hm.

C: . . . in an incompressible fluid exerted by an oscillating sphere would, uh, interest people like Kelvin and Helmholtz and,
Charney interview

 uh . . . and, uh, the son, V. Bjerknes, thereby met all these great men. And had been a student of Hertz and all that. So, I mean, here was, through my Norwegian professors at my own time, and then, going one step back to V. Bjerknes, and then, through him, I felt a kind of a historical connection . . . But I'd always been aware, of course, of . . . particularly, of Helmholtz, who was . . . who was one . . . was then and remains one of my great scientific heroes. I felt, um, this kind of historical consciousness of a link to the past, and that the nineteenth century wasn't really all that long ago because here was a living man who knew these great men. And I've always, . . . I think, uh, the motivation in my work . . . it's sort of a subconscious, almost, motivation, is that, uh, you're adding your own little bit to a great scientific tradition. It's in . . . You're not just working in isolation.

P: Did V. Bjerknes speak about Hertz?

C: Um. He must have. But I don't remember any particular things that he said. Uh, at that time, Høiland was working on his, uh . . . V. Bjerknes's, trying to collect . . . and, I think was wor . . . or perhaps it was V. Bjerknes himself . . . was writing a book on electromagnetism. Um. Well, I'm not . . . I . . . I can't remember whether Høiland was writing a book on V. Bjerknes's contributions to electromagnetism or, uh . . . At any rate, uh . . . I'm sure V. Bjerknes never lost his interest in electromagnetism. Uh, I met Stormer and . . . Stormer was a charming man, and mentioned to him that I had . . . you know, he said he was married to the aurora. And, uh, he showed me pictures . . . he . . . he was one of the first to introduce the candid camera to Norway and he . . . and this was in the very early twentieth or late nineteenth century, and he has pictures . . . Ibsen was in the habit of, um, coming regularly, like clockwork . . . you could set . . . set your watch by when he came to his table at the . . . at . . . at the café at the Grand Hotel and would have his schnapps daily and, um, Stormer had taken some pictures of him emerging from the Grand Hotel.

P: Oh really? (laugh)

C: And, uh . . . and when I mentioned to him that I'd never seen an aurora, he kept it in mind and one day about two o'clock in the morning . . .

P: (laughs)

C: . . . I get a telephone call at my pension, saying "Get up! There's an aurora. You could see it if you get up."

P: You mean he had to wake you up at two o'clock in the morning?
C: Yeah. (laughs) So I had a delightful time in Norway and, um, there was much good discussion, not as... possibly, as informal, but, yes, as informal. I think it was aided considerably by, uh, the schnapps. At two o'clock, uh... um... um, Hoiland, Eliassen and Fjortoft and I were not in the habit of going directly home...

P: Oh.

C: ... but, we stopped at a little restaurant called The Valkyrjen to continue... to really, well, to start our... or to continue our scientific discussions over a beer. The beer was very frequently laced with the schnapps and...

P: (laughs)

C: ... but they were, at any rate, rather... rather beery, but very exciting discussions. Scientific discussions.

P: Jule, we have maybe another minute or so left on this side of the tape.

C: Well, let's say that, um... I've already discussed the main work I did in Norway...

P: Yeah.

C: Ah. And, by that... You see, before I left Norway I, uh, wrote to Thompson that... that I would welcome an invitation and, uh, he brought the matter up with von Neumann and I received an invitation to come to Princeton. I decided that that was the most interesting place to be at that time. Even though, at that time, Bjerknes, when I visited him... had come to his home in Bergen... and when I visited him he offered me a position at UCLA, and pointed out that there were other possible positions, one, I think, in Seattle. But by then I was already convinced that, uh, the thing for me to do was to go to Princeton. Because I was convinced that I... that... that I knew how to integrate the equations of motions.

P: Hm. That's a very good statement on which to stop.

C: Yeah (?)..
The interview with Jule G. Charney continues. This fourth session of the interview is being conducted on Thursday, the 28th of August, 1980, in Boston at the Holiday Inn. Now we're in 1948, going to Princeton. Perhaps the first question is, what led to your association with Princeton, with von Neumann.

C: Yeah. I told you, uh, that when I was at Chicago, one of the inducements that Rossby offered me to stay at Chicago for a longer time than two weeks, or to stay for the . . . basically for the academic year, was that, uh, there was going to be this meeting . . .

P: Oh yes.

C: . . . at the Institute for Advanced Study, uh, to discuss the establishment of a meteorological group . . . meteorological work, um, at the Institute for Advan . . . under his direction at the Institute for Advanced Study. Ah, I don't . . . we probably didn't . . . we didn't go to any extent into the motivations that von Neumann had and the early beginnings. I wasn't there. Uh, well I . . . I went in . . . in the 1946 project. He had already established some connection with, uh, Panofsky and Haurwitz. They had, uh, to do objective analysis . . .

P: Oh, I remember that.

C: . . . and, uh, and I think that he had already had some commitments. One, from Bob Elliott to come there. And, uh, but the early beginnings . . . I know Reichelderfer said that he had been to see him. There was correspondence with ONR and, uh, through . . . and one of the intermediaries was Dan Rex. Um. His early interest in meteorology I think could very well . . . Zworykin says that, uh, . . . who was a good friend of von Neumann's . . . uh, Zworykin was then with GE . . . RCA labs . . . and, um, Zworykin was interested in the meteorological problem. And I think it very likely that through their friendship that von Neumann took up the meteorological problem as sort of par excellence a problem for a large computer . . . scientific problem for a large computer. Um, but previous to that, he had become interested in hydrodynamical problems in relationship to the Manhattan Project and the design of, uh, implosion devices and such things. And later on I think he did some hydrodynamical work in connection with the H-bomb. And I know that . . . that when the Prin . . . the computer at the Institute for Advanced Study was completed, one of the first, uh, problems was a highly classified problem . . . it concerned the H-bomb. I never knew what it was exactly, but . . . or not at all, and, in fact, didn't want to know. (laughter) . . . but met a number of the people from Los Alamos. And, at the time that
... well, I don't think it's appropriate to go in ... Others have gone into the ... the, uh, requ ... in ... into von Neumann's work on the design of logical controls for computers and so forth, and I don't know if it's appropriate ... I don't think it is appropriate to do that here.

P: When you say that ... that he had already contacted Panofsky and ... did you say Haurwitz?

C: and Haurwitz at ... at NYU.

P: Uh, was that ... do you mean before 19 ... before August 1946?

C: Before August 1946.

P: But ...

C: And I think Elliott too.

P: But, uh, isn't it true that there was no, uh, established project ... .

C: No, there was no project. In fact, nobody was ... that ... had then come to the Institute. Um. And von Neumann was at ... had analyzed the magnitude of the meteorological problem and had decided that it was compatible with the memory of the computer and the speed, uh, in some rather simple form. I think, as it turns out, he was overly optimistic, but, uh ... but it was enormously helpful in the design, actually, of the computer to know what was the order of magnitude of ... you know, what ... that is, the magnitude in terms of computing time, which meant, in those days, multiplication time, division time, and storage capacity. And, I think, in the early ... in his connection with Zworykin there was a electronic physicist, electronic engineer, named Jan Rajchman, who had designed what he called a memory tube ... called a selectron, and the original intention was to use it for the memory, but I think that they had problems with that and they ended up by using the Williams tube, which was a kind of modification of a cathode ray tube, a bank of Williams tube memories. And altogether I think the original computer had a memory for, uh, for one thousand and twenty four, uh, twelve-digit ... twelve decimal digit numbers, or something of that kind.

P: Was the Williams tube an English product?

C: Yes, I ... I believe so. At any rate, by the ... the time this meeting was held, I think they had a perf ... a fairly clear idea of what kind of machine they wanted to build and I think the meeting was held to enlist the support of the
meteorological community and their . . . some of the more, uh, well-known names were present. I was not a well-known name, but thanks to the offices of Rossby, I think, I was able to . . . permitted to attend. And I recall a number of things . . . I jotted down a few notes when I . . . at the time that . . . von Neumann began it with pointing out some of the difficulties involving a Courant-Friedrichs-Lewy condition and, uh, Rossby, as I've already mentioned, um, strongly urged that the problem was not simply a mathematical one, but a physical one, and that whatever group was assembled would have to work on the physics. And he illustrated his point, as I mentioned, by writing down the turbulent Navier-Stokes equations and pointing out that we still didn't know, uh, the components of the stress tensor . . . turbulent stress tensor. Um. This wasn't, you know, uh . . . von Neumann was really incapable of being strictly negative about anything. I mean, it was a positive approach, but, uh, . . . and I think that, um . . .

P: You said "von Neumann"; did you mean "Rossby"?

C: Oh, Rossby . . . uh, that he was entirely correct in his suggestion that the problem was by no means a simple mathematical one, or even a complicated mathematical one. Von Neumann was quite capable of accepting that, because von Neumann was . . . had a very deep knowledge of physics as well as mathematics. Um. Some of the people who attended were . . . I mentioned Haurwitz . . . but, uh, I believe Queney was there; but, if I don't mistake (?). I think Harry Wexler was there. Walter Elsasser, Chaim Pekeris. They already had some connection with the Institute for Advanced Study and I think Chaim Pekeris and Els . . . Elsasser was then working on the geomagnetic problem, and, uh, Pekeris was then interested in the Orr-Sommerfeld, using computer for, uh, calculating the accurate, um, . . . marginal stability curves for Poiseuille flow . . . which I think he subsequently did, although I think the first to actually use the computer for that purpose was Thomas, a man named Thomas. Uh. I myself made some very esoteric comment. It didn't occur to me . . . You see, at that time . . . that the solution was to use the quasi-geostrophic equations. I . . . I suspected that the primitive equations were not appropriate but, um, you see at th . . . then I really didn't know what the generalization of the geostrophic approximation, that I had used in my thesis, was. And in this august company I felt that I had to . . . you know, that anything as simple-minded as that would be out anyway, that I had to talk about . . . I actually raised the question that, if you knew the motion at a given level in the atmosphere as a function of time, to what extent could you infer the upper . . . the motion at other levels. I returned to that point later on, but I think it's a highly unstable inverse interpretation problem. And nobody paid much attention to it. (laughs) Uh. Justifiably. But the plan at that time definitely
was to integrate the primitive equations, and I think that Elliott was going to be put in charge. That never de . . . that never happened. I think Elliott fo . . . I forget why, never turned up. Um. At any rate, I was enormously impressed by von Neumann. Rather overwhelmingly, I think. Uh. And, uh, started to think about the numerical prediction problem. And I . . . I . . . I felt that if the . . . if one were . . . that is, the only physical approach to . . . to forecasting would be by integration of the equations of motion in some form or other. I was absolutely convinced of that. It didn't take . . . And, of course, the exciting thing was that here was a possibility. I was then somewhat familiar with Richardson's work. So that after a while . . . so I've already re . . . you've . . . we've already covered the mechanism by which I finally came to the Institute for Advanced Study, when I . . . so that when I left, uh . . . I left Norway in early spring of 1948 and came directly, via France and England, to the Institute for Advanced Study. At that time, hous . . . the housing for . . . uh, Bigelow had arranged to, uh, . . . had aqui . . . had arranged for the Institute to acquire some WPA or some kind of miners' . . . WPA miners' housing, and these buildings were actually towed to the Institute and established there. And . . . and they . . . they were fairly primitive, with coal stove . . . coal furnaces and . . . well you remember that, George?

P: Oh, I remember the barracks but I didn't realize that Julian . . . Julian was responsible for that.

C: Yeah. We should say that Julian Bigelow was the chief engineer, and he, von Neumann, and Goldstine were the prime movers in . . . although I think, uh, that some of the mathematical problems of . . . of the design of the . . . of the computer were . . . related to the design of the computer had already been discussed and reports had been written by not only Goldstine but Valentine Bargmann, the physicist at Princeton and the other chap who was very active in computer work and whose name escapes me at the minute. Mathematician.

P: I'm amused at the comment about Julian Bigelow's transporting those barracks because . . .

C: He did that with his house. (laughter)

P: He did the same thing with his own house but (?) . . .

C: He bought . . . he bought an old blacksmith shop and . . . and moved it to . . . closer to the Institute, and it turned out that he had forgotten that there were high wires, so he cut the thing in half like a layer cake (laughs), then had to bolt it together. Anyway, so I came there in the spring. While I was in Norway, uh . . .
P: Spring of . . .?

C: In the spring of forty-eight. While I was in Norway, uh, Petterssen was then in charge of the weather forecast . . . the weather forecasting of the Meteorological Institute, the Norwegian weather service. And I had come . . . I . . . I came to discuss with him the possibility of bringing, uh, Fjørtoft and Eliassen to Princeton, and suggesting first Fjørtoft because I'd really . . .

P: Excuse me. You came to discuss with whom?

C: Petterssen. Sverre Petterssen.

P: Oh.

C: And, uh, he was all in favor of the idea. Of course, these . . . the Norwegians had been completely cut off from communications with the rest of the world during the war, and this would, I felt, be very good for the project. And . . . And I felt for sure that when von Neumann knew who they were and of their abilities that he would fall right in. Because at that time . . . I think, of course, by that time Phil Thompson had come there and, uh . . .

P: Well, excuse me, but Phil . . . Phil ca . . . went to Princeton already in the autumn of forty-six . . . was it forty-six or seven?

C: Well, it would be forty . . .

P: Forty-six.

C: Yeah. Well, in the winter, perhaps, of forty-six, or early spring.

P: Late autumn.

C: Late autumn.

P: Late autumn. Yes.

C: Well then, he must have come there shortly after this meeting.

P: Yes.

C: And, uh . . .

P: But he didn't know about the meeting.
C: And, uh . . . No. But Gilbert Hunt, uh, who, uh, first-rate mathematician who had then, I think, just gotten his doctor's degree, and had served in the, uh, air force as a weather officer, was brought there, but the interesting . . . He was . . . The problem that he was brought there really to work on was the, uh, . . . the su . . . the well po . . . it was a problem that Leray had studied, namely, to what extent was the solution of the Navier-Stokes equations, . . . uh, an existence and uniqueness proof of the solution of the Navier-Stokes equations. I mean, there were some real questions about whether the solution was unique. And, uh, Volodner (?) had already shown that the two-dimensional equations . . . shown the existence and uniqueness for them. Later on, I became interested in a very amateurish way with the . . . because of the very close mathematical relationship between the general quasi-geostrophic equations and the two-dimensional vorticity equation . . . with the existence and uniqueness of . . . of the solutions of the quasi-geostrophic equations, and felt that the techniques that Volodner had used could probably be applied to that, because of the mathematical ana . . . analogies.

P: Did you ever pursue that?

C: But I never pursued it to any final conclusion.

P: Of course, there's been a lot more work done on these questions in recent years.

C: Yeah.

P: Some quad (?) . . .

C: Well, I . . . yes, I think that . . . that in . . . in the . . . in the final simplified form the quasi-geostrophic equations as we know them now . . . I feel quite sure that the proof could be carried through. I'm not aware that it has been. But these were mathematical problems which were, I would say, certainly secondary. I was much more interested in other things at that time. Um.

P: I'd like to go back for a minute to 1947, before you, uh, arrived in Princeton to stay. Did you stop in Princeton on your way from Chicago to Oslo to visit Phil?

C: God, I don't remember. I think I may have. I'm not sure. I think I may have. Phil would probably remember better than I.

P: M hm. Okay.

C: Ah . . .
P: Go ahead then. Mm. What . . .

C: And I'm not . . . I may even have met von Neumann, but he . . . you know, that is . . . that is . . . that . . . I don't recall right now.

P: What . . . Can you describe the status of the project at the time you arrived in Princeton?

C: Um.

P: You said the spring of forty-eight. Isn't it . . . wasn't it the summer?

C: Well, late spring.

P: Late spring.

C: Yeah, it may have been as late as June. Because I remember stopping in, ah . . . in, ah, England, visiting Imperial College. I think . . . I think I may have seen Eady then. And then going out to Cambridge. And I've already mentioned to you . . . I don't know whether this was on the record, uh, seeing Jeffreys.

P: Uh hm.

C: But that was just to pay my respects.

P: Yeah.

C: By that time I knew where I was going. In fact, I think after this August meeting in forty-six at . . . at the Institute, uh, I was pretty sure that I would end up at the Institute for Advanced Study. I think von Neumann, uh, well, come to think of it, von Neumann I think actually invited me at that point, or, uh, not in a com . . . a very formal way, but I think he asked me about my interest and I said yes I was very interested, but I had this fellowship and I'd already committed myself to going to Norway, but that afterwards I would be interested.

P: So what did you find when you arrived there?

C: Well, I found that they had . . . Let's see, I think the, uh, . . . the computer project building had al . . . had been built, uh, the housing was there, uh, the two people who were on premises were Thompson and Gilbert Hunt. I told you what Hunt was working on, um . . . At that time . . . well, I think Thompson has . . . has in . . . in . . . in a lecture, which I think is available, talked about his work. And, as I recall, he was interested in finite amplitude . . . to show that there were certain finite amplitude solutions of the Rossby-Haurwitz problem
... long wave problem on a sphere or in a beta-plane. Uh, it was kind of a, perhaps, generalization of Neamtan's work. And, uh, obviously concerned and working on the, uh, problems of the solution of the Richardson approach to the primitive equations. Ah, then of course the engineering group, who were already beginning to, uh, construct the . . . the computer.

P: This . . . this brings up a question . . . (?)

C: Or, well, I've forgotten . . . I . . . I can't tell you exactly when they started that.

P: When you arrived there, you already had the quasi-geostrophic prediction equation in your pocket.

C: Yeah, I did.

P: And that was in, let's say, the middle of 1948. Yet, it wasn't until . . . was it April of 1950 that the first attempt to use the . . . that equation was made, on a computer.

C: Well. Yeah, well, what . . . All right, well, the reason was . . .

P: . . . what (?) . . .

C: . . . that . . . that . . . that there . . . there were . . . that the schedule for the completion of the Princeton computer, or IAS computer, um, had slipped a great deal, and they had expected . . . I think they . . . they probably began working on it in maybe forty-six or forty-seven. Uh. And they thought that by fifty or so that it would have been . . . would have been completed. But it was not really completed until fifty-two, I think it was. And, uh, so a lot of the work was of a quasi-theoretical nature, and preparing. Now, I mentioned to you -- I think I did -- that . . . that I thought the logical thing . . . that, yes, indeed . . . that . . . that I thought I saw how to integrate, in general, quasi-geostrophic equations. It was a matter of, uh, you know, of advecting the potential vorticity and then solving a Poisson equation. And that was a perfectly straightforward thing. And I had even given some thought . . . I never published this . . . for the integration of . . . and then, well . . . but that the simplest problem and . . . considering that some success had already been achieved by Rossby and his students with the barotropic or two-dimensional vorticity equation that . . . that that was a . . . the logical starting point, but with . . . but . . . but the intention of quickly going to baroclinic, uh, flows, because I didn't . . . it was quite . . . after all, my thesis was . . . in other words, that . . . that while certain dispersive properties of nonlinear motions could be predicted with a barotropic model, the major pr . . . the haute problème of
of meteorology as I saw it in those days, in agreement with V. Bjerknes and his school, was the prediction of cyclogenesis.

P: M hm.

C: And nobody expected that we would predict cyclogenesis with a two-dimensional model.

P: Yeah.

C: But, I felt very strongly that . . . that, as expressed in the paper which I wrote in 1948 and published in 1949, "On a physical basis of numerical prediction of large-scale motions in the atmosphere." Something like that. That . . . that one should deal with a hierarchy of models and introduce new, um, . . . new physical and mathematical factors one at a time. Otherwise, one would . . . if things went wrong, you would never know what had gone wrong. I think that that's . . . I think that philosophy is as valid today as it was then, and, um . . . and people who don't pay attention to it have . . . suffer the consequences. And that the problem even now with the GCMs is that they . . . too many factors are introduced all at once, and when something . . . and it's very difficult to know, uh, what each of the physical and mathematical factors contributes. Although I think people are beginning to . . . to cope with that problem. Anyway, uh, so that was logical, and I felt that it would be very pleasing to Rossby (laughs) to do that.

P: Do you . . . Do you . . . Do you think, looking back, that the practical usefulness of the barotropic equation was generally underestimated?

C: I think so indeed. And, uh, . . . of course, I think . . . I think we were all rather surprised that they . . . that the predictions were as good as they were. Uh . . .

P: But now, coming back to this delay of . . .

C: (?)

P: . . . of over eighteen months. Might it not have been possible, in principle in . . . -- in reality --, to have done the ENIAC calculations a year earlier than they were done?

C: Ah, von Neumann didn't think in tho . . . in those terms, because the ENIAC had such a c . . . totally limited memory of . . . of only, um . . . As I remember it had a . . . a variable memory of twenty, uh, twelve-digit numbers, twelve decimal digit . . . forty binary digits, I think it was. And there, of course, was the prob . . . possibility of doubling or quadrupling the (?) . . .
P: It wasn't a binary machine. ENIAC was a decimal machine.
C: No, think it was a binary.
P: Well, it had . . . each . . . each number was represented by ten vacuum tubes.
C: Ten.
P: Each digit, I should say, was represented by ten vacuum tubes. That's my recollection of it.

[See interviewer's commentary 6, page 155. (GWP)]

C: Well, you . . . you may very well be right. I'm . . . I'm . . . That's . . . can easily be checked. In any case, the memory was totally inadequate, as one saw, for the meteorological problem. And I don't think it occurred to von Neumann in the beginning that that would be the . . . I think though that when the completion of the Princeton computer was delayed, um, he became very impatient and began thinking in terms of getting a preliminary . . . making a preliminary calculation on the ENIAC and then worked out this Fourier transform method, um, for doing it, solving the Poisson equation basically . . .
P: Yeah.
C: . . . by, uh, the Liebmann modified relaxation technique -- over-relaxation.
P: That came later.
C: That certainly was not yet. He didn't think in those terms in 1948. No. I think in 1948 he was hoping that the first calculations . . . Well, there had to be a period of indoctrination. At that point, von Neumann didn't know of . . . of . . . except maybe what Phil Thompson may have told him, about the quasi-geostrophic equations. And we had numerous sessions, in which I tried to explain what the problem . . . and . . . and . . . of . . . but I think ultimately he fell in with the idea.
P: Yes, but what I . . . what . . . what I mean to say is that the Fourier transform was used on the ENIAC.
C: That's right.
P: And the Lieb . . . the relaxation method was used later, on the Princeton computer.
C: Oh, forgive me. You're absolutely right. Yes. Yes yes.
integrate the non...the inviscid adiabatic equations.

P: When you say friction, do you...do you use that term deliberately, imp...to imply Rayleigh friction, or...

C: No. No no. Um. Navier-Stokes friction. In two dimensions, to begin with, but...but, in three dimensions, it would have been...At that point, it was really Eliassen's...who...who first pointed out that Ekman friction, uh...I mean, that friction should be treated as an Ekman problem and that...and that one could reduce that to a bound...to a boundary condition on the vertical velocity. That was incorporated in our joint paper.

P: I've always...I've always had the impression that, uh, Brunt deserves more credit in relation to that point than he has got...gotten. But we can discuss that.

C: (?) What did he do?

P: Well, in his book, he has a discussion of isallobaric convergence in the Ekman layer.

C: Yeah, but I think that's not the same thing as...

P: No, quite.

C:...as actually expressing it as a boundary layer...

P: That's true. He didn't go as far.

C: (?) Yeah.

P: Because he didn't have that problem in mind.

C: But Eliassen and I in our paper did do that...

P: Yeah.

C:...and actually calculated the spin-down. And that, of course, reinforced our view that, uh, friction could be safely omitted if one were concerned only with twenty-four prediction.

P: You were talking about von Neumann a moment ago. Uh. How did you interact with von Neumann?

C: Well, I found...of course, von Neumann, um...You see, I had been trained as a mathematician and I looked upon von Neumann as some sort of a god (laughs) at first. But von Neumann was an eminently approachable god and, um...I...and...and extremely...he was rather, uh...uh, he thought so
fast that he very often anticipated what one was going to say, so one felt that one had to be, um, very succinct and well prepared in discussing things with him. But fundamentally, he was a very, uh, a . . . pleasant, agreeable person with whom I got along very well, and eventually we became close friends, uh, and, uh . . . both with him and his wife and . . .

P: What . . . what can you . . .

C: . . . and so I had no difficulty in discussing things with von Neumann. Again, he wasn't a Rossby. He wasn't, by any means, a Rossby, because he was in . . . not intui . . . I mean, well, possibly you could say he was intuitive, but I . . . his outstanding characteristic was the amazing logic of his thought processes.

P: Mm.

C: I mean, there are many legends about this.

P: Yeah. What . . . What do you think were his strengths and weaknesses?

C: von Neumann?

P: Yes.

C: Oh well, I . . . I don't think that it's up . . . that it's for me to say actually.

P: Well.

C: For one thing, um, von Neumann had then . . . by this time become very much absorbed in the computer, and in the meteorological problem. Um. At the same time, he was consultant to a number of government agencies, and also to IBM, as I recall. Or that may have come a little bit later. And, uh, this was very greatly decried by a number of the mathematicians at the Institute for Advanced Study, because they felt that his . . . that his mathematical con . . . contributions were by far the most important thing that he could do.

P: Do you think they might have been right?

C: (?) And that he was wasting his time in these more mundane occupations.

P: Might they have been right?

C: It's not for me to say. I think that a person goes through certain stages in life. I think that he may have felt that . . .
that a mathematician... mathematics is a young man's game and
that he... but, not only that, you see, von Neumann...
What they didn't appreciate was that von Neumann had, in his
entire professional career, had a strong interest in physics. He
was actually trained as a chemical engineer in Budapest. Uh.
And, you know, he had written this... this book on theory of
groups, quantum mechanics and, uh, his interest in logic
naturally brought him to the theory of automata and the logical
design of computers. And, at the same time, as many European,
especially middle-European, mathematicians, he had a... it was
(?) that he had a strong interest in physics. I don't remember
where he took his degree. Was it at Göttingen?

P: I think probably it was.

[von Neumann received a Ph.D. in mathematics at the University of
Budapest; later he studied at Göttingen. (GWP)]

C: But I think he was very strongly under Hilbert's influence
too. And Hilbert had a great interest in physics. Uh. To be
sure, theoretical physics. But von Neumann, like all great men,
regarded the whole world as his domain and... and he saw...
and that hydrodynamics was a fascinating field to him. He had, I
think, just after the war, been commissioned by the... by the
Navy to make a survey of work in turbulence, and he wrote a paper
on turbulence which to this day is, I think, a model of insight
into the turbulent problem. And I think he thought of meteor
... so there was... the connection between turbulence and
meteorology was very natural. So I... obviously, I... I
would not say that he was... that he was wasting his time on
... on the meteorological problem. Uh. And I think that his
leaving the Institute for Advanced Study, uh, was very much
occasioned by the fact that he... that he was so... that he
and a number of the people... (?)... of the mathematicians,
particularly, at the Institute for Advanced Study were out of
sympathy with each other. I mean, there was a strong, uh, body
of opinion on the part of a number of people who were otherwise
his friends at the Institute for Advanced Study that nothing
experimental should be undertaken.

P: Yeah. Yes, that was...

C: And that, eventually, even after von Neumann left,...
that feeling penetrated ver... rather deeply into my own
consciousness, and it was probably the main reason why I left the
Institute for Advanced Study. In other words, I felt... I
didn't want to be in a... in an institution where, um, a
number of the people, a number of the dominant personalities
there, felt... were totally out of sympathy with what I would
be doing.
P: This is a good place to turn the tape, Jule.
C: George, this isn't (?) . . . we'd better . . .

END OF TAPE 4, SIDE 1
P: Okay. Here we go.

C: Ready?

P: Yep.

C: I should say that, um, the adverse, um, opinion of a number of the permanent faculty of the . . . mathematicians on the permanent faculty of the Institute for Advanced Study was balanced by a favorable, uh, uh, by favorable support from some of the other mathematicians and . . . and, uh, particularly by the physicists. When I went to the meeting in August, Aydelotte was still the director of the Institute. But when I came in, uh, . . . that is, in August of forty-six . . . but when I came in the spring or summer of forty-eight Oppenheimer had just arrived. He wa (?) . . . as the director. And Freeman Dyson, I think, uh, came. I don't remember whether he came as a permanent member or professor, but he, uh, had then, uh, done for quantum electrodynamics what Sch. . . . what Schrödinger had done for quantum mechanics, shown the essential identity of the Feynman and Schwinger approaches and, uh, on the strength of which he was made a permanent member and professor. Dyson was one of the more sympathetic people, and whom I came to know and became very fond of. Um, Dyson . . . his experience and, even though he was highly theoretical, was prone to . . . and he later wrote in an article, "How not to do physics," how . . . how some of the greatest prot . . . developments in . . . physical developments in modern times were done over the objections of the establish . . . the then-existing establishment. One . . . one he mentioned was radio astronomy in Cambridge.

P: Mm hm.

C: And another was the building of the computer and the meteorology project. He said it was probably a pity that the meteorology project was not kept in Cambridge . . . in . . . in . . . in Princeton, because it would have attracted, it would have served as a beacon for the field. And I wondered sometimes whether I did right in leaving the Institute, because it . . . even though . . . and, that is, to sort of sit out the opposition. When I decided to leave, Oppenheimer urged me to stay and, uh, said that I had a fifty percent chance of being appointed a professor. And he said that . . . that, because they felt that they wanted an applied mathematician . . . and he said, that he thought that my competition then would be Chandrasekhar. Did I ever tell you that?

P: No.
C: But I had enough modesty left at that point to feel that I
could . . . I was no competition for Chandrasekhar, even if I had
wanted to stay, but I really didn't want to stay anyway. (laughs) Uh.

P: Well Chandra judging from events had no great affinity . . .
(?)

C: I don't know whether he was ever actually offered the
position or not. They finally did bring an astronomer, Strømgren.

P: Mm hm.

C: . . . who left . . . who . . . who found that he was . . .
who didn't enjoy the isolation and didn't remain very long, I
think returned to Copenhagen.

P: Well now, you mentioned Dyson. Did you have important intel-
lectual contacts in Princeton, other than von Neumann that were
important for you professionally?

C: I wouldn't . . . that is, outside of our own group at the
Institute . . .

P: Yes.

C: . . . in . . . at the met . . . the meteorological group.
Um. I wouldn't say that actually. I also became friendly with
Frank Yang and did discuss what has later come to be called the
silent area problem.

P: What problem?

C: The silent area. That is, if you know what happens on the
boundary as a function of time, to what extent can you infer what
is going on in the interior.

P: Mm.

C: Um. Oh, and I think I would discuss mathematical . . . some
mathematical problems from time to time, mainly with some of the
physicists, but that . . . I don't think that led to any, uh, usable techniques or ideas that we incorporated in our work. You
see, that was the whole point. I mean, that for kindred spirits,
really, one had to go outside. But, of course, I should say that
there were a great many visitors who came to the project, uh,
beginning with Eliassen, who arrived, I think it was, was it late
forty-eight or early forty-nine.

P: Go ahead.
Charney interview

Tape 4, side 2 100

(Doors opening and closing. Another voice?)

C: Um. You remember when Eliassen arrived, George?

P: No, I don't.

C: He did come with me in June, but I think he may have come in the fall of forty-eight and... for a year. I think he must have stayed from forty-eight to... fall of forty-eight to fall of forty-nine. And then Fjørtoft came.

P: Yeah.

C: It was forty-nine. And when did you come?

P: Golly, I don't recall exactly. You thin... I don't remember whether I was there before EN... ENIAC? You could look all those things up.

C: Oh yeah, you were certainly there before ENIAC.

P: Was I?

C: Because you... well, after all, you... you helped... 

P: Yeah, yes, that's right.

C:... we both worked together in... in programming the calculations.

P: Yeah. We could look all those things up.

C: That was for... that was sometime... must ha... that was before forty-nine, I believe.

P: Mm.

C: I mean, before 1950.

P: Before fifty.

C: It must have been while Fjørtoft was still there.

[See interviewer's commentary 8, page 156. (GWP)]

P: Let me come back to a point that we touched on very lightly before we... 

C: Well, okay... one minute... I mean... Let me just list a few of the people who came there. There were the three of you. Well, you were more than visitors, you were participants.
But then, in addition to that, there were, uh . . . I invited Eady. Eady would . . . Eady was not there when you were there. I think the subs . . .

P: No.

C: . . . But Eady was there when Fjortoft was there.

P: Mm hm.

C: So Eady was there in 1950 for about a half year.

P: Mm hm.

C: But he remained, uh, rather much to himself, working, as I mentioned to you, on incorporating beta . . .

P: Yes.

C: . . . in his model. Um. Of course, Phil Thompson and . . . and Gil Hunt were already there. Um. Then, Rossby, of course, would visit us from time to time and, um, it was through his agency that Bolin came, and Berggren, from Stockholm. Um, Gambo, uh, Wadati, the director of the . . . of the Japanese Meteorological and Geophysical Service, whatever it was called, visited us. I think, Syöno came. And through Wadati's agency, Gambo . . . He asked that . . . to send a . . . a . . . who was then, he felt, his best man, uh, came . . . visited us. George Forsythe from Stanford, who was . . . had been a . . . a colleague of mine in the department at UCLA. Uh, and I think other visitors . . . who don't occur to me right now. So that we were not isolated by any means. Okay. You had a question.

P: Yes. Now this changes the subject of . . .

C: (?)

P: . . . but we touched very briefly on the Princeton computer, the Institute computer, and the fact that it was delayed . . . completion of it was delayed, and that this, to some extent, accounted for the . . .

C: Decision to, uh . . .

P: . . . decision to go to ENIAC. Well, it's . . . yeah. Well, the question is this: As I recall, the Princeton computer really was not usefully operative until 1952.

C: That's right.

P: Now, this was a significant delay, and the question is
whether that delay had a serious impact on the progress of work at Princeton. Of the meteorology work.

C: Yes, I think so, because, uh, in forty-eight, I think, we . . . that, you know, that . . . that, uh, we already knew, uh, what the hierarchy of models was. Now, um, we hadn't given a great deal of thought to, except in a general way, how one would go about integrating the three-dimensional, uh, quasi-geostrophic equations. Uh. But by that time, uh, . . . short . . . af . . . I think or so . . . a year or so afterwards, um, I visited Chicago . . . visited you in Chicago, and, uh . . . no no, that was later. I think it was after the ENIAC calculations I visited you in Chicago, and you introduced me to a young man named Norman Phillips, who had just taken his doctor's degree, or was getting his doctor's degree, uh . . . I don't know whether he was your student or Rossby's student or . . .

P: I think the . . . the . . . Isn't this . . . chronology there that I brought Phillips with me to the second ENIAC expedition, in 1951. That's how he got cued into the Princeton work.

C: I seem to remember, George, that . . . that, uh, even before that . . .

P: Really?

C: . . . that I was in Chicago . . .

P: Quite possible (?).

C: . . . that I was in Chicago and Phillips just outlined very briefly on the blackboard what he had done. And what he had done, of course, was to develop the . . . the two-layer . . .

P: Yeah.

C: . . . or the two-level. It was actually a two-layer model.

P: Yes, it was.

C: And you would und . . . you would know better than I . . . it was somewhat inspired by the geostrophic approximation equations, but I think it would be historically of considerable interest to know what led him to do that. You would know that, would you not?

P: Yes. That's another matter.

C: Was it you or was it Rossby who . . .

P: I don't . . . I don't really at the . . . I would . . . This
is something I'd have to think about. (?) It doesn't come to me offhand.

[See interviewer's commentary 9, page 156. (GWP)]

C: Well, we can ask him. In any case, um, uh, I recognized that right away as . . . as the next step. I hadn't proposed to do it in that way, but it turned out that the finite difference formulation in the simplest way, by vertical differencing of the quasi-geostrophic . . . geostrophic equations, one arrives . . . and later we showed, basically, that one arrives at the same equations as one gets for a two-layer model. So it was clear that that was the next step and . . . so after we had done the integrations of the . . . of the two-dimensional model on the ENIAC, and then subsequently on the Princeton computer, the . . . the logical next step was to go to two-lay . . . to, what we called, or what I defined as the two-and-a-half-dimensional model. Or maybe Eady first used that term.

P: I think he did. Yeah.

C: And, I think at that time, Eady had, whether independently or not, I don't . . . don't recall. Both Eady and Thompson had formulated two-and-a-half-dimensional models of their own.

P: Yeah. Um. I want to come now to a . . . a different matter, namely, operational numerical weather prediction, which came into being during the time that you were in Princeton. In fact, uh, the . . . the first operational organization was in 1954.

C: That's right. What happened was that Shuman and Cressman were detailed by the Weather Bureau to spend some time -- and that may have been in 1953 -- . . . uh, I think, in my . . . uh, I'm not absolutely certain of this, but, you see, what happened was that we used it . . . that in . . . in November of 1950 there was a . . . a very severe . . . in Thanksgiving of 1950, there was a very severe storm on the northeast coast, and, uh . . . which caused a great deal of damage, and in particular knocked part of the roof off the Palmer Laboratory . . . (laughs) . . . at the university. And we took that as our . . . as our . . . we got the data for that, and with the first baroclinic integration with the two-and-a-half-dimensional model . . . we used the initial data. And I think that Palmén had analyzed that, and for all I know he may have gotten some . . . th . . . th . . . his analysis of . . . of the initial data. He had analyzed that storm in some detail. It was a very sudden and rapid, uh, development of a major cyclone. Uh, just on the east coast of the United . . . the northeast coast of the United States. Or the east coast. Uh. And the two-and-a-half-dimensional model did not catch the cyclogenesis. There was some vague indication of something going on. But then, I . . . By then, of course, we
had . . . or I had formulated a two to the $n$-over . . . $n$-minus-one-over-$n$-dimensional model. (laughs) $n$ being the number of intermediate levels. And so we went to a three-level model, that is a two-and-two-thirds-dimensional model (laughs), and we did catch the cyclogenesis. It wasn't terribly accurate, but there was no question that . . . And I always thought that this was a terribly important thing. It hasn't received that much attention in the subsequent literature, but I published a short paper on that.

P: Yes, I remember that, but now (?) . . .

C: . . . I think in . . . in . . . in the Proceedings of the National Academy.

P: Yeah.

C: I wanted the world to know about that.

P: Right. But Jule, that, uh . . . I . . . I'd forgotten about that until you just mentioned it, and it brings to mind a strange thought that I acquired about . . . well, it must have been shortly after that, perhaps within a year after the appearance of that work. And I don't know who put this thought in my mind, but it might have been Norman, that there was some difficulty with the interpretation, with the interpretation of the results of the numerical computation, some error in the numerical computation, which, um . . . which made the conclusion that the model had predicted cyclogen . . . cyclogenesis somewhat suspect. Do you have any such . . .?

C: No.

P: You don't? Well, I should probably strike it from the record, or now that I've . . . now that it's in there we ought to (?) . . .

C: Whether we (?) strike it from the record, uh . . . I mean . . .

P: We should ask Norman what he (?) . . .

[See interviewer's commentary 10, page 157. (GWP)]

C: Yeah. Well. Indeed. I think we should, and . . . and, um, but I'm not aware of it. What I am aware of . . . that, you know, that there are arbitrary ways of formulating two . . . two-and-a-half-dimensional, that is, two-level or . . . models. If you choose the level . . . There's no reason why the levels have to be . . . or why the . . . have to be at any particular pressure . . . pressure levels. Of course, we always used
pressure as vertical coordinate, and we did not take topography into account in these early models, nor friction (?) In any case, uh, I think, with this somewhat different formulation where the . . . the lower level was lowered to 900 millibars rather than 750, and the upper level remained at about 250, we did get cyclogenesis with the two-and . . . In other words, there was nothing intrinsically wrong with the . . . with the two-layer model.

P: Yeah.

C: Um. But I think that the . . . so, the important thing was not that you needed three levels to predict cyclogenesis, but that we did predict cyclogenesis with a baroclinic model.

P: Yeah.

C: I know of no reason for suspecting that result.

P: Okay. Well, we'll . . . we'll look into that. But now, returning . . .

C: I mean, because . . . well, I mean, we know that in the operational forecasts, uh, predictions of cyclogenesis are commonplace. I mean, that's one of the easier things to do.

P: Yeah.

C: At that time, when this happened, you know, I had been very much imbued by the Norwegian school, that the prediction of cyclogenesis would be . . . was the major problem of meteorology.

P: Jule, return to the operational, the . . . the . . . the formation of operational groups. You were talking about . . .

C: Well, no, I . . . I . . . I, uh, but I think the point was this, that even as early as 1949, when Eliassen and I had developed this simple linearized approach to the . . . a kind of a one-dimensional prediction equation using observed motions at . . . at forty-five degrees latitude, five hundred millibars, and had some reasonable results, we gave a lecture at the Weather Bureau and it was then that one of the brighter, one . . . one . . . the person whom I recall very distinctly, who asked intelligent questions and who made an impression on us was Joe Smagorinsky. And it was after . . . right after that that we invited him to the Institute for Advanced Study. He was still then a student. He was then in Air Force uniform, but he was mustered out, returned to NYU to complete his doctor's degree. But even before that he came to the Institute for Advanced Study. And his doctoral dissertation arose very much from the work that we were doing there. But, let
me . . . In other words, the Weather Bureau . . . Of course, the Weather Bureau was constantly in contact with the work that was being done. But I . . . I hope I don't deceive myself when I think that . . . that the announcement of the prediction of . . . and lectures in Washington on the prediction of cyclogenesis was what determined the Weather Bureau into forming its own . . . to form its own, uh . . .

P: When . . . when did that take place?

C: . . . experimental group. In 1954. But that was aft . . . But by then, you see, they had already sent, um, George Cressman and William Shuman . . . Was it William Shuman?

P: No, Fred Shuman.

C: Fred Shuman.

P: But . . . but my question was not . . .

C: . . . to Princeton and . . . to learn the methods of NWP . . .

P: Mm hm.

C: . . . and they went back to the Bureau, although I think . . . to actually start operational predictions with the barotropic model. And an experimental group was, I think in either in fifty-four or fifty-five, set up, I think under Thompson's direction.

P: You spoke a moment ago of a lecture you gave in Washington about this cyclogenesis . . .

C: Yeah, I did. Yes . . .

P: When was that?

C: That, well, I couldn't say. I think that was probably in fifty-three, because, I think, the prediction was made in late fifty-two, or something . . . You see, that was the first prediction on the, uh, or one of . . . No, it was not the first prediction, because I think we did barotropic predictions on the Princeton computer first.

P: Did you ever look for financial support from the Weather Bureau?

C: Uh, no, we used their good offices for the securing of the ENIAC. They helped us to get the use of the ENIAC.
P: I would have imagined that von Neumann could have done that.

C: He may have, but I think that they actually helped out. Um. I know we acknowledged them in the... uh, in our joint paper on the results of the ENIAC computations.

P: Uh...

C: But, uh, well, I... I, you know, this is all vague recollection and we can check these points later on.

P: Yeah. I want to get back for a moment, uh, to the computer project, Electronic Computer Project. Was there any important work done within that project other than the construction of the computer and the meteorology group? What else was under its...

C: Well, I mentioned, uh, Pekeris, and, uh, well, Elsasser.

P: Was that in the Electronic Computer Project?

C: Yes, they were in the Electronic Computer Project.

P: I see.

C: Uh, but I would not be able to tell you whether... to what extent they actually made... I don't remember whether Pekeris used the Princeton computer or not, uh, but there was not very much contact between us. And Pekeris, of course, only came there as a brief visitor. He was in the habit of visiting the States from Israel, uh, once a year or so, and he always made his base at the Institute for Advanced Study. Uh, I can't remember... I think Elsasser was only there at the very beginning, and likewise Queney, and there was not very much contact with either of... any of us, I think, that is, between myself and either of them.

P: Of course, I suppose one should say here that, in addition to the actual physical construction of the computer, there was the logical design... the work that Herman Goldstine and others...

C: Yeah.

P: ... uh, did. But I just wondered whether there was any other major component of the Electronic Computer Project that you're aware of.

C: Not that I'm aware of.

P: Mm hm. What do you... What would you say were the main
achievements of the meteorology group at Princeton in this ten-year period, 1946 to fifty-six?

C: Well, I think there was first of all, the demonstration that even the integration of the two-dimensional vorticity equation gave reasonable results, and this was rather humorously, uh, mentioned by Richardson in his reply to my enthusiastic letter written . . . in which I notified him of our results. He thought that, uh . . . that I . . . He gave the edge . . . He said his wife gave the edge to . . . the four forecasts, two were definitely better than persistence, one was worse, and the third was . . . then he . . . and then he went on to say that he thought that this was remarkable. (laughter) As you remember. Um. So that was the first thing, and I think that attract . . . In other words, that we felt that we were now on the right track. The second thing was the prediction of cyclogenesis, I feel. The third thing, which I haven't come to, was Phillips' general circulation experiment, using a two-level model to actually produce a circulation in a teapot, so to speak. Um. Of course, there were antecedents to that. Uh. Phillips . . . there . . . we were, of course, very familiar with the empirical studies of the role of the large-scale eddies in the transport of heat and momentum, by Starr and Bjerknes. And, um, uh, there was then . . . and at that . . . and about 19, oh, I think, in the . . . in the early nineteen fifties, fifty or fifty-one, or possibly it's maybe a trifle later. Anyway, in the early nineteen fifties Phillips showed that the baroclinic wave in a two-and-a-half . . . in a two-level model, the un . . . the unstable baroclinic wave transported heat in the right direction and, uh, I showed, I think I gave a paper at an IUGG meeting, which was, um, in nineteen . . . was it fifty-one? . . . um, in which I showed that the baroclinic wave . . . no, in which I sh . . . in which I pointed out, I think, that, uh . . . that the transport of momentum occurred via horizontal transfer processes, not vertical, because that's demanded by the geostrophic approximation formalism. And, uh, that the maintenance of the westerlies, there was . . . called a paper "On the maintenance of the westerlies." The westerlies were maintained by . . . that one could show that the baroclinic wave in a jet-like . . . in a flow with a maximum velocity, the actually rein . . . that the tran . . . that, while the energy for the wave came from the potential energy of the flow, that the transport . . . that the kineti . . . that the . . . the secondary effect was to transport momentum in such a way as to maintain the jet against frictional dissipation and actually intensify it. And I think at the same time Kuo published a paper on the same subject, independently. And so in the early fifties we already anticipated that, if we were to run a . . . say, a periodic beta-plane model in a channel, that we would . . . that there was a good chance that we would get something that resembled the observed westerlies and so forth and, uh . . . But it remained for Phillips to actually put
these things together in a simple model, uh, which was carried out for a certain period of time. I think there ... it couldn't be carried out in too long, because at that time ... there were instabilities which finally destroyed the calculation, and it was Phillips himself who discovered the trouble. This nonlinear instability. Uh, but the calculations were taken far enough so that one, starting with a ... He had, uh, some kind of a horizontal heat transfer process which wasn't very realistic, but didn't really matter. Um, but the minute you established temperature gradient in a zonal flow it becomes unstable, waves form, they transport momentum in the right ... and heat in the right direction, and you eventually establish westerlies. I think that had an enormous influence on subsequent developments. So I would rank that as among the most important things that the project accomplished. And then, of course, there were a number of theoretical ideas which were advanced at that time, which I think we worked out in the form in which I presented the quasi ... the geostrophic equations in my 1948 Geofysica ... Geofysiske Publikasjoner paper. I didn't employ the geostrophic approximation altogether consistently. For example, I didn't neglect \( \frac{\beta}{f} \) in comparison with \( f \) multiplied ... multiplying the divergence term -- a few things like that -- so that the final simplification of the geostrophic ... I mean the final form of the ... I only developed the final form of the quasigeostrophic equations, in the form in which we now have them, in ... in the late fifties, and ... forgotten ... I think, uh, their publication was ... first occurred in my joint paper with Melvin Stern, uh, in the early sixties. But I think, you know, all ... all this ... a lot of this work was being done during the ... during the mid-fifties.

P: Thinking back to ...

C: At that time, Fjortoft ... uh. There was also the ... we ... we began rather early to realize the geostrophic approximation equations were not accurate enough for numerical prediction. And then, um, trying ... looking for generalizations and, um, in the case of the circular vortex, Fjortoft could show that the motions were such as to minimize the divergent energy. And I took that, um, ... and that the minimization principle led to what one could call the balance equations for symmetric flow, and to something ... and ... and ... and to basically (?) ... but the minima ... He wasn't able to extend the minimization principle to general flows, although he tried, and, uh, at that point I proposed using the balance ... or, deriving the balance equation from the simple assumption that the divergence was very small. And I think Phil Thompson did the same thing at about the same time independently ... And then, actual ... um, in the case of a two-dimensional ocean with a free surface, I actually integrated the balance equations in 1954 or something like that, showing
that they did indeed eliminate the gravity waves and gave very good results. And then immediately set out to integrate the three-dimensional equations for, I don't know, six levels, I think it was then. Um, integrate the balance equations. And that was finally completed at MIT. The first, uh . . . I had, through several stages of programmers, several . . . was begun by Jim Cooley of the Cooley-Tukey . . .

P: Mm hm.

C: . . . fast Fourier transform fame. And I've always felt that the balance equations are the natural generalization . . . not the quasi . . . not the semi-geostrophic equations.

P: I want to get back into operational numerical prediction. Uh.

C: Operational.

P: Certainly . . . Certainly, in those days, at the time of the ENIAC calculations and . . . and shortly after that, you might have had some dream, shall we say, of the future, as Richardson described so poetically in his book (?) . . . Do you feel that, uh, the present state of the art of numerical weather prediction exceeds the hopes you might have had in those days?

C: No. (laughs)

P: Can you elaborate on that?

C: Well, I think in those days we were very optimistic and we expected that . . . I remember at that time receiving reports that Norbert Wiener had regarded von Neumann and I as practically thieves. That we were trying to mislead the whole world in thinking that one could make weather predictions as a deterministic problem. And so there were almost two schools of thought. There was the Princeton school and the MIT school. Of course, I think that whole thing was exaggerated by the cohorts of Wiener and von Neumann, not by themselves, but I think in . . . in some fundamental way Wiener was probably right. That, in other words Wiener, I think, anticipated the unpredictability of the atmosphere that Lorenz later formulated rigorously. Uh, but . . .

P: Well, do you think that the first (?) . . .

C: Uh. And, uh, I . . . At that time, my own thinking on the subject was very vague, although I did have a notion of unpredictability. Uh . . .

P: Well, do you think you hoped for too much, or that there was
too little delivered?

C: I would say neither. I think that the development was a natural one. I think that one . . . that it was first necessary to explore the nondissipative equations and the no . . . and then gradually to introduce the energy sources and sinks in a rational way. And that this was done, I think, uh, one of the first problems that I suggested to Smag -- Joe Smag -- was to look at the precipitation problem, which he did. He was really the pioneer in that field, I think. Um. And that's, as we know now, far from having been solved, particularly convective precipitation, which is important even in the short-range problem, I don't . . . We never anticipated that radiation would be a major problem. I think that the calculus of radiation by then had already been essentially solved. We were probably overly optimistic, but, um, I still feel that boundary-layer turbulence and . . . and . . . and turbulent moist convection are still probably the two major physical problems. Uh, but I . . . but I . . . my feeling I shared with von Neumann or I . . . I agreed with von Neumann that the computer would be used as an experimental device to attain a better understanding and to point the direction of future research in physics.

P: There certainly have been improvements over the years in numerical weather prediction and one might ascribe them to a number of factors, input data, um, improvements in numerical modeling and improvements in the physical modeling, just to name three of the most important of them. Which of these things do you think actually is, or has been, most responsible for the improvements that we've seen?

C: I think you have to make a sharp distinction between the short-range forecast problem and the climate forecast (?).

P: Mm.

C: With climate, the physics is by all means the most important. with the short-range problem, I think the improvements are simply . . . have been mainly due to the . . . to . . . to the reduction of truncation error. I mean, I think we all underestimated the importance of truncation error.

P: What about input data?

C: I think that too, but I . . . I . . . I think that for localized or for regional forecasting, it's still truncation error.

P: Really. That's interesting.

C: But, I think, for local forecasting and for longer-range
forecasting, of course, the input (?) eventually destroys you. And we were aware of that. I mean, we already had an idea of what the influence domain was.

P: Jule, we have about two minutes on this side of the tape, and here's a question I'll put to you. Do you think that numerical weather prediction has gone in . . . that is, in a dynamic or deterministic sense, gone about as far as it can? (tape goes off, then back on) So the question is, has numerical weather prediction . . . The tape is on now . . . gone about as far as it can, and if it hasn't, is there anything left to do?

C P: (laughs) Well, I think . . . That's a rhetorical question, isn't it, George, because certainly I do not think that numerical weather prediction has gone as far as it can. I think that both the short- and the long-range and the climatic problems can be vastly . . . that much remains to be done and . . . on all three problems, and that, uh . . .

P: How would you go about doing it?

C: My God. Well, I think . . . I think that truncation error continues to bedevil the short-range problem. Uh, but boundary layer and, uh . . . and, uh, precipi . . . and . . . and moist . . . moist . . . and precipitation effects are also important, or are very important. Uh, that we still do not deal with topography in an adequate manner. That here you have a problem of multiple scales and the topography doesn't . . . doesn't oblige by being on the scale of the large-scale motions, nor do the large-scale motions oblige by remaining of large scale, and that one has zones of strong gradient which have to be resolved by one means or another, and we're a long way from having achieved that kind of solution. And then, of course, there remains, even in the short-range problem, I think, the . . . the physical factors, which are still not sufficiently well understood. The matter of boundary layer processes, matter of incorporation (?), and mainly precipitation pro . . . processes. I don't think that radiation plays a primary role in the short-range problem. Where it comes to the medium-range problem, I think, it has recently been established, I think, to my satisfaction, at any rate, that the failure to adequately provide for the small-scale motions . . . ultimately infects the large-scale motions. Um, this . . . these have been studies of statistical equilibrium in two . . . in two dimensions, showing . . . and there is always, since the advection time is much less than the dissipation time in the atmosphere, uh, there is always a tendency to approach statistical equilibrium. And what these people, Frederiksen for example, have shown, is that, if you get the short scales wrong, then in the equilibrium you'll get the lar . . . the large scales wrong too, and therefore truncation error affects as one . . . rather surprisingly, affects the large
Charney interview		Tape 4, side 2

scales very much, and the large scales, of course, are also affected by topography, since they're generated to a large extent by topography.

P: Is this, uh . . .

C: I think these problems still remain to be solved.

P: Is this effect that you've just described another one of those traceable to the two-dimensionality . . . quasi-two-dimensionality of the motion?

C: Yes, I think, basically. I think this goes back, uh . . . as far back as, certainly, to Fjortoft, but even before that, to Onsager. In 1949 he worked on statistical equilibrium. No, I think there's an . . . I think the emphasis now seems to be on climate, but I would hate to see, uh, the short-range and middle-range, the . . . unnecessarily (?) . . . extended range problems short-circuited, because I . . . fundamentally, I don't think that we'll understand climate until . . . until we can understand the elements, short-range elements, which make up the turb . . . the turbulent elements of climate. I don't see a . . . although, I think it's proper to . . . to emphasize the physics to a much greater extent. I mean, such things as cloudiness become decisive in climate, whereas they are not that important in the radiation field, and become decisively important in climate, whereas they are not so important in the short-range problem. So, uh . . . But nevertheless, I don't see any royal road to an understanding or a . . . or a simulation of climate, or of climate variability . . . equally well understanding of the short-range problem.

P: Do you think that enough effort is now being concentrated on the short-range problem?

C: Well, I think one could always devote more effort. Effort by itself isn't enough. I think inspiration is also important. (laughter)

P: On that interesting note we'll stop.

END OF TAPE 4, SIDE 2
TAPE 5, SIDE 1

P: We continue the interview with Jule G. Charney. This is the fifth session, being conducted on Thursday, August 28, 1980, in Boston, at the Holiday Inn. Okay, here we go.

C: George, before we go to the post-Princeton period, there are one or two things that I feel should be mentioned. Uh. I've already referred to the fact that, at the very inception of the meteorological group, uh, or even before, um, von Neumann had discussed with, uh, Panofsky and Haurwitz the problem of... of objective analysis, and the approach that was adopted then was to fit a high-order polynomial to the data.

P: Um hm.

C: And, um, later on, after I had arrived, we began, uh, considering that problem, and it occurred to me that, um, I could see no reason for high-order polynomial fitting since, um, uh, the data... that... that, uh, there was nothing about meteorological data that suggested, um, high-order differentiability (laughs), and that therefore there was no point in... in, uh... And that the, uh... that the motion at a given point -- let's say over the United States -- is not going to be influenced by the motion over China, or, at any rate, that... that's not a very good way of putting it, but, uh, the point is that... that... that objective analysis were a fairly local problem... a fairly local problem and that one way of going about it was simply to fit a second-order polynomial to redundant data within a certain area... within a... say a circular or octagonal or, that is, hexagonal area. Not hexagonal. Uh, what's the general term for, uh...

P: Polygonal.

C: ... Polygonal area. Just making sure that the, uh, that the problem was overdetermined and then simply to fit the data by least squares.

P: Uh hm.

C: And this, uh, suggestion was actually taken up by Gilchrist, uh, a student of Eady's who had come to the project, and, uh, Julian Bigelow. And I think they published a little paper on that. And it seems to me that this may have been, uh, well, by no means obvious, but, uh, that this ultimately was the approach which was eventually adopted by most weather services with... with, uh, information-theoretic overtones, particularly in Gandin's work. Um. That's one thing I feel that
ought to be mentioned. The other is, as we've been discussing off the record, uh, . . . We went on after the numerical forecast calculations to make further studies of the two-dimensional flow in more theoretical contexts, and one of the studies was to . . . to simply -- experimentally -- look at finite-amplitude instabilities of a jet-like flow . . .

P: Um hm.

C: . . . and we made some calculations in which the flow satisfied the Rayleigh criterion for instability and, indeed, the instabilities developed, and resulted in a . . . perhaps the first empirical, uh, numerical calculation of the vacillation phenomenon, with a steadily, uh, . . . In this . . . In this problem I think we had a bit of viscosity and so that the am . . . the oscillations were damped. And, uh . . .

P: I don't recall that I ever knew about them.

C: Yeah, well, uh, that was published in one of my papers. I've forgotten. I mean, uh, the . . . the bare results.

P: Mm.

C: Another thing that was done was to take the numerical calculations for . . . from the prediction problem and to simply look at the direction in which momentum was transferred, and indeed the barotropic motions always transfer momentum, um . . . um, that is, the barot . . . uh, to . . . to actually increase the kinetic energy of the zonal flow. Uh, which is to say that, in some sense, we're dealing with barotropic instabilities. Now wait a minute. Not doing (?) . . . No, to subtract kinetic energy from the zonal flow, but to in . . . concentrate it into jet-like, uh, motions. That's in the IUGG publication. The other I've forgotten where it is. Anyway, um . . .

P: Didn't you do something in the Rossby volume?

C: But in connection . . . Uh. No, that wasn't in the Rossby volume. But, um (coughs) (chewing?) Um, the other thing that . . . that we referred to in our private conversation was that, um, that when one looked at the vorticity field in these theoretic . . . in these ideal of . . . calculations of jet instability, uh, that while the stream field remained quite regular, in the calculations, the vorticity field generated . . . and there was a . . . clearly a steady migration of energy towards smaller scales, that is, toward vortical energy or amp . . . what we would now call enstrophy. And, um, I conceived somehow that this . . . this may have . . . that this was something that was . . . that we should try to eliminate, this what I call "noodling" phenomenon. And, um, being familiar with
the work of von Neumann and Richtmyer on the calculation of one-dimensional shocks where they found, when the shock became very rapid that ghost oscillations were generated. And to eliminate that phenomenon they introduced a viscosity proportional to the divergence of the flow, that is to say a nonlinear viscosity. And it seemed to me that the corresponding... uh, that what one was dealing with was instabilities on small scales and the way to overcome this instability was to eliminate the deformation on small scales, which becomes larger and larger because you get a more streakiness in the flow and, therefore, to introduce, um... um, a nonlinear viscosity proportional, uh, to the square of the components of the deformation tensor, basically. Or to the... yeah, the square root of the square. Um, I tried that out and, um, it did affect the noodling to some extent, but not greatly. But, of course, that depended on the coefficients, which were then sort of empirically determined. Later on, Joe Smagorinsky took up... Um, this particular technique was incorporated in a version that I wrote, um, for the integration... that is, a precis that I constructed for the integration of the two... of the two-level primitive equations, which was then to be undertaken by a group under Smagorinsky's direction at Washington. This came about because von Neumann accepted a... an appointment to the Atomic Energy Commission in Washington, and when he moved down there he was... he was very anxious to have a group functioning in his vicinity. And, I think, it was through his offices that a... that an experimental group was established under Smagorinsky's direction by the Weather Bureau. Although before that... I think but by that time there was already an operational numerical weather prediction group running.

P: What year was this you're talking about?

C: Well, I think this must have been in fifty-five... fifty-four, fifty-five. Fifty-five, I think.

P: The operational group was established in July 1954.

C: (?) Well, I think that then this was fifty-five or fifty-six, when von Neumann was no longer at the Institute for Advanced Study. And, uh... But I found so... but I... but then I tried a linear viscosity and I found that the results were just about as good and... and, therefore, ...

P: (laughs)

C: ... uh, gave up the idea of using the nonlinear viscosity. I think... But my own experience was too, um, meagre to, uh... from which to draw any, uh, very definite conclusions.
**P:** How do you view the matter today?

**C:** I think, in the end, that, um, other techniques are probably preferable, filtering techniques. Because the one ... one problem with these viscosities is that in the long run they can give you unrealistic transfe ... transports of momentum. Um. Well, I think these ... these, uh ... the ... I will refer to oceanographic work that I did in ... during the Princeton period but in a la ... in a ... later on, in another context.

**P:** All right.

**C:** Well, now, to start in with a ...

**P:** Cambridge.

**C:** ... post ... with Cambridge, Mass., and my work at MIT. What happened then was ... 

**P:** Wait a second. Let me lead you into that with a couple of questions. Uh, perhaps this is the first thing you were going to discuss anyway. But, first ... the first two questions I'll ... I'll group together. How did your choice of MIT come about? The first part. And, did you find -- and surely you must have sensed the change very quickly -- did you find MIT to be a stimulating change of environment -- after all?

**C:** I would say it brought me back into a ... a meteorological environment. Um. Many ... A number of physicists who had come to the Institute for Advanced Study, both Yang and Lee, for example, discovered -- and I think this may have been true of Strömgren, but I can't ... can't say at first hand -- that, uh ... they felt deprived by being out of an experimental context. And that was one of my major motivations for leaving, that I felt that we were too isolated, particularly when von Neumann himself had decided that Princeton really wasn't for him, and had begun actually, um, to, uh, consider transferring his activities to another institution. But in 1954, I think, already he discovered that he had cancer, and that, uh ... he continued to, uh ... continued conversations with other institutions, but it ... it rather soon became apparent that he would probably never be able to go. Among the institutions that ... that he was considering in those days was MIT. Another, I think ... well, of course, he could have take ... he would have had his choice. I think Harvard was interested in him. UCLA was interested in him. UCS ... well, UCSD didn't exist at that point, but there was Scripps, and there were plans to, uh, in which I think he participated to establish a new branch of the University of California.

**P:** Was Carl Eckart already there?
C: I believe Carl Eckart was already there. Um. I discussed some of this, uh . . . um . . . I think my . . . one of my principal motivations in going to MIT was, after an interview with Stratton . . . I felt that this was an institution which would be unlike the Institute for Advanced Study as a whole, which was . . . which was made for acceptance of the kind of work I was doing. And a great deal of understanding and sympathy, uh, that is, Stratton demonstrated much more of an understanding for my work than almost any other university administrator that I . . . There were a number of other universities who appeared to be interested in my coming, but I chose MIT partly for that reason, and another reason was that, um, among the visitors who had come to the Institute for Advan . . . whom I had actually invited to the Institute for Advanced Study were Starr and Lorenz, and I was enormously impressed by Lorenz and, in fact, at the time I left Princeton, I think it . . . I can say that Lorenz too was beginning to cast about for a possible, uh . . . for a possible move. Because I don't know that he considered his position in Pr . . . in . . . at MIT permanent or not. I think at that stage he was not a faculty member. He became . . . whe . . . when I first came to MI . . . or first had interviews with MIT, he was only an assoc . . . an assistant professor. And when Stratton asked me what could be done to improve the department, I felt that the first thing that they should do was to promote Starr and Lorenz. (laughs) And told him so. Um. And Starr, of course, um . . . And I felt that that was . . . that not only the presence of Lorenz and Starr in the Meteorology Department, but people like C. C. Lin in Applied Mathematics . . . I found them to be very attractive. I knew Lin and thought very highly of him. Also the contiguity of, uh . . . of . . . of . . . of the Woods Hole Oceanographic Institution, um, was a factor in my decision, because I was interested then . . . then in oceanography too. And, um, I think that was my basic reason. Boston itself didn't uh, at the time anyway, . . . was a . . . sort of an unknown quantity and . . . or that is, what I knew about it I didn't particularly like. (laughs) There had been disturbances and riots, anti-semitism, under Father Coughlin -- things like that. But, on the other hand, I knew it as a cultural center. So I actually wrote a letter accepting an appointment after an interview with Houghton who came to Princeton . . . accepting an appointment to MIT. And, at that point, um . . . This was in 1956 . . . I mentioned to you already, uh, Oppenheimer's attempt to keep me there . . . to keep me at the Institute, but, uh, I didn't, I think perhaps wrongly feel that that was the place for me. I felt it was an ivory-towerish place and, for the same reason that the other physicists, uh, ultimately left, because they felt a need to be in more contact with experimental work, I too felt the need to be in more contact with observational work. And there was, of course, observa . . . considerable observational work going on at MIT.
P: Is it true that another significant difference between, uh, the Institute and MIT was the fact that you began teaching again? Do you like to teach?

C: I didn't know for sure at that point, uh, no, I wouldn't say that that was my primary motivation. I was . . . My whole background had been much more research oriented. I was not . . . I di . . . I never considered myself to be a natural-born teacher, or my s . . . primary satisfaction would be derived from seeing young men and women grow under my tutelage.

P: But do you feel that teaching is a useful adjunct to research?

C: Yes I do, and, uh, I think it . . . it, uh . . . But what I discovered after moving to MIT was that I . . . what I did enjoy mostly was working more individually with graduate students on thesis research and research problems. Uh, because I find that my own work is greatly enhanced and, uh, becomes much more enjoyable when I can discuss it with other people and . . . and, uh, I enjoy doing cooperative work and I consider that working with students was cooperative work.

P: You have, in fact, worked with quite a good many postdocs who, uh . . . many of whom were really not trained in meteorology.

C: That's right. And I've enjoyed that very much.

P: What is your general . . . Do you have a general philosophy about that kind of activity?

C: Yes, I mean I . . . I, uh . . . I am attached to my field and I feel that bringing bright young people into the field is a useful occupation. And, um, more than that, they always bring a new point of view, and so I find that association with people who are even outside the field is stimulating. And I've discovered that, uh . . . I've had quite a number of postdoctoral students . . . Those who already come with a background in fluid mechanics, a strong background in fluid mechanics, are the most adaptable. People with fluid dynamics or, say, mechanical engineering backgrounds with a strong component of fluid dynamics have always done very well and pick up the subject very rapidly. I've had some theoretical . . . students from theoretical physics, particle physics, solid state physics. I find that their mode of thinking is more orthogonal and that . . . and that they encounter the same kind of difficulties that I as a mathematician encountered when I first entered the field of meteorology and it is uh . . . that meteorology is a field science and one has to be able to suspend rigor to a degree and . . . and leave oneself open and not demand rigorous, um,
explanations of phenomena. You have to be satisfied with an
... with explanations that appeal to one's intuition, and
that's much more difficult for a pure theoretician.

P: Jule, some of the important meteorological subjects you
tackled -- let's ... let's for a moment exclude numerical
weather prediction -- in this period that we're speaking of now
is ... Let me just list ... I picked ... I picked five out
of the publication list that you provided me: Vertical
propagation of planetary waves, stability of jet streams,
generation of hurricanes, geostrophic turbulence, and most
recently the climate dynamics of deserts. Which of these, uh
... Well, this is ... is a two-part question. Which of them
is the most appealing to you, or was the most appealing to you,
intellectually, most satisfying, and which do you think is the
most important?

C: You should add my most recent work with ...

P: Mm.

C: ... which has just beginning to appear now, on blocking.

P: Yeah.

C: Because I would number this among the other ... I think
... Otherwise, you've covered the crowd pretty well. Um. I
think, uh ... well, I would not take ... I don't think I
could single out any one of these as the most appealing. I think
... But a number of them are related to, uh, a consolidation
and appreciation of the geostrophic formalism. Um. I think, for
example, the work on baroclinic instab ... the ... what did
you meant ... the stability of internal baroclinic jets with
Stern, vertical propagation of planetary waves, geostrophic
turbulence. I mean, all these became possible, uh, with ...
and even some of the ... to a degree, even some of the work
that I did in oceanography, which I think (don't think ?) you
mentioned. Um. And even my current work on the blocking
phenomenon, multiple equilibria. These all became possible with
-- sort of this consolidation of the geostrophic approximation --
in the final form which I published ... well, which really came
out in the Proceedings of the Second NWP Conference in Tokyo, and
also in the publication with Stern on the internal baroclinic
jet. With respect to the stability problem, the formulation
... the ... that is basically the motion is governed by
conservation of a three-dimensional elliptic operator with
respect to the two- ... two-dimensional displacements, uh
... And in linearized form the ... you can begin to see very
clearly the relationship between the stability problem for
baroclinic flow and the ... and, let's say, the Orr-Sommerfeld
problem or, in general, the problem for stability of inviscid
parallel flow. And Kuo is the first as I . . . I think, to . . . to apply . . . to see the analogy with Rayleigh's theorem in . . . in the case of the classical baroclinic stability problem (?). And Stern and I generalized this then later to an arbitrary zonal flow and I was very quickly able to employ . . . to give an explanation for the instability of internal jets in terms . . . which was analogous to von Kármán's explanation of the Rayleigh criterion, in terms of vortical forces, um, and to show the relationship, in general, between the baroclinic stability problem and, uh, the problem of two-d . . . parallel shear flow, where the, uh . . . where stability is brought about by the homogeneity of the boundary conditions when you have fixed surfaces, but if you allow free surfaces you can have Reynolds stresses, you can have . . . giving rise . . . and . . . and that baroclinic instability can be explained . . . that the importance of the surface boundary condition in baroclinic instability and subsequently the notion of surface potential vorticity, which was then first formalized by Bretherton, although I had certainly appreciated this work, in fact had lectured on it as early as 1958 at . . . at our joint Woods Hole-MIT-Harvard seminar. Then I think it was Woods Hole-MIT. And my work on geostrophic turbulence, um, was . . . was a direct . . . The direct, the immediate influence was Fjortoft's 1953 paper on two-dimensional flow. And I could see . . . and I . . . what I did first was to construct, um, an n . . . um, n minus one over n dimensional model, that is to say, a numerical, an n-level model. Now the characteristic of an n-level model is that implicitly, um, the boundary condition is taken into account, the vanishing of the vertical velocity on a horizontal boundary, and so that the formula . . . so that one can show that the formulation is really isomorphic to the motion of a multi-layer model in which the interfaces do not intersect the ground and do not wiggle too much. Um, the consequence of that is that the multi-layer model is, uh, very analogous, that is, you don't have a problem with temperature gradients at the ground, so the multi . . . and I never . . . and so beginning with a two-level and then going on up, I realized that the Fjortoft theorem applied to that as well.

P: Mm hm.

C: And, uh, my . . . and that was first mentioned, well, in seminars even in the late fifties at MIT, Woods Hole. But it was only published in sixty-seven as a report of a conference at Traveler's Research Center, and then finally in 1971, as a report on geostrophic turbulence. Of course, the notion of geostrophic turbulence . . . that was influenced by Kraichnan's work on the idea of an enstrophy cascade. But I was aware much earlier than that that a . . . that a cascade towards high wave numbers . . . of an energy cascade towards high wave numbers was precluded, and that if anything energy had to migrate toward low wave numbers. But, you see, all this came from the geostrophic formalism.
Without it, one could not have . . . And, in fact, the appearance of fronts vitiates these results, and it is not known to what extent still. I mean, one speaks of two-dimensional turbulence or quasi-geostrophic turbulence, but I think that one always has to bear in mind that the results aren't in yet, because the real motions in real two- . . . even in real two-layer flows . . . there's nothing to prevent the interface from intersecting the ground. It's only in quasi-geostrophic flows that they don't, that it doesn't.

P: Incidentally, we've been . . .

C: And I think when the interface intersects the ground, all bets are off. Or, to a degree. One doesn't know for sure.

P: Uh, just a side comment in referring to Fjortoft's work, which certainly is crucial in this . . . in this area: Isn't it so, that historical accuracy obliges us to mention the very brief little note by T. D. Lee.

C: Yes. And I do mention that in my turbulence paper.

P: When . . . when was that published?

C: That . . . that was 1951, I think, in Physics Today. Uh. It's interesting. I think the problem . . . I think that Lee at that time either had . . . had some association with Heisenberg. That's a long story in itself.


P: Hm. Jule, I'm going to have to stop the tape . . . interrupt you to stop the tape.

(tape goes off, then on)

C: I think, in other words, Lee appreciated that a two-dimensional . . . and . . . and, anyway, Heisenberg . . . Heisenberg's thesis was on the Orr-Sommerfeld problem; and it's very interesting, two great . . . two of the great quantum . . . founders of the quantum mechanics, uh, began their work in hydrodynamics. And Heisenberg has written very percept . . . and Heisenberg, of course, went on to deal with the problem of turbulence in general. Homogeneous isotropic turbulence. And, um . . . um, Shrödinger's thesis was also on anamolous propagation of sound.

P: Really? I didn't know that.
C: Um. But to come back, um, I may be wrong, but I think that Lee was in . . . Lee was either directly influenced by Heisenberg or by Heisenberg's work. And it occurred to him, um, somehow to look at the two-dimensional problem and he quickly came to the conclusion that you could not get the energy cascade. But this was already apparent, in a way . . . not apparent, but adumbrated, shall we say, by, um, Onsager's paper, who was also interested in . . . in turbulence, uh, and looked at the statistical mechanics of a collection of line vortices in two-dimensional flow.

P: Um hm, yes.

C: And came to the conclusion that there would be a . . . an infrared catastrophe, that is, the energy would move steadily toward . . . But, of course, he didn't take all the constraints into account. Otherwise, he would have gotten a . . . I think . . . In other words, he, uh, just as Rayleigh found in black body radiation an ultra-violet catastrophe, I think Onsager found an infra . . . an infra-red catastrophe. But, at any rate, he demonstrated the tendency for . . . But he didn't . . . As I say, he didn't take all the boundary . . . all the . . . the invariance into account. Um. He certainly predicted the tendency for . . . for vortices to form in clusters of like sign. Um. Well, I think I've said enough about that.

P: Yes.

C: Um. Perhaps we'll come back to geostrophic turbulence later on, but . . .

P: Maybe this would be . . .

C: Well . . . Go ahead.

P: Would this be a good point to ask you, uh, about your oceanographic work?

C: Uh, yes, because . . . I've forgotten exactly . . . My . . . my two principal . . .

(tape goes off, then on)

P: Yeah.

C: My two principal mentors in oceanography were Sverdrup and Stommel. Uh, Sverdrup lectured on oceanography during the wartime training courses at UCLA. He would come up from Scripps. And I had read his book. He had a condensed version of "The Oceans" more on the dynamical side, uh, which we studied. And I came to know Sverdrup personally. And then was rather influenced
by Munk's, uh, paper on . . . And, of course, I knew Sverdrup's . . . the Sverdrup transport calculations in the oceans, and then read Munk's paper on the wind-driven circulation of an ocean basin. Um. And have . . . had an interest in oceanography, and then got to know Stommel after I had moved . . . while I was still in Princeton. I've forgotten . . . I think I must have paid some early visits to the oceanographic institute -- Woods Hole -- and knew Stommel. And in 1954 I was invited as a Woods Hole Associates Lecturer, where I spent a . . . something of the order of several weeks to a month, I think, at Woods Hole. And then turned my attention to oceanographic motions, particularly. And, um, that work was published in the Sears journal. But among the problems . . . Then I was . . . I . . . I read a good deal of Rossby's work on geostrophic adjustment and considered . . . and at that . . . and began thinking very much about how motions were generated by wind, and immediately came to the conclusion that . . . that given the space and time scales of the wind systems that you would only generate barotropic motions, to begin with. And this followed very much from Rossby's work. Um. That is to say, that independently of the stratification. Um. And then considered the way in which a wind-driven circ . . . that is, the way in which the thermohaline circulation might be spun-up. I think at that point Stommel had already suggested to me that . . . that one way of looking at the ocean circulation . . . I think he had already used this idea in his work, his own work, was . . . that is, his work beyond his 1948 paper on the . . . on the western intensification of the ocean circulation . . . was to break up the ocean, that . . . that . . . that the fifteen-degree isotherm seems to thread through the main thermocline and that a two-layer model in which the . . . the i . . . the interface was a fifteen-degree isotherm would be a good model to work with. And I remember making some very crude calculations of the spin-up time of the ocean and, uh . . . w . . . that is, given the stratification already, and was surprised to find that it was so long. I got a number of about eight to ten years. Whereas the barotropic spin-up time was the order of days. Um. And, uh, on the other hand, I never could really accept . . . I mean, many people thought that the motions of th . . . in the atmosphere and the oceans were quasi-geostrophic because of the adjustment process, that if you start an unbalanced flow, then by Rossby's process you would eventually end up with a quasi-geostrophic flow. Well, of course, to begin with, this is only true in an infinite ocean, but it didn't seem to me that this was the prob . . . that the real point, the real reason why motions are quasi-geostrophic is that the . . . that the driving forces are large scale in both space and time and therefore the energy flows into geostrophic motions to begin with, and that you go through, um, it's kind of a . . . like . . . like adiabatic . . . the adiabatic transformation in quantum mechanics. That is, you go through a set of states of equilibrium in which very little gravity wave energy is ever
generated. It isn't a question of ... I think I understand that problem better now and there's a little bit of both ... and I think it's interesting that a ... that a ... Ron Errico, a student of Lorenz's, was able to simplify the primitive equations in such a way that he could apply statistical mechanical arguments to show that if you took an arbitrary inviscid, conservative flow with sufficient energy that you would eventually get equipartition between geostrophic and ... and gravity energy, gravity-wave energy ... 

P: Oh?

C: Um, in other words, energy is ... and when I was in ... in sabbatical at Cambridge University I began work, but didn't finish it, on Lighthill's idea of the hydrodynamic ... applying Lighthill's idea of the hydrodynamic generation of noise to the generation of gravity inertial motions by geostrophic motions. Of course, the problem differs in a ... one essential way, because in ... in Lighthill's work, he considers a compact region ... isolated region, in which you have turbulence radiating acoustic energy, whereas in the case of gravity wave motions and geostrophic motions you have these two scorpions in the bottle. (laughs) They can't get away from each other. And so you do naturally tend to continue ... In other words, if you don't ... if you don't kill off the gravity wave motions you will eventually generate, um, a lot of gravity wave energy, but if it happens slowly but you have an infinite amount of time in an equilibrium situation.

P: I'm curious about this equipartition you referred to. Is that such that the total energy is one third geostrophic, one third gravitational potential, one third gravitational kinetic?

C: No, I think it turns out, if I recall correctly, that the energy is equally divided between ... As he defines it, you need to make a few approximations to do this, I think, like making a const ... invariant stability, or something like that. You end up with half the energy in the geostrophic motions, half the energy in gravity motions. Um.

(tape goes off, then on)

C: Hey. (?)

P: Oh, I know.

C: (?)

P: Oh yes, well, this is, sure, we have plenty of time.

C: Well, okay, what shall I do? (?)
P: Continue. It’s on.

C: Okay, um. I applied the, uh . . . in order to under . . . then I set myself, to try to understand how the wind actually generates motions, and then, of course, I used the idea that . . . that, uh . . . that Eliassen and I had published on the way in which the Ekman layer works . . . generates . . . dissipates motions in the atmosphere . . . to the way in which wind produces an Ekman layer . . . produces Ekman pumping, where the surface, what coul . . . one can show from dimensional arguments, that the surface can be considered rigid, and therefore the Ekman pumping simply results in a . . . in a vertical motion at the bottom of the Ekman layer. And that this vertical motion in a barotropic ocean simply spins up or spins down, the barotropic motion. Otherwise, it simply becomes a boundary condition on the baroclinic motion. And I, to my knowledge, this is the first time that this . . . had been explicitly stated, although . . . I mean, up until this time I'm . . . people spoke of body forces. For example, in Stommel's work he speaks of body forces. Um. And it was at this time that I began to . . . I think this time or even earlier, began discussions with Stommel about the actual gen . . . the actual generation of the Gulf Stream. The western boundary current. I couldn't accept Munk's analysis, in which he, undoubtedly under Sverdrup's influence, used a hypothetical horizontal, um . . . horizontal viscosity. Because to my knowledge this required a, uh, Austausch coefficient . . .

END OF TAPE 5, SIDE 1
P: Okay.

C: Okay. I couldn't accept Munk's use of the horizontal Austausch coefficient, nor could I accept Stommel's idea that the dissipation occurred via bottom friction, because the motion takes place mainly, I (?) assumed, in the upper layers, the upper part of the ocean. Um. Rather, it... I was, um... having been very aware of the Sverdrup transport relationship, and accepting this as fairly well established, um, it immediately led to the consequence that there would be a net transport into the western boundary, or into the... into the western edge of the ocean and... and that this would neces... and that therefore geostrophic dynamics could not be continued all the way to the boundary, and that what was more likely was that inertial processes took over and that... of course, there would still be horizontal and vertical friction at the continental shelf itself, but, uh, that this would occur in a very narrow boundary region not comparable in size to the Gulf Stream. Um. I knew of Stommel's nineteen... privately published 1954 paper, in which he showed that you would get a boundary current, not as a solution for the ocean circulation, but in... if you merely assumed constant potential vorticity, I think in a two-layer model. But this could not be a solution for the generation problem unless one assumed that, for some reason, the potential vorticity was constant, and that would have been an unjustifiable ad hoc assumption. The way I solved the problem... uh, solved my version of the Gulf Stream problem was to assume that you knew, uh, from the Sverdrup relationship... that you knew the transport of the west... into the western boundary, and then go over to non-geostrophic dynamics. And, um, given the fact that the boun... that the... then making use of boundary-layer ideas, it turned out that one could easily establish a solution, and this... and there was in the Sverdrup relation... In the Sverdrup relationship, it was that at the latitude of zero curl there would be zero transport, and... and then one could show that the boundary layer couldn't continue as a... simply as a boundary layer beyond that point, and therefore this created a kind of a dynamic requirement (?) for Gulf Stream separation, which I also concluded. I extended the analysis to a continuously stratified ocean where it turned out that the logical coordinate to use was density as vertical coordinate, but I never published it. I think subsequently, independently, Robinson employed that method, that device, for a continuous ocean model. Um. I mention this as my, uh, one... Also, I... Let... Let (?)... At the time that these discov... the time Stommel was visiting the Institute for Advanced Study, um, Veronis had... I had invited Veronis to come as an assistant with some of the numerical problems and the work that I had done and presented as the... as the Woods Hole
lect . . . Associates lecturer, where I considered the manner in which wind-driven motions would be generated in a two-layer ocean. It turned out that one . . . that there was a separability for not (?) . . . linearized motions between baroclinic flow and barotropic flow. But in my work I thoughtlessly neglected to consider the beta effect and therefore the baroclinic motions that I . . . (?) both baroclinic and barotropic motions that I studied were totally unrealistic. Um, subsequently, Stommel and Veronis took up the same problem of gen . . . gen . . . the general problem of generation of motions, or the general problem . . . I mean, they simply looked at the dispersion relationship for motions, and were able to classify oceanic motions into baroclinic, barotropic, showing that the baroclinic motions were very very slow indeed, and that basically the time of the baroclinic . . . say, a wind-generated baroclinic Rossby wave to (?) . . . to cross the ocean was really the spin-up time of the ocean. I don't think they showed that. That was subsequently done by Gill and others. And you get a number of the order of ten years. But I think this was a . . . an important paper of Stommel's and Veronis's, that they could classify the various . . . (?) kinds of motions on a . . . and with reference . . . actually, I think, one of the problems that I suggested to Veronis when he first came to Princeton was, uh, to consider the kinds of motions generated if you tweaked an ocean with different kinds of tweaks, that is, different kinds of vorticity curls. At any rate, I think this was an exciting and interesting period of . . . in my work.

P: When did all this take place?

C: This took place still in Princeton. But you asked about the oceanographic work. I didn't do very much, uh, as a follow-up. For one reason, when Stommel joined MIT, I felt that I should leave the ocean to him (?). (laughs) Uh. But, uh, I was at a conference in Princeton . . . in La Jolla in 1958, and at this conference the sad news that Cromwell had been killed in an airplane accident on his way back from an expedition to study the equatorial undercurrent, . . . which then the Scripps people immediately began calling the Cromwell current. I think the . . . the . . . Montgomery was the coiner of "equatorial undercurrent" as a more general title, but . . . and it was my first . . . it was my first knowledge of the existence of such a current in the oceans, and when I came back to MIT and began discussing it with Veronis and Stommel and Malkus, I made the statement that it seemed to me that something which was so much confined to the equator and so regular must have a simple explanation. And then it was proposed, I don't remember by exact . . . by whom, that there should be a competition in which . . . that we should all make an attempt to find an explanation for the undercurrent and in two weeks' time we would present our results. Ah. I came out with an explanation whereby I . . . in analogy
with the Gulf Stream, I looked upon it as a breakdown of the geostrophic approximation at the . . . at the equator, and then got some advice from George Carrier about what to do . . . I . . . I derived a set of equations in which what was given in the . . . the way that the Sverdrup transport is given in the case of the Gulf Stream, the pressure gradient along the equator would be given by the external flow, outside of the equatorial boundary layer, by the quasi-geostrophic motions. And given that pressure gradient, the problem was to find the motion, three-dimensionally. Then there was a question of the lower boundary condition and I assumed that there was to be some point at which, uh, . . . I can't remember . . . I think that . . . what my reasoning was exactly there, but, either a no-slip condition or a . . . viscosity becomes essential . . . you can't ignore viscosity . . . either a no-slip condition or a . . . rigid-boundary no-slip condition or a slippery rigid boundary, I've forgotten which. Anyway, . . . But Carrier's method turned out to be inadequate and my calculations did not give as strong a current, but it . . . I think, in the end the model turned out to probably be mainly correct. Stommel simply carried the Ekman solution to the equator, showing that you . . . of course, you got a motion that extended all the way to the bottom, because the Ekman layer becomes infinite. But then, of course, you have a bottom boundary layer too. And, uh, I think, Veronis made assumptions which were, in the end, not justifiable. At any rate, these three papers were eventually published in Deep-Sea Research. But we had . . . Stommel wrote out a kind of a circus-like announcement of the . . . this competition, at one of our joint seminars, which I established between Woods Hole and MIT the minute I arrived at MIT. This is in fifty-seven. Um. And that seminar has gone on till almost this day, although I think the most interesting sessions were the early ones, which were attended by people like Stommel, Rossby, Malkus. At any rate . . . And then Malkus gave account (?) and said "Each . . ." . . . So . . . "Oyez, oyez, three separate theories of the equatorial undercurrent, and each in only fifteen minutes, and three counter-arguments, or whatever, by Willem Malkus, each in only five minutes," or something like that. (laughter) Anyway, that is . . . that . . . that sort of brought a . . . that . . . that created my interest in equatorial phenomena, and I found them fascinating in the oceans. Subsequently, Spiegel and I wrote another little paper about what happens when the wind . . . when you have cross-wind currents and such things, and I think that there was an error in that paper and I don't really trust it very much. Um.

**P:** Who won the prize?

**C:** Oh, I . . . nobody. (laughs) Uh, I think in the same issue of Deep-Sea Research there was a paper by . . . there was a paper by Robinson on a sort of a genuine baroclinic . . . then a
thermohaline theory of the equatorial undercurrent. I've forgotten whether he takes wind forces into account at all. In which he applies the some formal and powerful techniques of boundary layer analysis, and I think that was one of the early papers in which Stommel introduced boundary layer techniques into oceanography.

P: Stommel?

C: No no, Robinson which I think have now flowered. Um. Well, I, um. In 1962, John Knauss at who was then in La Jolla, organized the expedition to look for the equatorial undercurrent in the Indian Ocean, and I joined that expedition for a month, and that required meeting meeting the Argo, which is the Scripps oceanographic vessel, in Mombasa. Stommel and I went down and we spent a little time in Nairobi and had a poor-man's safari in the Royal Nairobi Game Park, and had generally a marvelous time. Visited the Seychelles Islands on the way and. Altogether, uh, I think that, uh. And then my subsequent work on the undercurrent was certainly very much reinforced by this. I think, in my life, there have always been incidents which sort of set me off on something. My work on hurricanes came about because of 1954 hurricane Carol struck. And the von Neumanns were then visiting and staying in Szent-Györgyi's house right on the shore of Buzzards Bay and. They were staying in the summer cottage which was demolished during the hurricane, or swamped, at any rate. And I myself and a tree fell on our car, electricity was shut off, and I was ve very impressed by hurricane Carol. It it struck unexpectedly because it accelerated during the night and mo and many people didn't receive the warning. And I went back to Princeton and we tried got ahold and we tried that situation on the computer, the barotropic model, and found that indeed, um uh, we could predict the acceleration of the hurricane.

P: Wasn't your grid rather coarse?

C: It was coarse, but it still predic well, I think we used possibly a I don't know that. We may have used a somewhat smaller grid, but it was still a rather large grid.

P: Mm.

C: And at that point I don't remember whether th von Neumann's interest in hurricanes dates from that point, but I remember participating in discussions between with him and Teller on the use of atomic explosives to deflect the course of a hurricane. I found the idea of exploding atomic bombs in the atmosphere generally repugnant and sought for counter-arguments. One was that the energies weren't su were insufficient.
The other was that they had not taken into account... they were... in fact, they made use of Lamb's work on... on solid bodies in potential flow to... to conclude that the... the hur... hurricane would be permanently deflected, whereas I pointed out that, um... that... that in a stratified atmosphere the hurricane would merely oscillate and come back. But, when von Neumann was ill in... in Washington, I would come to visit him and we discussed the hurricane problem. And I became then interested in the mechanism of generation of hurricanes and this ultimately led to the notion of CISK, which came out in a paper that I published jointly with Eliassen in 19... let's see, it was sixty-four, sixty-five, some... But I could say... But I worked on the problem and one of the things that led me to the formulation of CISK was the idea that a hur... that the forces in a hurricane must be in essential balance, that you were dealing with a balanced flow, not an inertial gravity oscillation. But that if you take symmetric balanced flow it'll stay where it is. That is, if it's once in balance, it'll stay in balance. And therefore, starting... seeking for ways of disturbing the balance... Of course, I lit on the idea that a hurricane, being a warm-core phenomenon, is strongest at the ground, and therefore the frictional forces are enormous, and they would produce strong frictional indrafts, which would give rise to a hurricane circulation. But there was one thing... But, at that time, I was dealing with a conditionally unstable atmosphere, and we didn't really make very great progress until, in a conversation with Ooyama, he pointed out that... At the same time... Oh yes, I realize that it... you know, Syono had dealt with hurricane motions as sort of a gigantic convection cell. I knew that... In other words, an unbalanced convective cell. I knew that couldn't be correct, but I was still puzzled by the existence of conditional instability. But it was really Ooyama who pointed out that, despite the fact that the individual cumulus cells were condition... were conditionally unstable, that the hurricane as a whole was condi... was stable. And that sort of closed the problem for me because then, uh, the release of heat of condensation was simply a function of the frictional indraft, in a stable atmosphere, and that... and that, uh, the motion could remain balanced throughout. And that the clouds simply formed a cooperative function of... of... of... that the frictional indraft organized the clouds so as... so as to supply energy to the large-scale motion, and therefore you were dealing, not with a competing phenomenon as Syono would have had it, but as a cooperative phenomenon. And I invited Ooyama to MIT and he was there actually during the time that Eliassen and I were working on the problem, or maybe just subsequen... just subsequently to that. I don't consider that we ever solved... In... In... In the title of our article... We called it "The generation of a hurricane depression," not "The generation of depressions," because most
depressions have a cold-core component and, as has been found in GATE, the, uh, depressions that were observed there... that the... that the CISK process was probably not important. It's only when you develop a warm core does the CISK process become important, and then it becomes almost self-evident really, that the... that the frictional indraft of the hurricane is what supplies the energy for the storm. Ooyama also had been working on the same problem and, uh, would (?)... published a report about the same time that we published our paper. Since then I've remained interested in... by and large, the mechanism by which cumulus convection, which is so important both in the tropics and the ex... the extratropics is organized, and I've been rather impressed by Arakawa's work on that problem, but I don't consider that the... the problem by any means completely solved. For one thing, what one observes in these tropical motions is that... that there are not two... it's more than a two-... It's a multiple-scale problem, not merely a two-scale problem. That the... that the large-scale convective systems are not simply organized by the synoptic-scale motions, that there are intermediate motions of mesoscale, in which the cumulus activity is really taking place, and the understanding of these motions, I think, is still insufficient. Um. And actually, I supervised a graduate student, Inez Fung, in... in her thesis on the generation of cloud bands -- wave bands -- in a hurricane, in which, um... I think the proposal had originally been made, at least by others, in particular by Faller, who had actually looked into... done experimental work on Ekman instability, the instability of the Ekman boundary layer, and had shown that... and then, subsequently, had... then there was theoretical work by Lilly and others, showing that... in which one could explain these spiral bands that form in an Ekman... from spiral instabilities that form in an Ekman layer, and I think it was Faller who suggested this may be the explanation for rain bands. But the... To actually carry this thing out one had to do the stability problem in... that is, the... have a bound... a boundary layer stability which interacts with the stable layer above. And this is what Inez Fung did in her thesis. In other words, she solved the global problem, which was very analogous to the problem of... as it turns out, of spiral nebulae. The instability mechanism is, of course, totally different, but the mathematical problem is very analogous. And she was aided very greatly in her work by C. C. Lin, his associates, and their work on spiral nebulae. But I think here you have an example of an Ekman instability producing mesoscale motions. Er, mesoscale motions being produced by an Ekman instability, I mean. How are we doing?

P: Oh, I have fifteen minutes on this tape.

C: Okay. Um. Well, George, I think I've touched on a number of the...
P: Okay, let me . . . let me . . .

C: . . . papers that you mentioned.

P: Let me back up. But . . .

C: But maybe you have some things to . . .

P: Well, one thing you said earlier that is unrelated to what you've just now been talking about I want to ask about. You were talking about geostrophic adjustment, and it occurred to me that, I think, Obukhov's work has not yet come up in that connection . . . in our conversation, and, in particular, that aspect of it having to do, not with geostrophic adjustment, but with the quasi-geostrophic equations.

C: Yeah.

P: I think it's quite interesting historically. Do you want to comment on that?

C: No, I . . . When I did my work I was . . . well, I think my work appeared in print before Obukhov's, but the . . . I was certainly unaware of any work that he had done.

P: Right. (?)

C: But as I understand now, he had independently arrived at something very analogous to the geostrophic equations.

P: Mm hm.

C: That's true. As for his work on adjustment, I haven't kept up very much with that. (?)

P: But that's the same paper.

C: I know it. But you see, I'm really not that familiar with either one . . . either. (?) I'm aware of it, but I . . .

P: Well, let's . . . let's have a little change of pace and talk about, let's say, pseudo-science for a few minutes. You were involved in the deliberations that led to formation of NCAR.

C: Yes.

P: Do you think that it was a good idea to establish NCAR?

C: (laughs)

P: Do you think, whether it was good or bad, it has turned out
to be good?

C: Yeah. I think in the beginning I had great qualms. And I can tell you how this arose. Um. I became a member of the Committee on Atmospheric Sciences at the National Academy -- I think this was probably through Rossby's offices -- while he I think may have been still the chairman. This then would have been in fifty-six or fifty-seven. Then after Rossby's death, Bj . . . Lloyd Berkner became chairman, and Lloyd Berkner was then president of the Associated Universities. And it was he and maybe Paul Klopsteg who brought up the idea of the establishment . . . that further meteorological research . . . that there might be a point in establishing a separate meteorological . . . government-supported meteorological research institution. Another member of the committee was Tom Malone and, for some reason, Tom and I were assigned the job of sort of canvassing the principal movers and we . . . movers in the country on their reaction to the notion of . . . and I remember personally interviewing such people as Iselin -- Columbus Iselin -- Edward Teller, Harold Urey, and then Robert Oppenheimer. The greatest impression on me was made by Oppenheimer, perhaps because of my own experience at the Institute for Advanced Study. He said that these things succeed when they're done in good style, and that the way in which these . . . these institutions are inaugurated will influence their subsequent development. I . . . And it did. And so I made the point that I thought the institute should have a . . . that if there were an institute, and if it could be justified, it had to be at least partly on the basis that it would establish a standard of excellence, and that the universities should be willing to sacrifice some of their members to establish a very strong center. But the center should work in cooperation with the universities and not completely dominate them. That was a touchy problem. I could not at that time accept the notion that . . . that such an institute would be justified as a garage for aircraft or . . . or even for a large computer. Since then I've changed my mind somewhat. In other words, I think that the fact that an institution of this kind could make facilities available that would not be available to a university group has become an important raison d'etre. I think that where the institute has probably fallen down, and maybe it was not possible, um, in an institute run by the universities to . . . to have an establishment which was clearly superior intellectually. But I think it has been good enough intellectually to attract out . . . people from the outside, university people, as a center, not only for . . . that is for . . . I think it has been essential that they have in-house people who are doing interesting work to attract students, but also to attract . . . to act . . . to help in organizing meetings and joint enterprises with the universities. So, on the whole, I . . . I've always been very ambivalent about such things and expressed it in the early days, but on (?) . . . but in the end I
think that it has served its purpose, and . . . and, uh . . .

P: Have you yourself had much contact with NCAR?

C: Not as much as I would like, but I have visited there from time to time.

P: Have you used their facilities?

C: No, I have not, because of my connections with the Institute for Space Studies, first, and then, uh, I find that it has been possible to do a good deal of my work cooperatively . . .

P: Mm hm.

C: . . . that is, where it has involved a computer to do it cooperatively with, first the Institute for Space Studies, and now the Goddard Laboratory for Atmospheric Sciences, GLAS. Um.

P: Is there anything that NCAR should be doing today that it's not doing at all or doing well enough?

C: Well, I think that their primary problem . . . Of course, I'm being . . . having been a member of the President's Science Advisory Committee, that is, the President of NCAR's Science Advisory Committee, I'm aware of their fiscal problems, but I think, as in all intellectual institutions, their problem is still personnel. I mean, to be able to attract and hold able scientists. They have some, but I think it's still an important . . . I still feel that the element . . . that it should remain . . . I mean, it should strive to become as much an Institute for Advanced Study as it possibly can become. I think that not . . . not all people in the meteorological community, nor at NCAR, share this view. I think they . . . fiscally, it has a responsibility to the nation to undertake certain large-scale projects in which it isn't required that everybody be an independent prima donna. In fact, it's necessary that you have supporting personnel to carry out certain kinds of work.

P: If you were the director at NCAR, how would go about establishing the Advanced Study atmosphere that you speak of?

C: I . . . George, I'm glad I don't have that job.

P: (laughs) Okay, I'm going to change pace . . .

C: I think . . . Of course, I think, by and large, that good people attract good people.

P: Yeah.
C: And when I mentioned my . . . one of my reasons for going to MIT, I think, that if I wasn't . . . that . . . that Lorenz and I had a sort of a quasi-agreement that if . . . that if I went to MIT he would stay there. (laughs) That I would not have gone to MIT.


C: Yeah.

P: . . . in which he proposed the more extensive use of satellites for peaceful purposes. And a . . . And a part of that proposal had to do with enlarging the scope of meteorological data. Can you explain how that came about?

C: Yes I can. Because I had . . . I was intimately involved with that, and f . . . at least my personal knowledge goes . . . runs something like the following. In 1960, Bruno Rossi called me and . . . Bruno was then a . . . a consultant or adviser to Jerry Wiesner, who was then the president's science adviser -- saying that the president would like to have something done in the way of international cooperation in the light of developments in outer space, primarily. In other words, he felt very strongly that space technology could be used for other than military purposes, and should be used for such purposes. And . . . And I . . . as I subsequent learned, Kennedy himself was a bit of a meteorological buff. He was a great sailor.

P: Oh really?

C: And, uh, sailors can't help but be interested in the weather. But I can't guarantee that this was the way that proposal came about, because it originated with Wiesner. And, um, all I did at that time was to suggest calling a conference of a certain number of people, which included at that time Sverre Petterssen and Henry Houghton and, um, I think, Harry Wexler, and, oh, one or two other people, I think, where we discussed what might be done by way of meteorological cooperation. And, at that point, I think, all that was spoken of was, establishing meetings or exchange of personnel with the Soviet Union and some things of that character, nothing . . . But then, um, it came about that, perhaps through this, Sverre was a . . . was appointed to two things. One was to, I think, . . . At that point, the Committee on Atmospheric Sciences suggested that there be some kind of a ten-year plan, and I think, uh, Sverre was appointed to be the director of that study, to come out with a ten-year plan for meteorology. And also Sverre, I think, was head of a committee of the Amer . . . of . . . of the American Meteorological Society to look into the question of international cooperation. And, at
one of the meetings, I . . . that . . . which was . . . which took place in the fall or winter of nine . . . winter, I think it was . . . yes, I think it was . . . of 1960 -- so that was still the same year -- Sverre . . . Oh, look there was also, I think . . . the State Department had . . . I . . . I've forgotten the exact sequence of events here. But the State Department asked him to head a group to advise the State Department. Now, I've forgotten the order in which these things occurred and whether this was at all related to the AMS committee, but, in any case, there was a meeting, sponsored by the AMS, in late 1960, to discuss this general question of cooperation -- not merely with the Soviet Union, but world cooperation in meteorology -- and I simp . . . I . . . I can't . . . It suddenly occurred to me that, my god, the king has no clothes, and I blurted it out. I mean, everybody knew the fact that, before one could talk about long-range prediction, climate, or anything else, that we had to observe the atmospheric circulation, and that this had to be done with integrity; that is to say, that the whole circulation of the atmosphere for long periods of time was in mutual interaction and therefore one had to study the atmosphere as a whole and that modern technology made possible doing this. At that time, um . . . And I think the suggestion struck receptive ears . . . And, you know, I proposed that, you know, what . . . what with satellites and . . . and aircraft and balloons and whatever, uh, we were in a position of doing it, not as an operational experiment . . . operational activity, but something that could be done for a limited period of time. This would supply the data that one could then use to, ah, . . . to study the large-scale evolution of the atmosphere and climate and . . . and fill in the gaps that were presently missing. Because it could be shown that only about twenty percent of the atmosphere was adequately observed. And I was then made, perhaps on the strength of this or bec . . . oh, th . . . th . . . Then immediately afterwards there was a meeting, I think in December or January, of the Committee on Atm . . . Atmospheric Sciences, where I again made the proposal, and they appointed me chairman of a . . . of a panel on international cooperation. And, um, I proposed at that time holding a meeting at the Academy . . . the National Academy in March of nineteen . . . God, am I . . . I may be off by a year here, because I'm not sure wheth . . . whether it was March of sixty-one or March of sixty-two. I think it was March of sixty-one. In which we invited a great many representatives . . . in which . . . to . . . to . . . And I wrote a kind of a position paper, which was to serve as a . . . as a basis for preliminary discussion, about the feasibility of a global observation system, and its . . . and . . . the desirability and feasibility of a global observation system. The meeting was held but, you know, no great resolutions were made. I think that it was at least the first time that it was introduced to, um, the general meteorological community . . . And Harry Wexler, of course, knew about this in advance and, um, he telegraphed me or wrote me from
Geneva, where he was attending a . . . a meeting to form . . . to establish a world weather watch, and he asked me for this position paper, and I like to think that this played a role in the concept of the . . . not only in the concept, but also in the working out of the notion of the world weather watch. And, uh . . .

P: Under whose auspices was the meeting at which you presented that position paper?

C: The position paper . . . That was under the National Academy. It was held in the headquarters of the National Academy in Washington. And after that, uh, Sverre asked me to be on this committee . . . I think it was after that . . . which was to propose . . . Okay? . . . which was to propose, um, a . . . a . . . mechanism for international cooperation or propose ideas and . . . and which led to . . . That led to Kennedy's . . .

P: Oh, I see.

C: . . . offering of the resolution about international cooperation and the study of the dynamics of climate, but did not incl . . . Nothing was said about a global observation system at that stage.

END OF TAPE 5, SIDE 2
TAPE 6, SIDE 1

P: We continue the interview with Jule G. Charney. This sixth session is being conducted on Thursday, August 28th, 1980, in Boston, at the Holiday Inn. Okay.

C: Okay . . .

P: We don't . . . (?)

C: Uh. Look, I . . . I'm . . . Let's see, where was I? Um, oh, we were talking about the Kennedy resolution. Subsequent to that, I became chairman of . . . Well, in my capacity as chairman of the panel on international cooperation, I did a phys . . . a feasibility study, which appeared finally in print in no . . . in 1965, I think, on the feasibility of a global observation system, with many people involved. I really sort of directed the thing, wrote the introduction.

P: Was that the NAS report?

C: That was the National Academy report. And then, after that, in 1968, was appointed chairman of the, uh, committee on the Global Atmospheric Research Program. But, of course, much had happened in . . . in between. There was a conference in Stockholm proposing a global atmospheric research program. I mean, my . . . my idea was much more limited to a global observation system or global observation experiment. Uh, but as . . . as it developed, I think it was a . . . (?) development into a global atmospheric research program was probably a very good idea. And it was then that we proposed the GARP tropical Atlantic experiment, because, being convinced that we had to understand the tropics much better as a main supplier of the atmospheric energy . . . And one more role that I played was that, uh . . . At the beginning there was no oceanographic component, but I did strongly urge at a number of meetings that, uh . . . You remember, in particular, one in . . . at NCAR, that there should be a . . . a global . . . an oceanographic component, because this was an unprecedented opportunity, that you had ships that would be here or on the equator anyway, that you would have the meteorological observations, and these should be . . . there should be a study of equatorial oceanography conducted at the same time. And, despite the fact that the . . . MODE was then in full operation, and that demanded the time of ocean . . . oceanographers, and ship time, there was a fairly substantial oceanographic GATE component. Uh, and I think the results were very well justified there. Well, George, I think that's enough for that. Um. I will have to check the sequence of events. I may have gotten some things wrong, but we can do that subse . . . later on. Um. So, do you have any other questions, or should I simply . . .
P: Why don't you just carry on.

C: Talk about other things.

P: Yeah.

C: Well, I think we mentioned a great deal of my work already at MIT. Oh, you did touch on, um, the question of vertical propagation. Let me just say a few things. One is that, uh, the notion of vertical propagation, um, had already been . . . that I had already come to this idea, um, as early as 1948 with the discovery of the geostrophic equations, and the i . . . and wh . . . and looking for the vertical influence domain in the atmosphere. But at this stage, um, I wa . . . became concerned . . . became interested in taking up the no . . . the opposite idea, namely, I was already by then rather really . . . I never really believed that the . . . that the high-level mesospheric motions could exert very much influence on the at . . . on the lower atmospheric motions, simply because they didn't have the energy. But it remained a completely open question in my mind whether the opposite was true. And so, I sim . . . I set out to investigate the vertical propagation of energy from the lower into the upper atmosphere. And what puzzled me, to begin with, was why we didn't have an atmospheric corona because this was after all, uh, Schatzman's and Schwarzschild's idea for the solar corona, that you had energy propagating from the energetic part of the sun's convective layer. So, at that time, Phil Drazin had . . . was visiting the mathematics department at MIT and, I've forgotten just how it occurred, but he was interested in atmospheric motions, and so he came over to our department and began working with me. And I proposed that he should join me in this activity, which was a very good thing indeed. And, to- gether, we, uh . . . we calculated . . . we . . . we . . . we . . . applying a W . . . Well, first of all, we set up some models, in which we calculated the propagation of energy. And then, we applied WKB methods and . . . where we had a . . . a kind of an index of refraction and we could investigate the trapping of . . . or the propagation of . . . the vertical propagation of planetary waves and found out two interesting things. One was that easterlies were very strong trap . . . trapped the waves, and this accounted for the lack of . . . for the . . . in a sense, for the independence of the upper stratospheric motions, um, in the summertime, when you have easterlies. And we had then come to the conclusion that the motions were . . . must have independent energy sources, mainly of . . . essentially ozone heating. And later on I supervised Leovy's thesis, which was to actually calculate the symmetric motions with the ozone heating. And to my knowledge this was the first time that anyone had combined the stratospheric ozone chemistry with the dynamics. Uh. Well, I think this was later on. Maybe it was before. I'm not sure. Anyway, the other thing
is, that the motions would be trapped by strong westerlies. And by and large the observations seemed to confirm our conclusions. Then it occurred to me that . . . to look into the . . . the influence on the large-scale motions . . . the transports of heat and momentum by the large-scale motions. And by just straightforward calculation, taking into account the stratosphere and even the discontinuity between the troposphere and the stratosphere, you got . . . You know, the eddies then acted as a source of heat and momentum for a symmetric circulation. And when . . . But when you combine the two, you . . . you got the result that, in a steady motion . . . in this steady inviscid flow you got no . . . there was no trans . . . there was no effect at all, on the zonal flow. I, uh, left that for further study, I think realizing that viscosity or transience would . . . would vitiate these results. But . . . And then . . . This was in 1960-61, and I presented the work at the Helsinki meeting of the IUGG . . .

P: Mm.

C: . . . and on the way back from that meeting, I stopped to see Arnt Eliassen in Oslo, and it turned out that he had worked on a very s . . . on . . . on both horizontal and vertical propagation of both gravity and planetary waves and I've forgotten whether . . . I don't think he paid particular attention to the westerlies and easterlies and their influence on the trapping, but everything that we had done was also implicit in his work, and furthermore, he had discovered a relationship between energy and momentum propagation (?), something like that, and the zonal flow, which was a thousand times neater than our calc . . . straight (?) calculation, and I subsequently incorporated his result in our paper, with due acknowledgement. I, uh . . . I suppose I mention this because people have then asked me whether this played any role in our paper, in our original work, and the answer is no, it played no role. But I was glad to . . . I think it would have been advisable to have these . . . these mundane and nasty questions of . . . of attribution arise, and I think I can say that his work and our work was totally independent, but that his work now has . . . and our work has been vastly generalized by I think, . . . first by Holton and then, more recently, by McIntyre and . . . and Andrews, so that there is a now a . . . you know, a very extensive theory of wave-mean-flow interaction. But it remains true, in a very general context, that you have to have transience or viscosity before you can get an interaction. Once you have an interaction, you get all kinds of interesting phenomena, like the quasi-biennial oscillation in Plumb's experiment. Are you familiar with his experiment?

P: No.

C: That's fascinating. You take a . . . a stationary
cylindrical annulus and you produce a standing oscillation on the bottom with some kind of rubberized material or some kind of a flexible material, let's say wave number . . . I don't know, I've forgotten (?) four, five, or six, something like that. And then . . . And then . . . the . . . You make the fluid stratified with salt or whatever. And you first engender stationary gravity oscillations, but after a while you find a circulation beginning to develop near the top . . . or at the top, where it doesn't remain constant but works its way down, followed by a circulation in the opposite direction. And this goes on forever. And it's a wave-mean flow interaction in the presence of dissipation. And that is . . . was used by Plumb to explain . . . or to substantiate Lindzen and Holton's work on the explanation of (?) . . . essentially they gave this explanation for the quasi-biennial oscillation.

(phone rings, tape goes off, then on)

P: Okay.

C: Well, George, um, so much for that. Um. One minor thing. Um. In my . . . I . . . Erik Mollo-Christensen, my colleague at . . . in . . . carried out a series of experiments on the stability of the Ekman layer, very accurately, to determine the parameters which this kind of Ekman-Rayleigh number criterion for the onset of instability. He actually found two instabilities occurring at different values of these parameters and, um, this has subsequently been investigated analytically, numerically . . . numerically, mainly, and they found the two instabilities. One can be explained as basically a Rayleigh shear instability of the normal component of the flow. The other had no explanation. Really, . . . Well. Really no explanation. I think the explanation has now been given by a student of mine, um, Kerry Emanuel, who shows that it's basically a . . . a centrifugal inertial instability.

P: Mm. Mm hm. I'm familiar with that.

C: Um, that's one interesting thing about the Ekman layer. (laughs) Another interesting . . . well, another thing that interests me was that I've often wondered why the Ekman layer in the atmosphere and in the ocean is the depth it is. And it occurred to me that the Ekman lay . . . that the depth of the Ekman layer is such that the small-scale instabilities that occur because of the shear act as bulk viscosities, um, in the presence of which the motion is marginally . . . the largest-scale motion, that is, the motion of the depth of the Ekman layer is marginally stable. And that turns out to give you excellent values for the depth of the ocean and the atmospheric boundary layer.
P: Mm.

C: I published this as my contribution to a ... a memorial volume of Oceanographia, the Russian publication, uh, for Shtokman, when he died. So much for the Ekman layer. (laughs) Um. I'm just skipping around from one subject to another.

P: Fine.

C: Um. You mentioned my work on deserts. I ... When I took my sabbatical, I spent half of my time ... Pekeris had been urging me for a long time to come and help him establish some kind of a meteorological group at the Weizmann Institute, but I couldn't. Or ... Or come there myself. Well, I couldn't see how to do either of these, basically. And I pointed ... And I suggested to him that the only way to do this was to send some students to me and I would ... whom I could help to train and who would then go back and serve as a nucleus which I could then interact with. Well, this never happened. It really began ... It finally did happen, but only ... but at the Technion, not at the Weizmann Institute, where I had two postdocs, Israeli and Merkine, who I think are really first rate, and ... and would serve as a nucleus. Actually, the Weizmann has made some gestures toward Merkine. But by now Pekeris has retired and it's not clear that they have any interest in these things anyway. Anyway, so I resolved to spend half my time in Cambridge and half my time at the Weizmann, and, um ... I told you that I worked in Cambridge basically, or mainly, on the, uh, generation of gravity motions by quasi-geostrophic motions.

P: Mm hm.

C: But I was thinking then in terms more of ... of the Lighthill problem where you could generate gravity motions in the lower atmosphere and they might propagate into the upper ... upper atmosphere and you ... as a possible source of gravity and wave energy. But also of the general problem, which I think, in the end, was ... one aspect of which was solved by this student of Lorenz's, Errico.

P: Did something come of your work in Cambridge?

C: No, I never published it. Uh.

P: You know, that's a problem that I attempted to grapple with when I was in London for a year. The generation of gravity waves by ... 

C: Oh, really?
The jet stream. Yes. Through the Lighthill mechanism. I never got anywhere with it.

No, well. It's a tough problem and, I think, part of the problem lays in the appropriate formulation, really.

Yeah.

And I'm not sure. I had a couple of discussions with Lighthill on the problem. He placed a great deal of emphasis on a necessary impedance match.

Mm hm.

In other words, you. The kind of motions you're most likely to generate are those which match in length and frequency, or in scale and frequency, the geostrophic motions. But, as I say, nothing much came. I worked on one or two other little problems, but I was really too busy enjoying the splendors of Cambridge. (laughter) to really get very much done. Going to tea, talking to people there.

Is that the. Were there other occasions when you spent a substantial block of time in England?

No. Uh, yes, one other time. Um, on my leave of absence from MIT, and spent the spring in Oslo, part of the late spring and summer in Imperial College.

Mm hm.

where I gave a few lectures and had an opportunity to discuss various matters with Eady and Green, and others, got to know, uh, Percy Sheppard much better, but I didn't think that that was particularly. It didn't lead to anything in terms of research very much. But very thoroughly enjoyable. Um. Where was I before that? (?) don't know that it really matters.

Oh yes. Oh, while I was in Oslo I did write this paper on the general circulation for the Rossby volume, in which I showed that you could have derived using a method of J. T. Stuart for calculating finite-amplitude instabilities, that you could have derived Eliassen's not Eliassen's, but Phillips' numerical results. Um, I'm not sure of the correctness of that work because subsequently Pedlosky's work on finite-amplitude instability suggests that. Well, my work was had a
strong ad hoc component. In other words, it wasn't rigorous. It may have some ultimate justification but . . . I think the dynamics of my waves was very different from what Pedlosky has found in his studies. Well, if we can leave that subject, there are two other things that I could . . . I could . . .

P: By all means.

C: . . . perhaps mention. That when I was at the Weizmann Institute, um, I felt that I should work on something which would be of interest in the context of the Weizmann Institute in Israel, and at that time . . . I had always been puzzled by the phenomenon of deserts and had always . . . and then I had a strong aesthetic interest in deserts because I loved the Mojave Desert in California and would frequently go there, both in connection with mountain climbing expeditions and just simply to visit the des... . . . the desert because it's so very beautiful in the springtime. And then, even when . . . even when the flowers aren't there, I . . . . . there were . . . there is something about a desert that holds great attraction for me. I've seen . . . I've driven through the Arizona and New Mexico deserts and all that . . . To me, they have a . . . a most impressive beauty. Anyway, . . . And I wasn't satisfied with current explanations of deserts, and it oc . . . At the same time, I was aware of some of the satellite observations, which indicated that the desert was a sink . . . was a . . . a sink of heat, radiatively, rather . . . that is to say . . .

P: (?) Do you (?)

C: . . . the net radiation over the Sahara and Arabian deserts, uh, in the summertime, when you have an excess of radiation at these . . . at those latitudes . . . well, in general, in the northern hemisphere . . . you have a deficiency of radiation over the deserts, and that could only be . . . in my mind, could only be attributed to the high albedo of the desert, despite the fact that the desert is hot, that is, the surface of the desert is hot. That the . . . well, or maybe because of that, that the radiation emitted by the desert was much . . . that there was less than . . . than the radiation absorbed by the sun. I mean, the sun is enough to heat the surface of the desert, but it's not enough to heat the whole air column. And since . . . At first, it seems a slightly paradoxical result, but I convinced myself that this was the case. And I worked out an elementary dynamical theory of this phenomenon, in which the radiative imbalance was balanced by frictional inflow. In other words, that since you were losing heat from the desert, you had to import the heat, and that the way the heat was imported was basically from the ITCZ at the edge . . . at the southern edge of the desert. And, um . . . But at that time, we were getting news of the terrible drought in the Sahel, and it occurred to me that the . . . that the
overgrazing which I ascertained occurred over an enormous area... I mean, the cow, since there's very little forage to... in the first place, that the... he could crop whatever brush or grass exists over an enormous area, and that... that it was not unreasonable to take an area the order of 400 kilometers in north-south direction and going across the whole Sahara, practically, in the east-west direction. And I proposed to the... Jastrow and Halem at the Institute for Space Studies that... that they should take their... they had already made some calculations with a model in which they used a... Clapp's and somebody else's albedo values and simply raise the albedo over this particular strip and see what happened. And indeed that resulted in about a forty or fifty percent reduction in rainfall. And there I became interested sort of in the general climatological question of how alteration of surface properties could influence regional climate. And, of course, it's not only albedo, but also soil moisture which affects the... because evaporation is a very important thing. And so we did a number of experiments subsequently to study the effect of moisture and albedo, and we found that these effects are really very large. And... I mean,... After that, uh,... it's partly as a... as a consequent... I was really rather surprised that the results that we... that we extended the calculation for longer periods of time and did them over again with different initial conditions, and the results were very stable. In fact, too stable, in my view. And so we began looking at the causes of variability at low latitudes and came to the con... and... Shukla and I did this together and we came to the conclusion that... that there was really less variability at low latitudes. Shukla did some empirical work with such observations as existed. Manabe had also studied variability, but he sort of terminated his calculations at 30 degrees because he simply didn't think he had enough observations. But we tried to extend this further down and came to the conclusion that there was less variability, and we attributed this to two things, partly to the absence of baroclinic instability, and partly to the fact that easterlies inhibit propagation of energy from westerly... from westerly middle-latitude regimes. Because this business of propagation of gravity waves... not of... of planetary waves applies for hori... to horizontal as well as vertical propagation -- as I had mentioned in an earlier paper, and used it partly to account for the absence... for the fact that the meanders... the... the... the unquestioned meanders of the upper circulation, say over the trades, seemed to have no effect on the steadiness of the trades. And I attributed this... First, I arrived at the result by a scale analysis and then also, uh, derived it in a second note as a consequence of the trapping of planetary wave energy by easterlies. And, so Shukla and I came to the conclusion that there was more hope for predictability at low latitudes than at high latitudes because of the... and that even if long period motions, which were capable of
propagating did enter the low latitudes, they would be of long period. They too, therefore, would be more predictable. And this idea has been carried further by Shukla and some others' work. And, I think, um, that the . . . you know, there's much more to be done along those lines. And this leads me to my final topic, which I . . . which arose when I spent a sabbatical semester, or a sabbatical half-year . . . not a sabbatical -- leave of absence -- in UCLA, in 1978. Um, they asked me to teach a course or give a seminar and I offered to give a seminar on long-period, large-scale motions in the atmosphere, on the grounds that while people were making extended-range forecasts, we seemed to have absolutely no dynamical inkling of (?) influence the variability of the atmosphere on very long scales. And I thought that . . . you know, . . . that as Oppenheimer once answered a reporter who was . . . asked what they did at the Institute for Advanced Study, he said, "Well, we study various problems, and what we don't understand, we explain to each other." And I thought that in the seminar we would do the same thing. And so I reviewed a great deal of the empirical evidence of . . . for long-period fluctuations, and then was reminded of the fact that in 1967 I spent another leave of absence at UCLA, my home, my alma . . . my old alma mater, and at a time when Dick Lindzen was visiting . . . during part of which time Dick Lindzen visited. And we got to talking about the generation of long waves. And I proposed . . . I remember proposing that if you had a fluctuating zonal current over topography, this could generate long waves. But I didn't pursue that idea. Subsequently, Hirota, in 1970 or seventy one, or maybe sixty nine, or . . . around 1970, published a paper in which he showed that if you . . . if you had a fluctuating zonal flow, you did indeed generate planetary waves, which looked very much like the observed planetary waves. And I reported on Hirota's result to the seminar, and then asked the question, What causes the . . . Could there possibly be a feedback between these fluctuating flows and the fluctuating zonal current? And it occurred to me that, if the disturbances of the zonal flow were caused by topography, why shouldn't the torque created by the disturbances, uh, influence the zonal flow su . . . such as to, perhaps, accelerate it, or retard it, as the case may be, and thus give rise to an instability? Which I sub . . . later on called "form-drag instability." And I thought . . . And then . . . I proposed to the seminar that we should all look at . . . we should take a spect . . . a low-order spectral model of the kind first advanced by Lorenz in his explanation of dishpan circulations, but for a barotropic flow, which would be driven by some unspecified momentum source and dissipation, and see what we got, with sinusoidal topography. So we took a beta-plane channel, with sinusoidal topography -- periodic channel --, and did this, and lo and behold, ah, such an instability exists. Ah, we did this first for an in . . . for inviscid flow. Then, later on, um . . . but th . . . then when we put in viscosity, we found that
... that you didn't get the fluctuation, that the ... what happened was ... Then, investigating that further, we found ... or, at this point, when I say "we," I mean ... there was one of the graduate students who was working with me, John DeVore, and myself. We found that, in a driven flow with dissipation, there exists a multip ... three equilibrium states, not one, and that ... that ... and that the instability we ... inviscid instability occurs only for the inter ... for one of the intermediate states, which lay between super ... strong super-resonance and sub-resonance, that is, with respect to flow over a sinusoidal body. That is, where the Rossby waves would be ... forced Rossby waves would ... I mean (?), the free Rossby waves would be sta ... stationary. And we found that when we disturbed that intermediate equilibrium, um, the flow fell into the attractor basin of one of the other equilibrium points in the phase space, then wound down into it, so we didn't get an oscillation. But then when we added, uh, y-modes ... we had only one y-mode to start with ... that then you got the ... ... an instability of the kind already discussed by Lorenz and Gill, that is a Rossby wave instability, which then could give rise to fluctuations. And subsequently, Dave Straus and I ... Dave Straus was a postdoc of mine. I had started him off on looking at the MIT model that Phillips had developed for the study of, uh, propagation and dynamics of the stratosphere, and which was carried forward by Ron Prinn, our ... our atmospheric chemist, who studied the dyn ... the ozone dynamics and ozone chemistry of the stratosphere ... to look into the dynamical properties of this model, which hadn't been done. And so he was the logical person to work with me on the baroclinic version of our ... this barotropic study with DeVore. And we found a number of interesting things. Uh. We found that the form-drag instability continues to exist, that it occurs at the point where the zonal flow attains resonance with the ... with the topography, that it gives rise to multiple equilibria, that these equil ... that the ... Oh, I should add, to the barotropic study, that when you looked at the two stable equilibria, one looked very much like the normal state of affairs, and the other looked like blocking. So, I then ... this im ... then it immediately occurred to me that blocking was an alternative ... that the atmosphere can exist in a multiplicity of equilibrium states of which blocking was one. That it was ... that it represented not a spurious concatenation of events, but a ... but a true equilibrium, quasi-stable.

P: How many blocks do you get?

C: Well, so far we're dealing with ... with sinusoidal topography.

P: Oh, I see.
C: Then it turned out . . . Well, we did that and, um, we found
that these blocks in certain parameter ranges were
orographically, or form-drag, stable, but were Lorenz-Gill, uh,
unstable . . . or maybe not Lorenz-G(r)ill, because these are not
baroclinic waves, and they have, um, . . . uh . . . These waves
themselves could be baroclinically unstable. And I came to the
. . . this . . . that . . . it occurred then to me that perhaps
these long propagating gravity waves which had heretofore been
studied as free waves were in reality, uh, instabilities of the
quasi-sta . . . of the quasi-stationary flows, of various kinds
-- I won't say wh . . . how, because it's difficult to separate
them, the barotropic from the baroclinic effects. Um. And very
recently, Shukla and K. C. Mo, one of the, uh, scientists at the
Goddard Space Flight Center, have worked together in wh . . . on
models in which we've made use of a . . . of a work of John
Hart's, in which he shows . . . in which he simply, mainly,
points out that under . . . that . . . that he could have
derived the Charney-DeVore equations if . . . without truncation
if he merely had assumed that the scale of the north-south topo
. . . topography was much greater than that of the east-west
topography. And I adapted that idea to more realistic motions by
appl (?) . . . showing that if you take a Fourier series in the
north-south direction, and take the actual flow in the east-west
direction, and then truncating the north-south Fourier series,
you . . . that the equation for the perturbation of the zonal
flow is linear, and therefore you can deal with arbitrary
topography in the zonal direction. And we've done that, and we
find that we can explain the global characteristics of blocking
phenomena reasonably well, that is, in the sense of its troughs
and ridges, but of course we can't explain the regional character
of blocking. And now I'm working on . . . And also Lee-Or
Merkine and Eugenia Rivas, former postdocs of mine . . . well,
not postdocs; Lee-Or was a postdoc, Eugenia was my doctoral
student. They have undertaken, first Eugenia and I, and now
Eugenia and Lee-Or, to study the flow in a channel over an
obstacle . . . or over obstacles. And Lee-Or has made . . . And
I had arrived at the conclusion that . . . that if you have an
isolated obstacle, that the high . . . highly truncation methods
that . . . that . . . that we had used before were not adequate,
and that the important . . . and that by analogy with the fact
that one knows already in hydraulic flow over an obstacle that
. . . that at the critical Froude number you do get multiple
equilibria. There are two possible states. Uh. And that these
instabilities arise from an unstable bifurcation. That the same
thing might apply, uh, to an isolated obstacle in the atmosphere.
This is in order to get the regional character of the block. And
I have now by verbal communication from Lee-Or Merkine that he
has shown that if you take zonal flow, where you specify the flow
upstream, over a two-dimensional obstacle, isolated, that that
flow is stable when the disturbance group velocity is essentially
stable. In other words, it's . . . Oh, what I showed earlier
not showed, but what I had come... I had concluded earlier, that with an isolated obstacle, as opposed to an infinite sine wave, it's the group velocity that you have to think about, not the phase velocity. You get a... something that corresponds to resonance. And Merkine has now shown -- rigorously -- that indeed this happens, that... quite independently, um, and that when the group velocity of the disturbance is comparable to the... is zero, you do get an instability. And so that... I find this is where we are now. I find this a very exciting field, and hope to have time left to work on it. (laugh)

P: Wonderful! I'm sure you will. This last that you were discu...

END OF TAPE 6, SIDE 1 (END OF INTERVIEW)
1. See transcript page 41.

I asked Hsiao-lan Kuo to comment on these passages. He said that, when engaged in his doctoral research (published in 1949), he was familiar with Lin's work on hydrodynamic stability. However, although he and C. C. Lin attended Tsing Hua University (Peking), it was not until after he became a staff member at MIT in 1949 that he established professional contact with Lin.

2. See transcript pages 59-60.


Later Rossby co-authored with Ertel: A new conservation theorem of hydrodynamics. Pure and Applied Geophysics, 14, 189-193, 1949. (A more extended version, in German, was published the same year in Sitzungsberichte der Deutsche Akademie der Wissenschaften zu Berlin.)

3. See transcript pages 67-68.

I believe Jule was mistaken in asserting that the dispersion relation in Rossby's paper of 1939 is wrong (and I was mistaken in saying it has a misprint). Unfortunately, I did not examine this baffling assertion until the transcript was available, almost a year after Jule's death, so it is not possible to know what he had in mind.

The reader can compare the texts reproduced in Appendix A, one from Rossby's paper of 1939, the other from Jule's thesis of 1947. Jule's equation (31) is clearly the same as Rossby's (39), and Jule's (34) the same as Rossby's (42) or (43). Moreover, Jule's implication (transcript p. 68) that he went farther than Rossby by deriving the cubic equation and interpreting its roots is also apparently wrong.
4. See transcript page 70.

In referring to Richardson's "fundamental error" Jule remarks that on first reading Richardson's book he thought the fundamental error (in the forecast) was that Richardson did not properly evaluate the horizontal velocity divergence, and he implies that I had taken the view this was a mis-reading of the book. His recollection here seems confused. Richardson himself had come to realize that use of unbalanced initial data was, indeed, his fundamental error, and I concurred in this diagnosis (Bulletin of the American Meteorological Society, 48, 514-550, 1967; see Section 6).

What I think Jule may have intended to say is that I had pointed out to him (and in print, op. cit., end of Section 4) the common misconception that Richardson's forecast failed because the condition for computational stability was not satisfied, a mis-reading that Jule himself seemed at times to voice. For example, in his contribution to the Compendium of Meteorology (1951, p. 477) he wrote:

Although Richardson ... effectively excluded sound waves with the hydrostatic approximation, he did not exclude external gravity waves whose speeds are nearly as great as those of sound, about 300 m sec⁻¹. He chose Δx to be about 200 km; consequently his Δt should have been smaller than 200,000/300 sec or[11] min. In point of fact he chose 6 hr. Hence a direct application of his method would inevitably have led to computational instability.

Notwithstanding the tense in the last sentence, an unwary reader might not realize that, since Richardson took only one time step, computational stability had no bearing on his forecast.

5. See transcript pages 77-78.

I asked Arnt Eliassen to comment on this part of the transcript. He wrote (1982 September 27):

... It is right as Jule said in the interview that he first became acquainted with my quasi-geostrophic (as I called it) method through seminars and conversations -- my English manuscript was not finished until after he left Norway early in 1948. The transcript confirms my impression that he didn't show much interest in this part of my work; in fact, I am not sure that he quite understood it at the time. This is perhaps not surprising, since my explanations must have been very poor; I don't think I recognized then that one of my equations was in fact a kind of quasi-geostrophic vorticity equation. Besides, I had great
difficulties in expressing myself in English, as well as understanding spoken English.

What I called the quasi-geostrophic wind formula in my paper, I inherited from works of Hesselberg, Brunt & Douglas, and Sutcliffe, to which you refer. I think I knew also the paper by H. Philipps, which you mention in the interview, but that didn't add anything new of interest to me.

My point was that a closed set of equations could be obtained by combining the "quasi-geostrophic wind formula" with the continuity and thermodynamic equations, and I knew from pilot studies that these equations gave tendencies of reasonable spatial distribution and right order of magnitude.

As I have told you in a previous letter, I had two versions of my quasi-geostrophic equations. In the first the convective acceleration was written as $v \cdot \nabla v$, and in the second as $v \cdot \nabla v$. It was Ragnar Fjortoft who gave me the idea to the second version, which is now called the geostrophic momentum equations.

I don't recall that Jule and I discussed the matter in Princeton 1948-49 or in the following years. I had come to the conclusion that it was much nicer as Jule did to use the geostrophic approximation in the vorticity and the advection velocities. In comparison, I found my own geostrophic equations rather messy, with somewhat arbitrary approximations based on intuition rather than mathematical rigor. I have a dim recollection that I once derived a very distorted potential vorticity theorem from these equations. I may have told that to Jule, and I think this must be what he refers to on the bottom of p. 77 in the interview transcript.

When I worked with frontal circulations later in the fifties, however, I came to the conclusion that the geostrophic momentum equations were perfectly suited for this purpose since they contained the all-important advection of entropy and momentum by the cross-frontal ageostrophic, divergent velocity component, which was ignored in Jule's quasi-geostrophic equations. It is clear from the transcript that Jule ultimately, after the Hoskins and Bretherton papers, came to the same conclusion. Thus the choice between the two versions would depend upon the nature of the problem to be studied.

The "previous letter" referred to above was written in reply to an inquiry from me as to "the historical roots of the quasi-geostrophic equations, both in the wide sense of the converging schools of thought and in the more local circumstances that led to your own ideas." (I intended to comment on this subject in a
It is with some reluctance that I do as you ask me to, i.e. give my views of the historical roots of the quasi-geostrophic theory. I remember quite well how my own ideas developed, but little about other people's. Therefore, my account will be disgustingly self-centered; balance can be restored only by your getting similar stories from the others involved!

I came upon a version of the quasi-geostrophic equations during the war, when I was employed as a meteorologist in the forecasting section of the Meteorologisk Institutt in Oslo. This was during the German occupation of the country, and there was no Norwegian forecasting service in operation; instead we served as a coding center for the underground, but there was time also for studies.

I had recently taken my cand. real. degree with Professor Halvar Solberg, together with a nice group of students, of which you know Ragnar Fjöroft and Geirmundur Arnason ("Snorre" for short). Solberg went through parts of Physikalische Hydrodynamik and Brunt's textbook in a two-year lecture cycle. Solberg was not interested in the geostrophic approximation, which he barely mentioned. He wanted meteorology to be "exact" and was even reluctant to use the hydrostatic equation. However we (his students) had learned about isallobaric and cyclostrophic wind components from Sverre Petterssen who gave a course for us in January – March 1939 just before he left for MIT. I think we were aware that accelerations, and hence ageostrophic winds could be estimated geostrophically. It is possible that we had read Hesselberg's paper from 1915, and H. Philipps' paper from 1939 (Met. Zeitschr. 56, p. 460). However, the paper by Jeffreys from 1919 was unknown to us, I believe.

What I did in 1942 was to combine the quasi-geostrophic wind formula with the continuity eq. and the thermodynamic energy equation, and obtained two linear diff. equations for the vertical motion and the pressure tendency, with coefficients and forcing terms expressed in terms of the instantaneous pressure distribution. I could not solve them in general, of course, but I constructed a three-dimensional pressure field corresponding to waves in a baroclinic westerly current, and could then evaluate the fields of $w$ and $\partial p/\partial t$, and hence the phase velocities. They came out with reasonable magnitudes. I was pleased because I had previously tried a similar calculation based on the
"tendency equation" and could not get values of \( \frac{\partial p}{\partial t} \)
that made sense.

In 1943 I gave some seminars at Meteorologisk Institutt about this work. But at that time there was a big gap between theory and practice in meteorology, so it was not surprising that I did not get much response (Fjörtoft was in Bergen then, and Arnason in Stockholm). I remember that I received the advice that approximations should always be made in the final result, not at the outset! Also Einar Höiland was very skeptical to begin with.

Arnason once told me that he did something very similar when he was in Stockholm during the war.

The last war years were rather hectic, and so were the first post-war years; there was little time for research. In 1947, I had finished a manuscript about pressure coordinates, when Einar Höiland asked me to add the quasi-geostrophic theory, because then the paper would be sufficiently big for a dr. philos.-thesis. I found that the theory was simpler when expressed in pressure coordinates.

At that time I had also read Richardson's "Weather Prediction by Numerical Process", which I understood quite well, and Rossby and Coll. (1939), which I did not understand then -- I didn't see what relevance the non-divergent dynamics had to the atmosphere. Rossby himself was not very clear -- he gave a lecture in Oslo in 1946, and justified the non-divergent condition by referring to the small depth of the atmosphere compared with the earth's radius -- a rather irrelevant argument.

Then Jule Charney came to Oslo in the summer of 1947 and stayed almost a year. And he had a much better argument in favor of Rossby's barotropic model in his Ph.D. thesis, I thought. Until then, I had taken it for granted that NWP had to be carried out in three dimensions in order to simulate the atmosphere -- but Jule gradually convinced me that it was worth while to try the barotropic model first. It was only after I met Jule that I realized that one of my "quasi-geostrophic" equations was in fact a version of the vorticity equation. It seemed to me then that Jule's method was nicer, i.e. first derive the vorticity equation, eliminate the divergence and then introduce the geostrophic approximation; and this is still my opinion, if you ask me to choose between the two.


The accumulators, the main computing units of the ENIAC, used decimal arithmetic. See Section 9 of Arthur W. Burks and

7. See transcript page 93.

Jule implies here that I had told him Jeffreys had attempted to evaluate the divergence geostrophically in formulating the problem of dynamical weather prediction. That is not so. What I think Jule probably had in mind were my remarks (*Bulletin of the American Meteorological Society, 49*, 497, 1968) on an early paper by Jeffreys (*Philosophical Magazine, 1919*) that contains a nearly correct form of the barotropic, quasi-geostrophic potential vorticity equation. Jeffreys's error was not, however, in evaluating the divergence.

8. See transcript page 100.

According to progress reports made in 1950 and 1951 to the Office of Naval Research by the Meteorology Group at the Institute for Advanced Study, Eliassen arrived in Princeton in 1948 and remained as a member of the Group through August 1949. Fjortoft was there as a member from September 1949 through June 1950 and from September through December 1950. I was a consultant to the Group in October, November, December 1949, in March, October, November 1950, and in May, June 1951.

9. See transcript pages 102-103.

After reading this page of transcript I wrote to Norman Phillips for help. He replied (1982 April 19) as follows:

In the late summer of 1950 I had applied the quasi-geostrophic formulation, as it was then explained in Jule's recent papers, to a 2-level incompressible fluid model that Rossby had used in his lectures at the University of Chicago. I had not succeeded in deriving dispersion relations, however, because shear in the basic current introduced a $y$-dependence in the perturbation equations through the slope of the interface. I believe it was in the fall of 1950 that you, as my thesis advisor, arranged for me to present my work to Rossby and Charney. They encouraged me to ignore this $y$-dependence. (Later theoretical developments showed this to be a legitimate step for quasi-geostrophic motion.) The simple dispersion relations that resulted from following their advice enabled me to use Eady's dispersion relation to translate my 2-layer model into atmospheric terms. My
thesis was therefore derived completely from work by Rossby, Charney and Eady. It did affect the Princeton project, however, since until then an advective baroclinic model that ignored static stability had been considered by Charney as the next step after the barotropic model. It was Jule, in the summer of 1952, who took the step of using finite-differences in the vertical to derive baroclinic models in a logical way.

10. See transcript page 104.

In his letter to me cited above, Norman Phillips comments as follows:

I know of no numerical error in the 3-level computations reported by Jule in his 1954 paper in the Proceedings of the National Academy. Perhaps the "error" that you recall refers instead to a meteorological failing that I may have voiced to you in those days, concerning the overestimate of vorticity advection in the upper troposphere that is introduced by using the analyzed isobaric heights to define the wind field. (Aside: the 3-level results reported by Jule evidently did crystallize the decision to establish the Joint Numerical Weather Prediction Unit in 1954, as it was the first model programmed for use, beginning in May 1955. (H. Elsaesser, 1960: JNWP Operational Models. National Meteorological Center Office Note No. 15.) But it gave such unreliable results that it was quickly superseded in April 1956 by a 2-level model formulated by Philip Thompson. A revised 3-level model designed by Cressman was reintroduced in early 1963. End of Aside). Jule published a study with Bruce Gilchrist and Fred Shuman in 1956 (The prediction of general quasi-geostrophic motions. J. Meteor., 13, 489-499.) that is relevant to this part of the interview. It shows that Jule was well aware of problems associated with any geostrophic model.
APPENDIX

A. See interviewer's commentary 3, page 151.


If we replace $f + \zeta$ on the right hand side of (31) with a constant $f^e$ and introduce a constant mean value $D_{\infty}$ for the undifferentiated $D$ in equation (33), equations (31) to (33) are readily solved. For perturbations which are independent of the $y$-coordinate, one obtains finally an equation for the wave speed which may be written

\[(U - c) \frac{4 \pi^2}{L^2} = \beta + \frac{\sigma f^e}{gD_{\infty} - (U - c)^2}.\]  

It is interesting to note that for $U = 0$, $\beta = 0$, this equation gives the wave velocity obtained by Sverdrup (1926) in his study of tidal waves on the North Siberian shelf. For $U = 0$, $\beta = 0$, $f^e = 0$ it reduces to the well known formula for long gravitational waves

\[c = \pm \sqrt{gD_{\infty}}.\]

For standing perturbations ($c = 0$) it reduces to the relation obtained earlier in this paper by elementary methods,

\[L^2 = 2\pi \sqrt{\frac{U}{\beta}}.\]

For a homogeneous atmosphere the product $gD_{\infty}$ has a value of about $8 \times 10^4 \text{ cm}^2\text{ sec}^{-2}$. For the perturbations in which we are interested $(U - c)^2$ is of the order of magnitude $10^2 \text{ cm}^2\text{ sec}^{-2}$ or less. Thus it is a permissible approximation to write

\[(U - c) \frac{4 \pi^2}{L^2} = \beta + \frac{\sigma f^e}{gD_{\infty}}.\]

or, after solving for $c$,

\[c = \frac{U - \beta L^2}{4 \pi^2},\]

in which expression the length $\lambda$, defined by

\[\lambda = \frac{1}{f^e} \sqrt{gD_{\infty}},\]

has a value of about 2800 km for a homogeneous atmosphere. Even for wave lengths double the value of $\lambda$ the wave velocity determined by (43) is only 10% less than the one obtained from the approximate formula (17).

Simplifying the perturbation equation (25) by means of these relations and introducing the expressions (26) for $u', v'$, and $w'$, we obtain

$$
iu g[1 + (s/g)(ar{u} - c)^2]U + sf
\begin{align*}
iu (ar{u} - c)^2 U - f(\bar{u} - c)V_s &= - (g/\rho)(\bar{w})_s, \\
iu (\bar{u} - c)^2 U - f(\bar{u} - c)V_s &= 0, \\
iu U + (1/f)[U - \mu^2(\bar{u} - c)]V &= - (i\bar{u}/g)V.
\end{align*}
$$

The last two equations show that $U$ and $V$ are constant, and elimination of $U$ between the first and third equations gives

$$
\left\{ \begin{array}{l}
\frac{\partial \beta}{\partial z} \left[ c + \mu^2 (\bar{u} - c - u_0)(\bar{u} - c)^2 \right] \\
+ \frac{\mu^2}{f^2} g \beta (\bar{u} - c - u_0) \right\} V = - \frac{g}{f} (\bar{w})_s, \quad (28)
\end{array} \right.
$$

where the quantity $u_0$ is defined by

$$
u_0 = \frac{f_s}{\mu^3} = \frac{\Omega L^2 \cos \varphi}{2s^2R} \quad (29)
$$

and is called the 'critical speed' by Bjerknes and Holmboe [7]. Integrating (28) from 0 to $z$, and utilizing the condition $W(0) = 0$, we obtain

$$
\left\{ \begin{array}{l}
(\beta - \beta_0) \left[ c + \mu^2 (\bar{u} - c - u_0)(\bar{u} - c)^2 \right] \\
- \frac{\mu^2}{f^2} (\beta - \beta_0)(\bar{u} - c - u_0) \right\} V = - \frac{g \beta}{f} W, \quad (30)
\end{array} \right.
$$

where $\beta_0$ and $\beta_0$ are the mean surface density and pressure. Evaluating the terms at $z = \infty$, we derive the following equation for the wave velocity:

$$
\bar{u} - c - u_0 = \frac{f}{\mu^2 g H_0} \frac{c}{(\bar{u} - c)^2}, \quad (31)
$$

where $H_0 = R \bar{T}_0/g$ is the height of a homogeneous atmosphere whose surface temperature $\bar{T}_0$ is equal to the mean surface temperature of the barotropic atmosphere. This equation was derived by Rossby [14] for an incompressible atmosphere and by Holmboe [12] for a barotropic atmosphere.

Introduction of $\bar{u} - c - u_0$, from (31), into (30) gives

$$
W = - \frac{fc}{g} \left[ 1 - \frac{gH}{gH_0} (\bar{u} - c)^2 \right] V, \quad (32)
$$

where $H = R \bar{T}/g$. It can be shown that the equations of the present section hold not only for an adiabatic barotropic atmosphere but also for an arbitrary barotropic atmosphere. In particular, we may set $H = H_0$ in (32) and deduce the interesting conclusion that $W = 0$ in an isothermal barotropic atmosphere.

Since (31) is of the third degree in $c$, it has three roots. Two of the roots can be shown to be nearly equal to the solutions of

$$
(\bar{u} - c)^2 - gH_0 = 0, \quad (33)
$$

which is Lagrange's equation for gravitational waves in a moving fluid. Writing

$$
gH_0 = R \bar{T}_0 = \sigma \bar{c}/c,
$$

we see that the gravitational wave speed is of the order of magnitude of $\sigma$, the speed of sound. As we are here concerned only with long waves whose speeds are very small compared with that of sound, we may ignore $(\bar{u} - c)^2$ in comparison with $gH_0$ in (31) and so obtain the equation

$$
\bar{u} - c - u_0 = \frac{fc}{\mu^2 g H_0}, \quad (34)
$$

whose solution can be shown to be nearly equal to the third root of equation (31).
Lieutenant Philip D. Thompson  
Institute for Advanced Study  
Princeton, New Jersey  

Dear Phil,

I thoroughly agree that the questions you propound lie at the very heart of the whole problem, not only of numerical forecasting but of the solution of the equations of motion by any means whatever, and I am very pleased to hear that you are now grappling with them. As you know, I have long been aware of these questions and have from time to time sounded off at some length about them. I am therefore not only willing but anxious to discuss them with you.

Let us begin with your last question, "Why don't the large scale atmospheric disturbances move with the speed of sound?". One answer was given by a scientific pundit writing in the Readers Digest. It is obvious he says, that man exists only because of a very improbable concatenation of events. If the solar radiation were twice as great the oceans would dry up and man would simply find existence too uncomfortable. Or of the earth rotated at a much reduced speed he would freeze in winter and roast in summer, etc., etc. Done, Dieu existe. One could add in the same vein that if cyclones traveled with the speed of sound man would be whisked right off the earth, which is manifestly impossible according to our learned scientist. In case these anthropomorphic arguments leave you cold, and you do not believe in the Bible or even in the Readers Digest, I propose the following argument.

In the terminology which you graciously describe to me we might say that the atmosphere is a musical instrument on which one can play many tunes. High notes are sound waves, low notes are long-inertial waves, and nature is a musician more of the Beethoven than of the Chopin type. He much prefers the low notes and only occasionally plays arpeggios in the treble and then only with a light hand. The oceans and the continents are the elephants in Saint-Saens' animal suite, marching in a slow lumberous rhythm, one step every day or so. Of course, there are overtones; sound waves, billow clouds (gravity waves), inertial oscillations, etc., but these are unimportant and are heard only at N.Y.U. and M.I.T.
To become literal we might say—the energy that goes into an atmospheric disturbance depends on the initial mode of excitation. A forced perturbation of long period produces a disturbance of long period. A perturbation in which energy is released so fast that the air does not have a chance to get out of the way could produce sound waves of very large amplitude to consume this energy. But with the exception of volcanic eruptions and atom bombs such agencies are never found. Even the atom bomb converts only a small part of its energy into waves of concussion.

Let us illustrate by considering the motion of waves in a constant barotropic zonal current. The equation of motion are

\[
\frac{\partial u}{\partial t} + u \frac{\partial u}{\partial x} + \nu \frac{\partial^2 u}{\partial x^2} + \omega \frac{\partial^2 u}{\partial z^2} = 2\nu \frac{\partial \zeta}{\partial x} - \frac{1}{\rho} \frac{\partial \rho}{\partial z} - 2\nu \frac{\partial \zeta}{\partial x} - 2\nu \frac{\partial \zeta}{\partial z}
\]

\[
\frac{\partial v}{\partial t} + u \frac{\partial v}{\partial x} + \nu \frac{\partial^2 v}{\partial x^2} + \omega \frac{\partial^2 v}{\partial z^2} = -2\nu \frac{\partial \zeta}{\partial y} + 2\nu \frac{\partial \zeta}{\partial z} - \frac{1}{\rho} \frac{\partial \rho}{\partial z}
\]

\[
\frac{\partial w}{\partial t} + u \frac{\partial w}{\partial x} + \nu \frac{\partial^2 w}{\partial x^2} + \omega \frac{\partial^2 w}{\partial z^2} = 2\nu \frac{\partial \zeta}{\partial y} - \frac{1}{\rho} \frac{\partial \rho}{\partial z}
\]

and for waves of small amplitude and infinite lateral extent, propagated in the \(x\)-direction, they become

\[
\frac{\partial u}{\partial t} + U \frac{\partial u}{\partial x} = f v - \frac{\partial p}{\partial x}
\]

\[
\frac{\partial v}{\partial t} + U \frac{\partial v}{\partial x} = -f u - \frac{\partial p}{\partial y}
\]

\[
\frac{\partial w}{\partial t} + U \frac{\partial w}{\partial x} = -f v - \frac{\partial p}{\partial z}
\]

where \(U\) is the zonal speed, \(f = 2\Delta \omega \theta \), \(u\), \(v\), \(w\), respectively the velocity components and barotropic pressure function \((\int \rho / \rho_0)\) of the disturbance, \(\phi\) is the undisturbed value of \(\int \rho / \rho_0\) and as usual we neglect the vertical components of acceleration and coriolis force as well as the horizontal component of coriolis force involving \(w\). These equations were solved by Rossby for a disturbance of the form

\[
u = A e^{i(x-ct)}
\]

\[
u = B e^{i(y-ct)}
\]

The solution gives the value of the velocity \(c\) :
where \( \beta \) is equal to \( d\phi/dy \) and is assumed that the atmosphere were incompressible and homogeneous \( \phi \) would be the dynamic height.

Now here is the important point. The last equation has three roots. One is very nearly equal to the solution of this equation without the \( \omega^2 \)-term in the denominator on the right hand side. The other two roots are nearly equal to those obtained by setting the denominator on the right equal to zero. This means that there are three modes of vibration, and it is easy to see that the first root corresponds to long waves and the remaining two to gravitational waves traveling in opposite directions. (Sound waves are eliminated by the assumption of no vertical acceleration [quasi-horizontal motion]). The general solution for a given initial disturbance would embrace both long inertial and gravitational waves. But if, for example, the initial disturbance were harmonic and had a period equal to that of the long waves no energy at all would go into the gravitational wave components. In general, of course, every disturbance, if broken down into harmonic components by Fourier analysis, would exhibit components with all periods, and therefore some of the energy would produce gravitational oscillations which you will observe, have velocities of the same order of magnitude as that of sound. (In an isothermal barotropic atmosphere, for example, two solutions of the above velocity equation are given approximately by

\[
(U-c)^2 = \frac{2\kappa}{\kappa} \frac{dP}{dP} = RT
\]

whereas for sound we have

\[
(U-c)^2 = \frac{c^2}{c^2} RT \sim RT
\]

But since most of the energy of the initial disturbance goes into long period components very little of the energy will appear in the gravitational wave form.

This lends us to the next problem, namely, how to filter out the noise. Pardon me, but let us again think metaphorically. The atmosphere is a transmitter. The computing machine is the receiver. The receiver is a very good one indeed, for it produces no appreciable noise itself, i.e., all noise comes from the input. (I am supposing that you can compute to any desired order of accuracy). Now there are two ways to eliminate noise in the output. The first is to make sure that the input is free from objectional noises, or the second is to employ a filtering system in the receiver. Translating the first method implies that the unwanted harmonics shall be eliminated from the raw data by some type of harmonic analysis; the second that you transform the equations of motion and make approximations in such a way that the bad harmonics are automatically eliminated. Let us consider the second method and illustrate by
means of the foregoing example of wave motion. If, in the solution of the equations of motion, whenever a term containing the factor 
\( \frac{1}{\sqrt{-u-c}}/s \) appears, we replace it by 1, then the resulting equation for \( \phi \) would be:

\[
U - c - \frac{\beta L^2}{\sqrt{\pi \tau}} = \frac{L^2 + \frac{\beta}{\sqrt{\pi \tau}}}{\sqrt{\pi \tau}} \frac{c}{\phi}
\]

instead of

\[
U - c - \frac{\beta L^2}{\sqrt{\pi \tau}} = \frac{L^2 + \frac{\beta}{\sqrt{\pi \tau}}}{\sqrt{\pi \tau}} \frac{c}{\phi - (\frac{\beta}{\sqrt{\pi \tau}})}
\]

Thus the equation would have only one root and that one would correspond to the long waves. But this does not tell us what to do with the equations of motion themselves. If you work backward you find that the approximation is equivalent to ignoring the \( x \)-component of the acceleration i.e., by assuming that the north-south perturbation velocity is geostrophic. Now don't jump to the conclusion that the latter approximation may always be made. We can do it here because we have assumed no variation in the streamline pattern in the north-south direction. I do not know what will happen if you consider waves of finite lateral extent as Haurwitz does. Here the problem becomes more complicated since Haurwitz assumes that \( \phi \approx \frac{\beta}{\sqrt{\pi \tau}} \), which, of course, is tenable only for barotropic motion.

In my paper on baroclinic waves I find that one has to make a number of approximations of the type \( \frac{\beta}{\sqrt{\pi \tau}}/\phi \approx \frac{1}{\phi} \) to arrive at a tractable system of equations, from which gravitational waves, Helmholtzian waves, sound waves and inertial oscillations are eliminated. But I also consider only waves of infinite lateral extent. I still don't know what types of approximation have to be made in more general situations.

On the other hand, don't think that compressibility is what boggles up the works. Even if you were to replace the actual atmosphere by a non-homogeneous incompressible atmosphere with the same stability you would still have gravitational waves. However, if you accept the consequences of the above reasoning you will perhaps share my conviction that there is a general type of approximation or transformation or what have you that will eliminate the noise and the problem is now to find it.

Enough of this. Let us change the subject. Do you remember my suggestion that you study simple types of finite amplitude motions as a preliminary step to attacking the general forecasting problem? In particular, the Rossby wave model? Well, for various reasons I do not think that that particular study will lead to anything very

\*This approximation is justified since \( \phi \) is of the order of the square of the speed of sound.
interesting. If a barotropic system is stable, then the horizontal divergence is negligible and the first order approximation is very nearly the exact solution. On the other hand, if the motion is unstable and develops into vortices, the successive approximations will be significant. I have begun an attack on several such problems and the results look promising.

I would like to discuss some of these things with you personally since the time scale of interchange of ideas by correspondence is just too great. If I had the dough I would hire myself to Princeton and have it out with you, but naturally I haven’t. Our Quarter ends on the 21st of March and I am catching the boat for Norway on the 22nd so I will not even be able to stop over in Princeton. With all that Navy money lying around why don’t you invite me to come to Princeton for a couple of days? In any case, write and let me hear your reactions. Also give my best regards to Panofsky.

Sincerely yours,

Jule Charney

JC:RF
Dear Phil,

I have put off writing this long because I have been working on the "filter" I discussed with you in Princeton, and I wanted to wait until I should be able to tell you the results in a reasonably complete form. But now, although I am not quite finished, a circumstance has arisen which makes it advisable to poke my head through the lace curtain.

The circumstance is this. Having, after seven months, seen a pretty fair sample of Norwegian meteorology and meteorologists, I was able to judge who had gathered most of the slivers from the scepter of V. Bjerknes. The choice narrowed down to two. They are both young men at the meteorological institute in Oslo: Klissassen and Fjortoft by name. Now besides playing the part of Zarathustra, I have also been making strenuous propaganda for the Princeton Project. The more I see of the feable efforts of meteorologists here and elsewhere, including myself, the more I am convinced that weather forecasting is a computing problem, and that its solution requires one highly intelligent machine and a few mathematico-meteorological oilers. Well at present it is my impression that you have, or are about to get the machine, but that sufficient oilers are lacking. Therefore it occurred to me that you might be able to use one of these bright young men. Klissassen has had a very good mathematical and physical training under V. Bjerknes and has worked on the problem of numerical computation. He is aware that the finite differences cannot be chosen arbitrarily and that the initial conditions propagate at a finite rate. He is also aware of the impossibility of integrating the equations of motion as they stand. In short he is not naive. Furthermore he is one of the best forecasters in Norway and so has a proper appreciation of the physical aspects of the problem (Norway is a small country and even the theoretical meteorologists work). Fjortoft is damn good also, but he is not available.

I talked the thing over with Sverre Pettersen who now directs the meteorological institute and he heartily approved of the idea. It seems that Klissassen is scheduled for a year's leave to devote to research anyway. He had intended to work with Chapman and Taylor in England, but it was not difficult to convince him that Princeton was a likely place.

Pettersen is going to pull wires. He mentioned Captain Orville. I suggested however that since Von Neumann is the de facto head of the affair, a private whisper in his ear through his chief lieutenant might not come amiss. Hence this letter.

You may be wondering at all this solicitude for the project. Well, I may as well break down and tell all. Yes, I wouldn't mind chipping in my two cents for a while. Rossby expects me to go back to Chicago and Bjerknes to California, but I can't see either of these deals at the moment. Rossby is scheduled to remain in Sweden for two years, Starr has left for M.I.T., and while Palmen is a hell of a swell guy, I don't feature knocking around all alone with him and Byers -- both synoptic men. As to California -- it has a fine climate.

But the main reason is that I have been brooding about the problem of numerical computation ever since coming to Norway, and I think I have come up with an answer to at least one of its most vexing aspects, namely, the practical impossibility of determining the initial vertical velocity and acceleration fields with the necessary accuracy. The solution is so absurdly simple that I hesitate to mention it. It is expressed in the following principle. Assuming conservation of entropy and absence of friction in the free atmos-

Oslo, 4 November, 1947
sphere, the motion of large-scale systems is governed by the laws of conservation of potential temperature and potential vorticity and by the condition that the field of motion is in hydrostatic and geostrophic balance. This is the required filter! It really does eliminate the small-scale "noise". It is possible to justify the approximations used in deriving the filtering principle by a method of scale analysis analogous to the type of reasoning used in justification of the boundary layer approximations of aerodynamics. The value of the filter for numerical computation lies in the fact that the equations of motion can now easily be reduced to a single equation in the pressure alone!

The paper has not yet been sent to the publishers so please, Phil, keep the results under your hat until further word. I will send you a typewritten copy as soon as I can have one made.

Maybe I am all wet, but if not it looks as though it should soon be possible to start turning the crank. All one needs by way of initial data is a knowledge of the initial three-dimensional pressure field. (This would not be true if the primitive equations of motion were used. It is only true for the filtering equations).

Harry Wexler once invited me to join the project, and so, I think, did Von Neumann. What is the setup now? How are things with you and yours? I return to Los Angeles in May; Minor and Nicky perhaps earlier, but I will certainly stop over in Princeton to see you. Are you there? Would be much obliged if you could send me some of your literature (the projects I mean). Are you able to continue degree work? What the hell's happening in Princeton? The socialist government here promised too much electricity to private industry. As a result they turn of the lights at 11:30. As now finishing by candle-light, although the days are getting so short that candles will be needed all the time. Will save chit-chat about family and Norway for later. Minor sends her love.

Jule
D. See interviewer's foreword, page iii.

Following is a facsimile of a letter from Jule Charney to George Platzman, 1980 May 9.

Boston, May 9, 1980

Dear George,

A very belated but heartfelt thanks for the Rayleigh biography. Rayleigh and Helmholz (and Klein) are my great nineteenth century heroes. They came alive for me when I heard V. Bjerknes in Oslo tell of meeting them at international meetings he attended as laboratory assistant to his father, Christian. The biography was read with pleasure throughout my hospital stay.

I recovered well from the upper left lobotomy and the subsequent radiation treatment, but I have been left with a debilitating temperature which the doctors can’t explain. They have found no evidence of cancer spread and think it more likely that I have some internal infection, but the final answer is not in yet. Up to now my chances of a cure seem moderately good, but of course one lives with a rather heightened sense of mortality.
I hope this will not sound unmodest to you. But I feel one of the tasks of meteorology to present some of my intellectual autobiography, and given the uncertainty of time, I feel that the best first approximation would be a tape-recorded oral autobiography taken in the presence of a knowledgeable interlocutor. Afterwards, if time permitted, a more finished product could be worked up from the tape record.

Actually, Harpers has asked me, among others, to write such a biography, with generous assistance from the Sloan Foundation. (You may have read one such, Freeman Dyson’s “Understanding the Universe,” which first appeared in The New Yorker.) I don’t consider myself a literary craftsman and so rejected the idea at first, but then began having second thoughts. By an accident of fate, I was lucky to play an important role in the technological-scientific transformation of atmospheric dynamics in
The mid-twentieth century

The idea would not be to produce merely a biography, but an intellectual history of the times, looked upon personally. The interrogators with whom I could interact currently would be first myself. We went through much the same experience. Then Norman Phillips. Younger people would not look upon the problem of solving the mystery architecturally as a problem. Joe Pedlosky would be beguised on the dynamics, but he has never been interested in NWP.

Some of these ideas just whatever
have discussed in the past. Now I have a more personal reason. In any case, George, please let me know what you think of this whole project.

With warmest best wishes to you and Dorothy.