Serendipity: Research Career of One Scientist

Akira Kasahara

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Preface and Acknowledgements

Approximately from the time of my graduation in 1948 from the Geophysical Institute, University of Tokyo, the science of meteorology began making a dramatic progress by the advent of a new weather prediction method based on numerical time integrations of atmospheric models with electronic computers. Reflecting on my life during the last 65 years, working as a research scientist at various prime educational and research institutions in the world, I realized that my career was influenced directly or indirectly by this rising tide, which provided me serendipitous opportunities to work on various topics of numerical weather prediction and computational modeling of the atmosphere. Since my career evolved in parallel with the advance of modern meteorology, I thought that it would be of interest to document how such research opportunities came about, what my motivation was to undertake a particular task, and what I learned from it. This essay describes my research memoirs on selected topics more or less in chronological order.

I would like to thank Rick Anthes and Rol Madden who read an early version of the manuscript and gave me constructive comments and editorial suggestions to improve the contents. I am pleased that Warren Washington and Joe Tribbia gave me useful advice for writing this essay. Writing of the war-time recollections during my youth was assisted by Jerry Meehl, to whom I would like to express my gratitude. Also I am delighted that Bill Large, Director of Climate and Global Dynamics Division, and Jim Hurrell, Director of NCAR, have extended my appointment at NCAR as Senior Research Associate. Submission of this technical note was assisted by Christy Edwards.

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January 2014
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Table 1. Landmarks of numerical modeling for weather and climate since 1950.

Fig.1. Prof. J. Holmboe and I posed in front of the building of Department of Meteorology, UCLA. Date: June 18, 1954.

Fig. 2. My family members with wife, Yuko and daughter Alice on the baby carriage met George Platzman and Robert Simpson (far right) on the campus of University of Chicago, dated July 6, 1959.

Fig. 3: A dinner party during the First International Numerical Weather Prediction (NWP) conference in Tokyo, November 1960.

Fig. 4. Group picture taken during the Study Conference on Parameterization of Sub-grid Scale Processes, March 20-27, 1972 at Main Geophysical Observatory, Leningrad, USSR, arranged by the Joint Organizing Committee (JOC) of Global Atmospheric Research Program (GARP). From front to back and left to right:

Fig. 5. From left to right three fireman's hats were worn by then NCAR Director, Tim Killeen, CGD Director, Jim Hurrell, and UCAR President, Rick Anthes together with Warren Washington and myself. This picture was taken after a special symposium in honoring Warren in August 2007.
1. My life before leaving to the U.S. in 1954

I was born on October 11, 1926 in Tokyo, Japan. My early childhood education was very much affected by the turmoil of war-related events. I was well aware of the Japanese conflict with China that started in the early 1930s, and when I got to middle school, it was really at the height of militarism. All children had military training, but it was considered to be athletic exercise. Toward the end of the fourth year of middle school, I was taught how to use a rifle. Japan entered into the World War II in 1941, but the following year I was admitted to go to the Urawa Senior-High School. The War ended in 1945 and the same year I was accepted to receive my undergraduate education at the Geophysical Institute, University of Tokyo and graduated in 1948.

On December 7, 1941, when Pearl Harbor was attacked, it was December 8 in Japan, and I was in senior-high school. Since the war had been going on in China for so long, it wasn’t a big shock. People were anticipating that something could happen, because we knew the diplomatic situation with the U.S. was going downhill. I was too young to make judgments, but I remember that initially most people were very excited about Pearl Harbor. It sounded like things were going very well until we heard about the Battle of Midway. The Japanese media made it sound like it wasn’t all that bad, but we knew we couldn’t trust the media because it was controlled by the government.

I remember what was later called the Doolittle Raid in 1942. The American bombers came right overhead. The bombers I saw were flying low, and they seemed quite large, bigger than the other planes I had seen earlier over Tokyo. The media didn’t report the event very much, but people started to talk about how badly the war was going.

During my senior high school, we only had classes one day a week. The rest of the week, including Saturdays, all the students had to work in factories and the teachers too. There was also one day of military drill. I was working at an iron press factory just in the outskirts of Tokyo. We would get big chunks of iron, and we’d put them into a press that would make them into plates. About a year before the end of the war, we noticed that we had more and more idle time because we were waiting for the materials to be delivered. At that time, even before the bombing started, I realized that the war wasn’t going well for Japan because the government wasn’t able to deliver the iron to us like it had before. Obviously, we knew we couldn’t trust everything we read in the newspapers, but we were hesitant to talk about the situation or say anything negative about the war.
So we just sat around at the factory waiting for materials to come in, and what else could we do but talk about things. Some of my friends were very vocal and the sort of activists who said more than others about how they thought we were losing, but then I heard that they were arrested. We realized that if someone overheard anyone talking about things going badly for Japan in the war, they’d be turned in and arrested. So after that I didn’t talk about the war. You had to be careful.

There was a military draft, and draft age was nineteen. I went through my physical examination and would have had to go into the military service except I got a deferment because I was a science student. I think medical students also got a deferment. But, of course, a lot of students in other areas of study in our college had to go into the military.

The air raids before 1944 were sporadic and infrequent and didn’t really bother us, but when the B-29 raids started in 1944, and we saw so many planes flying overhead, we knew something had changed for the worse. But even though we saw the bombers dropping bombs, as long as they don’t drop them on your head you don’t worry too much. I think that is the psychology of survival. It’s kind of strange. No one really got angry at the Americans when the bombing started, and neither did it increase our will to fight. We couldn’t get angry at the Japanese government either. The military police were particularly active, and if you said something critical that someone overheard, you could be in much worse shape than if you were bombed by the Americans. So, that’s the choice you had, and when you come into a situation where there is no solution, you just don’t think about it. We couldn’t do anything but try to survive. We couldn’t go anywhere. We couldn’t do anything to stop our country from fighting, so what option did we have? It was a very helpless feeling. All we could think about was how to live under these circumstances, how to survive.

During the last year in senior high, I took the entrance test to go to the University of Tokyo and luckily I was accepted in February, 1945. Then, in March 1945 our family’s house was hit by a big firebomb raid. I happened to be home that night with my parents, my two sisters, and elder brother. I had another elder brother who was drafted and sent to China for a while, but he got sick and they sent him back to somewhere else in Japan at that time. So he wasn’t home.

First we could hear the planes coming overhead, but we couldn’t see them because it was dark. Then, air raid sirens went off. The bombing was taking place downtown and we saw fire in distance. We decided to see what happened next rather than going to a designated evacuation area. Then, I
saw big fires in the sky. It was a B-29 that got shot down in distance. It went down in a long bright streak in the darkness. But as the downtown fires intensified, the flames propagated and started burning toward us. They were getting closer and closer, and we were finally told we had to evacuate. We had a designated place to go, and we realized that our house was going to burn and there was nothing more we could do. So we grabbed our things and left. To get to our evacuation location, we had to go through an area that had just burned. I saw a lot of people who had been burned. There was a bomb shelter where people had gone, and they were killed down in that shelter, either from the heat or lack of oxygen. I saw people dying as there weren’t many medical people around, but I did see a few fire engines.

My father had made a shelter in our backyard, not for us but for some of our belongings, including my books, which I thought important. We dug out a hole and made a concrete shelter. We figured we were probably safe from the bombs, but fire was what we were worried about. We thought we could escape to our evacuation area, but we figured we can’t carry our belongings with us. That is why we made the shelter. I put a lot of books in the shelter, and I still have some of the books that were burned around edges. It’s kind of interesting what you keep in a situation when you think everything might be totally destroyed. I have a feeling that people are basically optimistic. They never think this is an end of the whole world. You believe you are going to survive somehow, so you try to keep whatever is important to you. I must have had hope for the future, even though I didn’t realize it at that time.

So it was a frightening night, with so much of Tokyo burning --a huge fire-- and I think it was especially traumatic for old people. My father was about fifty-five or so, and I think it was hard for him. If something like that happened to me now, it would really affect me! But I was young, and it seemed to me at the time that we were just going to deal with things as they happened.

We got to our evacuation area in a nearby park, and a relief group was there to help people. People were talking about all the areas of Tokyo that were burning. I remember very well the fires and evacuation, but I can’t recall where we stayed that night. It must have been in the park, and it wasn’t just that night but for the next few days. A few days later we went back to look for our house, but it was totally destroyed by fire. We dug out our possessions from the shelter and moved outside of Tokyo to live with relatives. After that raid I went back to the university, and I found that our Institute had already evacuated to the town called Iwamurata in Nagano prefecture, about 70 miles northwest of Tokyo. There the life was peaceful though the food shortage was very severe. Nevertheless, we had nothing
except to study, attending classes and reading books. One day in August, we heard the news that two atomic bombs were dropped on Hiroshima and Nagasaki.

That news really worried me a lot. I had taken a university physics course, so I knew what it was, and the other science students knew what it was too. A few days later, we all listened to the Emperor’s address on the radio telling us we’d lost the war. I was really surprised by what he sounded like. We had never heard his voice before as we were told that the Emperor was a god. After the Emperor’s address, I heard that American ships were coming into Tokyo Bay. We didn’t have any idea what could happen next. But very soon, even in the first few days after the Americans came, things started to turn around for the better, so I felt very relieved. Trains started moving again, and we started to get electricity and water back, so the infrastructure came back fairly quickly. But then things became really bad for the economy. We had a terrible inflation and severe food shortages. The rebuilding was a massive task.

My father ended up going back and rebuilding our house, but he rebuilt it on another lot nearby. He didn’t own the land we were living on, so he rebuilt on another smaller lot. I can’t remember exactly how that worked out, how he was able to manage to rebuild the house when there were so much shortages and a lack of construction materials. I think he did it gradually over time as things became available. I was still living with relatives in a suburb of Tokyo. Right after the ending of the war, our Geophysical Institute of the University returned to the original location in the campus of the University of Tokyo in Hongo. I continued my education at the Institute and finished my undergraduate degree in 1948 from the University. Then I had graduate education and passed a qualifying exam in 1950, which was like receiving a master’s degree, writing an article on the structure of tropospheric jet stream. I was fortunate to be accepted as a special graduate student and a small stipend was provided by the government. This gave me a great relief as I knew how much hardship my parents had to have to raise our family.

While I had rather scanty education during my senior high school days amid the war hysteria, I received a solid education at the University of Tokyo. For example, physics courses on classical dynamics given by Prof. Takuzo Sakai and on fluid dynamics by Prof. Ichiro Imai were excellent and I learned a great deal from these able professors. At the Geophysical Institute too I took all excellent courses on seismology, geomagnetism, earth dynamics, oceanography as well as meteorology. During the four-year program at the Institute, each student had to decide after two-years a particular area of geophysics for specialization. I chose meteorology as my field and studied
under Prof. Sigekata Syōno who was an outstanding teacher and researcher on dynamic meteorology.

One of the things the American occupation forces did when they came into Tokyo was to set up a library. They really worked hard to win our hearts. During the war we couldn’t read any American or English journals or publications, but now we were able to read at the library in Hibiya all kinds of publications from the U.S., including science. That was the first time I ever saw the *Journal of Meteorology*. In fact, all my senior and junior colleagues at the University and Japan Meteorological Agency as well as myself eagerly spent a great deal of time at the library reading and copying the articles of science journals.

One unforgettable incidence related to our exposure to foreign science journals happened one day when Prof. Syōno rushed to come to our class room in the Institute with a copy of now classic paper by Jule Charney (1947) on baroclinic instability in *Journal of Meteorology* in his hand and excitedly proclaimed “Look! This paper has modernized the science of meteorology!” Obviously, we students had to read that paper very carefully from the beginning to the end. My impression was that because Prof. Syōno himself had been trying to develop similar dynamical theories on the disturbances in the westerlies in a series of his work on atmospheric disturbances (for which he received a prestigious Japan Academy Award in 1950), he was shocked to find that his long-term wish had been accomplished already by Charney. [John Lewis (1993) wrote an article on student activities at the University of Tokyo under Prof. Syōno during the post-war era in Japan.]

In 1952 my senior colleague Kanzaburo Gambo was invited by Jule Charney to work at the Institute for Advanced Study and left Japan for Princeton, NJ. At that time, Dr. Gambo had been Research Associate at the Institute. I was then appointed to take his position as Research Associate to Prof. Syōno. This was my first regular employment position. In the same year I married Yuko. Her parents graciously agreed to build one room for us next to their residence in the lot they owned and my father managed to build the addition. Yuko was a classmate in the elementary school. Although we didn’t see each other until toward the end of WWII, we met again at a class reunion after the war and started to date. At the time we married, Yuko had a good job in the ladies’ dress section of the Matsuzakaya Department Store in downtown Tokyo. Since both of us had income, our life was fairly well off even though the economy after the war was pretty bad due to material shortages and high inflation.
2. The beginning of my career in the U.S.

In 1954, I completed the graduate course at Geophysical Institute, University of Tokyo with a Doctor of Science. The title of my dissertation was “An investigation on the structure of typhoon.” Meantime, I was told that Dr. Gambo would be returning home from Princeton. Thus, I had to start to look for my future as I must return the position of Research Associate to Dr. Gambo. When I mentioned my situation to Prof. K. Hidaka, a noted oceanographer, he suggested that I consider visiting the Department of Oceanography and Meteorology, Texas A & M University. Earlier Prof. Hidaka was a visiting professor there and was familiar with their willingness to accommodate a visitor from Japan. So I wrote to Prof. John Freeman and mentioned my interest to visit him. John was one of the earlier members of the Princeton group on numerical weather prediction. He then gave me an invitation to visit the Department for one year with a possibility of extension. I thought that it would be great to spend a year or two there and I gladly accepted his offer.

In May 1954, I left Japan as a passenger in a cargo boat. There were 12 passengers on board and we ate in the dining hall with the captain, purser, engineers and medical doctor. The food was wonderful and the sea was mostly calm with occasional showers. After two weeks of crossing the Pacific and a short stop at San Francisco, I landed at Los Angeles. I spent few days at the home of our acquaintances. They were so kind to ask me where I wanted to go sightseeing during the stay. So, I asked them to take me to the Department of Meteorology of UCLA. Thinking back in those days I must have been very naïve. I started to walk in the corridor of the Department building and saw the doors of the professors’ offices which were all open. So, I dropped in the office of the famous Prof. J. Bjerknes who was willing to meet me! I said in effect that I would like to learn from him how I should study as I want to be a meteorologist. Then he just smiled at me and said that “I am not a person as you think I am” then went into a long silence. Since my conversational English was poor, I didn’t know what to do and decided to leave after thanking him.

The funny thing was that apparently I was not very much deterred by this embarrassing experience. Soon I saw Prof. J. Holmboe’s office and decided to visit him. After a little conversation, mentioning that I studied his text book, he noticed that I was interested in dynamic meteorology and offered to talk to me about what he was working on. Then, he closed his office door and began writing from the top left corner of his blackboard to its right bottom and filled the board with equations after equations without notes. I was really flabbergasted as I hadn't seen such a marvelous lecture performance before. Moreover, he did it just for me! I believe that he talked
about the stability of disturbances in a zonal flow with vertical shear and I was able to ask him occasionally for clarification as I understood his main results. He was rather pleased by my questions and asked me about my future plans. Then, he promised me a position in the Department to work with him after my one-year visit to Texas A & M. What a surprise! Of course, I accepted his offer with a great pleasure and left the Department. Figure 1 shows that Prof. Holmboe and I posed in front of the building of Department of Meteorology, UCLA in 1954. I had no idea then that the meeting with him had set the future course of my life.

I flew from LA to Dallas then took a train to College Station and began to work in a research project managed by John Freeman and Walter Saucier, studying the trajectories of high-level free-floating balloons in the jet stream. About half way through my one-year stay, they asked me what my future plans would be. I told them that I would join UCLA to work with Prof. Holmboe, mentioning my meeting with him at UCLA. A few days later, I received an offer to be appointed immediately to a full time position (my initial appointment was on a half time fellowship) as a visiting scholar with an arrangement for Yuko to obtain her entry visa to join me. Since the proposal was so attractive, I decided to take their offer. In fact, a few months later, Yuko was able to come to College Station. I explained to Prof. Holmboe about my irresistible reasoning for not joining his project. He graciously understood my situation and gave me his good wishes. Thus, my career began at College Station in the U.S.

In January 1956, I attended the 143rd annual meeting of the American Meteorological Society (AMS) in New York to make my presentation based on the balloon trajectory work I had done with Guy Franceschini and John Freeman at Texas A & M. Among many talks I listened to there was the presentation by Norman Phillips (1956) on his famous general circulation experiment. Norman’s talk gave me a lasting impression on me and I wished to work someday on the development of a general circulation model, which became a reality later when I joined NCAR in 1963.
3. Numerical prediction of hurricane movement and development

Just prior to attending the AMS meeting in January 1956, I was invited to attend a conference at the U. S. Weather Bureau (USWB) in Washington, D.C. During 1954-56 major hurricanes hit the U.S. mainland. The U.S. Congress urged the Weather Bureau to step up research on forecasting of hurricanes and the Weather Bureau had a series of meetings in order to take action on the request of the Congress. For example, the National Hurricane Research Program was set up in Miami, FL under the directorship of Robert Simpson. The purpose of this conference I attended at the Weather Bureau was to ask people working in the field of numerical weather prediction (NWP) how they can apply the newly developed NWP techniques to improve the hurricane predictions. I remember among the participants there were Jule Charney, George Platzman, Herbert Riehl, Robert Simpson, Lester Hubert and Robert Gentry. I believe that the meeting was arranged by Harry Wexler, Director of Research at the USWB. It was the first time for me to meet with so many of these famous scientists. So, I was very excited and expressed my desire to apply the NWP techniques to hurricane research as I had worked on both hurricanes and NWP before coming to the U.S.

One action item from the discussion meeting was that George W. Platzman of University of Chicago received a research contract from the USWB. Then, Platzman asked me to join his Department to work on hurricane research. I was naturally very anxious to accept his invitation, but I was somewhat uneasy about leaving College Station so soon after accepting a new position at Texas A&M. Anyway, I talked with John Freeman and the Department head, Dale Leipper, about my dilemma. It turned out that they were willing to let me accept Platzman’s offer because they didn’t want to disappoint Platzman by holding me back. They felt that they owed something to Platzman as their teacher. I found out that many of the Department staff graduated from the University of Chicago. I was very grateful for their consent and good wishes, and I felt obliged to do something for them someday. In fact, my wish became reality much later when I had an opportunity to serve as an Affiliate Professor of Meteorology at the Department of Meteorology, Texas A & M University during 1966 - 1968 and I taught a course on dynamic meteorology during the fall semester of 1967.

During the next 6 years, beginning from April 1956, I had a great time working on Platzman’s research project on numerical prediction of hurricanes in the Department of Meteorology. First I engaged in research to predict the movement of hurricanes and later to explore the mechanism of hurricane development using numerical methods. The other staff members of the Platzman’s project were Gene (Edward) Birchfield and Robert Jones. Both Birchfield and Jones were developing what is called today a fine-mesh model
approach, while I was using a steering flow technique. Ferdinand Baer was also a staff member of the Platzman’s project, but he worked on the development of spectral method to solve the barotropic vorticity equation over the sphere.

One of my interests to come to the U.S. was to have an opportunity to engage in research on NWP using electronic computers. The first electronic computer used for operational forecasting in the U.S. was the IBM 701 installed in 1954 in the Federal Office Building No. 4 in Suitland, MD and was operated by the Joint Numerical Weather Prediction Unit (JNWP). The computer was also used for the Weather Bureau research through the General Circulation Research Section (GCRS) of the then U.S. Weather Bureau which was also located in the same Federal Office Building. Since the Platzman’s project was funded by the U.S. Weather Bureau, Platzman made an arrangement with Joe Smagorinsky, Director of GCRS for the staff of Platzman’s project to use the JNWP’s electronic computer.

During my stay in Chicago until January of 1962, I often visited Suitland to use the IBM 701 and later IBM704. While I was running my job on the computer, I had many enlightening conversations with Joe and his staff, notably Suki Manabe who was a PhD graduate from the Geophysical Institute, University of Tokyo and joined the group in 1958. They were all working to develop a general circulation model (GCM) of the atmosphere and I wished to work on GCM someday. In fact, I knew that Lewis F. Richardson wrote a book on a numerical model for weather prediction and I wanted to read his book. So, I wrote the publisher, Cambridge at the University Press, asking to purchase a copy of the book. I was not sure that a copy was still available, since it was published in 1922. However, to my surprise the publisher sent me a copy! I studied the book rather carefully and I found that his dynamical formulation was very sound, though many physical processes and numerical methods that he discussed needed to be replaced by more recent knowledge.

Because I am a research-type person, I was not thinking about applying the prediction scheme of hurricane trajectory we developed for operational forecasts. Platzman persuaded me to ask Lester Hubert of the U.S. Weather Bureau to test our scheme on an operational basis. We gave him a computer deck to run at the JNWP and he prepared the input data. One day I got a telephone call from Hubert that he dropped the program deck while he was trying to load it on the card reader. It was a binary deck and there was no serial number on the punched cards, so he was unable to straighten out the deck. We ended up sending him a new deck by an express mail service. I believe that Hubert’s test was the first attempt to operationally forecast the movement of hurricanes using an electronic computer. See Hubert (1959).
I felt that, as far as the prediction of hurricane movement is concerned, we have tried all the approaches that were feasible at that time and I realized that to improve the prediction further we need to improve the prediction of large-scale steering flows in which tropical cyclones are embedded. This was indeed the problem of operational numerical weather prediction (NWP) and its further improvement was necessary not only for the prediction model, but also the data analysis procedure. Since my interest was research on tropical cyclones, I decided to investigate the mechanism of formation of the tropical cyclones and how they develop into hurricanes or typhoons. Actually that was a topic of research I was working on with Prof. Syōno at the Geophysical Institute before coming to the U.S.

I formulated an axisymmetric numerical model of tropical cyclone to calculate how a weak depression would intensify with the latent heat of condensation released in a conditionally unstable environment. However, in the numerical model instead of getting the formation of tropical cyclone, grid-scale cumulus cells developed and dominated over the initial weak cyclonic circulation. Anyway, I presented this finding at the first International Conference on numerical weather prediction (NWP) held in Tokyo in November 1960. Incidentally, subsequent international NWP meetings were held in Oslo in 1962 and Moscow in 1964.

The first NWP conference was a very unique one in the history of Japan Meteorological Society. Because this was the first International Conference on NWP, almost all the scientists in the world who were working in the field of NWP attended and the conference was very successful. Figure 2 shows a photograph taken at a dinner party during the Conference at a Japanese restaurant in Tokyo. Although Prof. Syōno and many other participants were missing, one will get an idea of a rather unique atmosphere of congeniality among foreign participants. Prof. Syōno spent extraordinary efforts to organize this Conference. It seemed that he had two intentions for having this meeting in Japan. One was to show off the progress made in Japan on the development of NWP. In fact, in 1959 an operational NWP was started at the Japan Meteorological Agency with the IBM 704. The other was to provide an opportunity that young meteorologists in Japan could meet personally with the world-class scientists working on NWP and thereby create chances for Japanese scientists to visit abroad. Syōno went through a great deal of hardship during the war and was unable to concentrate on his professional activity. So he didn’t want the young scientists to experience the same thing. Of course, I was already in Chicago then and I was fortunate to attend the meeting with George Platzman. The families of both Yuko and me were very pleased to see us as this was our first visit to Japan after six years of absence since we left Japan in 1954. Another joy for our families was to meet with a new member of our family, Alice, who was born in Chicago in
1958. Figure 3 was taken on the University of Chicago campus when Alice was less than a year old. I don't remember at what occasion we all met with George Platzman and Robert Simpson (far right) who was the Director of National Hurricane Research Project at that time.

I now return to the subject of numerical experiment on the formation of tropical cyclones and my struggle with the simulation of hurricane development. It was clear to me that something had to be done to control an excessive development of convective cells in the model. Convective cells are present in the tropics and there is nothing wrong with having convective cells in the model when the model state becomes conditionally unstable. While working at the University of Chicago and listening to the intense discussions between Herbert Riehl and Joanne Simpson and reading their articles, I was fully aware of the importance of deep convective systems, called hot towers, in the hurricane formation process. The question, however, was how to deal with the sub-grid scale activity which provides the energy source of tropical cyclones. Since I have written an essay entitled “On the origin of cumulus parameterization for numerical prediction models” (#84, 2000) to deal with this problem, I will not repeat its history here further.
4. My acceptance of appointments at NCAR and Courant Institute at the same time

In the fall of 1961, I was invited by Phil Thompson to join a newly created research Institute, the National Center for Atmospheric Research (NCAR). So, I visited Boulder, CO in the early December, 1961. There, I met with Walter Orr Roberts, Director; Thompson, Associate Director; and Aksel Wiin-Nielsen, who was in charge of the program called “Dynamical Aspects of Atmospheric Circulation.” Walter and Phil explained me the future plan of NCAR as described in the famous “Blue Book” which, I thought, was very impressive. Phil and Aksel even took time to give me a ride outside of the city from where they could point out the future building site of NCAR on top of a mesa and talked enthusiastically the Walter's vision on the work environment in which research scientists should engage. Phil's description reminded me a passage from the book of Richardson (1922, p. 220) that "Outside are playing fields, houses, mountains, and lakes, for it was thought that those who compute the weather should breathe of it freely."

Thompson’s offer was attractive. He said that I could work on any topic I wanted. However, I was very concerned, because there were practically no research facilities and only a handful of scientists in the office located at the old armory building behind Macky Auditorium on the University of Colorado campus. During my stay in Chicago, I had to travel to Washington, D.C. many times for extended periods to run my programs on electronic computers. It was stressful to check out computer programs in a limited time. So, I didn’t want to move to an organization where no supercomputers were available on site. I talked with Phil frankly that I had been considering an offer from the Courant Institute of Mathematical Sciences (CIMS), New York University (as described next) and mentioned my interest to join the Institute. However, I said to him that since I am not a mathematician, I have no intention to stay there too long and I would like to come back to NCAR a year or two later. So, I had accepted Thompson’s offer and decided to join CIMS in the meantime. I may be the only employee of NCAR who accepted a position and then immediately took a leave of absence (as described in the NCAR Reports of Research and Facilities Programs - 1962, on page 46).

One and half-year later, during the summer of 1963 our family drove to Boulder from New York.
5. Numerical studies of mid-latitude frontal cyclones

During my Chicago days, I was working on the numerical prediction of hurricanes. I had many conversations with George Morikawa of the Courant Institute of Mathematical Sciences, who was also working on the prediction of hurricane movement using the idea of a point vortex. When I mentioned to him that I had a job offer from NCAR, he immediately talked to James Stoker, Director of CIMS about my availability. Stoker was familiar with the development of numerical weather prediction through the activity of Von Neumann’s weather prediction project at Princeton’s Institute for Advanced Study. He recognized that the numerical weather prediction was an emerging field of applied mathematics and he himself was interested in atmospheric dynamics. In his book on “Water Waves”, Stocker (1957) discussed his idea on the life cycle of extra-tropical frontal cyclones. Since Stocker had been looking for an atmospheric modeler in his research staff, he immediately offered me a position and I gladly accepted it.

When I joined CIMS in January of 1962, Stoker suggested that I work on a numerical study of frontal motion. Stocker speculated that the cold front must move faster than the warm front so that the frontal configuration eventually leads to an occlusion. He wanted me to verify this development through the numerical calculation of a shallow water (cold) layer underneath a warm layer.

A special problem arises due to the fact that the cold air layer intersects the ground. Thus, the cold air domain has a free boundary along which the movement of the air parcels must be calculated. I discussed this problem with Eugene Isaccson who was my advisor at the Institute. He suggested that I talk with Robert Richtmyer, who was at that time Director of the Computing and Applied Mathematics Center of the Institute. Richtmyer suggested the use of a shock fitting technique that he developed while working with John von Neumann on shock wave problems. The numerical method worked satisfactorily and we were able to demonstrate that Stoker’s hypothesis was essentially correct in explaining the occlusion process of frontal cyclones. The results were published jointly with Isaccson and Stoker in *Tellus* (#19, 1965).

The most memorable family event while we lived in Manhattan, NY was that our second daughter, Margaret, was born in Columbia Presbyterian Hospital. When we left Manhattan, Margaret was just less than year old and Alice was five. We converted the back seat of our 1957 Chevy to a playpen for them and drove from New York to Boulder to join NCAR in the summer of 1963.
6. Development of the NCAR general circulation model

Soon after I joined NCAR, Warren Washington, who also joined NCAR about the same time after receiving a Ph.D. from Pennsylvania State University, talked to me about his interest to work on a general circulation model (GCM). I said to him that “It is great if we can work together. Let us ask Phil about it.” So, we told Phil about our plan and asked his advice during our lunch meeting at the Lamp Post restaurant in Boulder. Phil was delighted to hear our plan and gave us his strong endorsement. He then raised his eyebrows a couple times, a sign of his approval. That was it and we finished lunch.

Actually, Phil had been working then on a statistical-dynamical model of the atmosphere which describes a long-term fluctuation of zonally and vertically averaged flow patterns. Warren was working on a numerical scheme for solving Phil’s model and using the Control Data Corporation (CDC) 3600. Warren and I were interested in developing a full-blown GCM to complement Phil’s work.

After reviewing several GCMs developed at MIT, the General Circulation Research Section of the US Weather Bureau, UCLA, and Lawrence Radiation Lab, Warren and I set the design principle that “it should be sufficiently general that a number of scientists can incorporate their findings in the model and test their hypotheses” (Reports of Research and Facilities Programs, 1964, NCAR). Since we were relatively late-comers in the GCM arena, we felt we should offer something different from other models, namely “to make the model available to the atmospheric science community as a facility for numerical experimentation, as well as to fulfill a need at NCAR for studying the large-scale dynamics of the atmosphere” (see Preface of “Development and Use of the NCAR GCM”, NCAR Tech. Note, 1975). This philosophy carried forward many years into the future as NCAR pioneered the Community Climate Model and later the Community Climate System Model.

One unique feature of our early GCM was that we followed closely the dynamical formulation of the model that L. F. Richardson (1922) had described. I knew about the Richardson’s work by reading a copy of his book which I got during my Chicago days and I was very curious about the so-called “Richardson’s failure” that many modelers mentioned. In fact, I could not see anything wrong with what Richardson had intended concerning his dynamical formulation using the geometric height as the vertical coordinates.
One dynamical modification that we had to make was to place the top of the model at a finite height, while Richardson placed the model top at infinity. Of course, the numerical method for time integration of our equations had to be developed. Thus, a great deal of attention was given in those days to develop numerical schemes for our model. In this connection, David Houghton, David Williamson, and Gerald Browning worked on various aspects of improving numerical integration methods. Likewise, we had to develop all the physical parameterizations, including the radiation package designed by Takashi Sasamori. The early history of the NCAR GCM and the specifications of the first, second, and third generation models are described in detail in an NCAR Technical Report entitled “Development and Use of the NCAR GCM” (1975).

As an additional note to the early history of GCM development at NCAR, I should mention that Warren made a significant contribution by introducing to the NCAR Computing Facility a graphic display unit, called DD80 for debugging of GCM computer programs. Such a visual display device had been used by Cecil (Chuck) Leith at Lawrence Livermore Lab, who made the computer movie of his successful moist GCM simulations in the early 1960s. Use of such a device greatly speeded up our GCM development. Of course, computer software had to be developed to fully utilize the capabilities of DD80. Bernard O’Lear’s coding work set the pathway towards development of the popular NCAR graphic package.

Historical accounts leading to the development of NCAR general circulation model (GCM) and the earlier efforts to develop GCMs at various organizations are presented in an article that Warren Washington and I wrote as Chapter 3 in the book of “The development of atmospheric general circulation models”, edited by L. Donner, W. Schubert, and R. Somerville, published by Cambridge (#96, 2011).
7. Dynamical effects of mountain on atmospheric flows and visits to the USSR

The GCM development was a time-consuming tedious task that took a long time to get some results worthwhile to report. So, I soon realized that I should work on a simpler problem in parallel with the GCM development that can produce reportable results fairly quickly. One thing I noticed living in Boulder is that we are struck from time to time during winter and spring by strong (often hurricane force) and relatively warm winds that suddenly flow down from the Rockies, the phenomenon called the chinook. I thought that the phenomenon is very similar to the hydraulic jump that develops downstream from an obstacle in a river. I remember that I read an article by Robert Long in *Tellus* who described laboratory experiments on the shallow water flow over an obstacle. Because David Houghton and I were testing the Lax and Wendroff (L-W) time-integration scheme for GCM application, we thought that it would be interesting to apply the L-W scheme, which is suited to handle discontinuous solutions, to numerically simulate chinook events. The problem was to find the numerical solutions of one-dimensional nonlinear shallow water flow over an obstacle. We displayed graphs of the height and velocity of the flow in each time step on each camera frame using DD80. We then ran the frames like movie. It was an excellent way to see the results graphically and we could see the results of different cases very quickly. I was impressed by the scientific value of computer graphics in movie form. (Later, Warren and I made many computer movies of our GCM results.)

By looking at the computer outputs, we soon realized that the steady-state solutions are eventually established over an isolated ridge after the flow generated disturbances over the ridge moved away downstream. There are various steady-state conditions which are expressed in algebraic conservation equations. In fact, there are ten unknown variables, but we could find only nine steady-state equations. What was missing and how could we find the missing condition? We pondered on these questions for some time. In the beginning of 1966, David Houghton had a chance to visit Russia for three months. He had an opportunity to talk with Oleg Vasiliev, Director of the Institute of Hydrodynamics, Novosibirsk, who suggested that the missing equation is the rarefaction condition. Luckily, his suggestion enabled us to complete the nonlinear solutions of the one-dimensional shallow water flow over an isolated ridge. We talked about this problem with Eugene Isaacson of Courant Institute, who was visiting NCAR. With his assistance our work was later published in *Communications on Pure and Applied Math* (#28, 1968).
Speaking of Houghton’s visit to Russia in 1966, I should mention that I visited Novosibirsk in 1965, a year earlier than David’s visit. In those days, Russia was still called USSR and travel to USSR was restricted. However, both US and USSR scientists were eager to collaborate in connection with an international effort, which was later called GARP (Global Atmospheric Research Program), to respond to the WMO (World Meteorological Organization)’s resolution to develop an international program to expand global meteorological observations and advance research for improved weather forecasting.

I was invited to attend the International Symposium on Dynamics of Large-Scale Processes in the Atmosphere in Moscow in June 1965, organized by A. M. Obukhov and Norman Phillips. Earlier, when I met with Guri I. Marchuk, Director of Computing Center, Novosibirsk, USSR, who visited NCAR in 1963, he encouraged me to visit him in Novosibirsk. So, I thought it would be interesting to visit him in Novosibirsk too during my travel to USSR. In fact, I decided why not take this occasion to visit prominent scientists working on numerical weather prediction in Europe as my first European trip. From June 18 to July 13, 1965, I made my first trip to USSR (Novosibirsk and Moscow) and Europe (Helsinki, Oslo, Stockholm, Hamburg, and London).

The USSR Academy’s Computing Center was in a relatively small academic town which was located about 30 miles northwest of Novosibirsk (a major city in Siberia). There are several government research organizations there, similar to Boulder as a suburb of a major city in the west, Denver. I was interested in this geographical similarity between Academic Town and Boulder. However, I didn’t realize how difficult it would be to make the arrangement to travel to Siberia, because travel to Siberia was not open to general tourists in those days and special arrangements were needed to fly by Aeroflot to Novosibirsk. Guri Marchuk kindly arranged for me to meet an Academy representative at Leningrad airport on June 19, 1965 who would assist me to fly to Novosibirsk.

I left Denver on June 18, 1965 by a flight to Chicago where I was scheduled to catch the flight to London and then fly to Leningrad via Helsinki. When I landed in Chicago, I found that the scheduled flight to London was going to be delayed. That means, I thought, that I would miss meeting with the Academy representative at Leningrad. I explained my problem with the airline agent who suggested that I switch to another flight to go to London. By taking this alternate flight to London, I was able to meet with the Academy representative at Leningrad who helped me to take a flight to go to Novosibirsk via Moscow. At the Novosibirsk airport I was greeted by Guri Marchuk around 4 a.m. in the morning who took me to Academic Town. I noticed that my luggage didn’t arrive, but I thought that it would come with
the next flight. It turned out this was a beginning of my ordeal to locate my missing luggage for the next five days.

After my visit to Academic Town I flew back to Moscow to attend the International Symposium on Dynamics of Large-Scale Processes in the Atmosphere. I started to phone the airports where I went through to locate my luggage, but I didn’t have much success. Meanwhile, I tried to buy some clothing at the stores. I could buy a few things, but I couldn’t find a white shirt in my size. When I mentioned this problem to K. Gambo who was my senior colleague at University of Tokyo and was also attending the same meeting, he kindly gave me his extra shirt. After several days of struggle, I finally found my luggage which was sitting all the time at the Moscow airport. Apparently, when I switched flights to London at Chicago, there was no time to get my luggage transferred and the luggage traveled by itself to Moscow and stayed there.

In spite of the logistic difficulty in getting my luggage, my travel to Novosibirsk and Moscow was very successful. I had some knowledge of Russian in those days by taking a Russian language course offered by NCAR which turned out to be extremely useful. For example, for going from Leningrad to Novosibirsk via Moscow I had to take a taxi from the international Moscow airport to the domestic airport. Once I got to the domestic airport, I couldn’t find anyone who spoke English to find the gate to fly to Novosibirsk and found that a little bit of my Russian was very helpful.

In parallel with my collaboration with David Houghton, I was also working on the effect of orography for the large-scale flows using the two-dimensional shallow water model including the effect of Earth’s rotation. During my Chicago days, I learned from Dave Fultz of the Hydrodynamics Laboratory that westerly flows past over an isolated mountain produce a train of long waves on the lee side, which can be interpreted as “planetary” waves. In contrast, easterly flows are little disturbed by the obstacle and long waves are not produced. I was curious about this contrast in the flow regimes. To my amazement, the numerical solutions indeed reproduced these flow features and I was able to explain these differences from a theoretical analysis of the planetary vorticity equation. In fact, the number of waves produced in the westerly cases agreed with that obtained from the steady-state Rossby-Haurwitz wave formula for various intensities of flow past the obstacle. I presented the results at the Moscow symposium as well as the Novosibirsk meeting and later published in *J. Atmos. Sci.* (#21, 1966).

One unforgettable episode I experienced at the Novosibirsk meeting was that I was one of the three speakers together with Robert Richtmyer of the
Courant Institute, who I met earlier in New York, and Sir James Lighthill of University of London. Actually, I was overwhelmed by being among these prominent scientists, but what was shocking to me was that they presented their talks in Russian, while I had to get the help of interpreter for my English presentation. This experience gave me a lesson that I should at least prepare the slides in Russian, even though I would be presenting my talk in English next time I visit Russia. In fact, I traveled to Novosibirsk Academic Center again in August, 1969 to attend the conference on “Application of numerical methods in modern gasdynamics”. I presented a talk on the computer simulation of Earth’s atmosphere and the results obtained from the NCAR general circulation model. Although I presented my talk in English, I had the slides prepared in Russian. On the way back to the U.S., since I went through Moscow again, I visited the State University of Moscow, the Computing Center for the Academy of Sciences, USSR and the Institute of Oceanology.

Another impression from my first USSR travel in 1965 was that I felt rather insecure during my stay in USSR, even though our Russian colleagues did everything to please me. Perhaps my psyche was influenced by Cold War hysteria in those days and by my experience of receiving a phone call during midnight in the hotel room where I was staying. When I answered the phone, I didn't hear any voice but clearly someone was listening. It could have been a wrong call or someone was just checking my presence. I thought about where I should go to get help if I got in trouble. If I went to the American Embassy, they might say that I should go to the Japanese Embassy since I had my passport issued from Japan. If I went to the Japanese Embassy, they might say that I should get help from U.S. since I live permanently in Colorado and had a legitimate job. The solution, I thought, was to get U.S. citizenship and travel with a U.S. passport, since I was expecting to make more foreign trips in connection with my participation in the GARP activities (see Section 19). So I applied for my U.S. citizenship which was granted in May 1969. And in fact, I didn't feel insecure at all during my second Russian trip in August 1969, traveling with my US passport.
8. Instability of mid-latitude frontal motions in the atmosphere

Right after I joined NCAR in the summer of 1963, I was still continuing the numerical work that I started at the Courant Institute, namely the development of cyclones on mid-latitude fronts. The model we used at that time was a two-layer shallow water model, but we assumed that the motion of the upper layer (corresponding to warm air) was stationary and only the motion of the lower layer (cold air) was transient. As an extension of this work, I decided to formulate a new model in which motions of both layers were treated as transient. When I ran this new model, it kept blowing up. I looked and looked to find bugs in the code that would cause numerical instability.

One day a thought flashed across my mind that the cause of the model blow-up was not due to numerical bugs, but that I was dealing with a problem of physical instability. Since there is shear in the zonal flows between the upper and lower layers, it is possible to produce unstable motions for short waves caused by an instability mechanism something like the Kelvin-Helmholtz instability. Actually the numerical model was very similar to the model that Erik Eliassen had studied. He published a report from the Danish Meteorological Institute in 1960 entitled “On the initial development of frontal waves”. Eliassen (1960) investigated in detail the instability property of frontal motions on a discontinuous surface between two homogeneous layers of different densities. He found that under polar front conditions small-amplitude perturbations can become unstable. The maximum instability occurs for wavelengths of ~ 2000 km with the growth rates on the order of one day. However, Eliassen didn’t mention the possibility of small-scale instability with much faster growth rates. I was wondering why he didn’t get it in his model.

So I decided to investigate the instability property of the two-layer model which has a free surface at the top instead of a rigid level surface adopted by Eliassen. Otherwise, the mathematical formulations of the two models were identical. In the late 1960’s when I started to work on this problem, I was very busy working on development of the NCAR GCM with Warren Washington. So I asked my colleague, D. B. Rao, who just received a Ph.D. under George Platzman at the University of Chicago and was visiting NCAR at that time, to work with me to solve this problem. Because this is a non-geostrophic problem and the quasi-geostrophic assumption should not be used, the mathematical formulation becomes a non-standard matrix problem which involves quadratic eigenvalues. We showed the formulation of this eigenvalue problem to John Gary who was a numerical analyst and serving as head of the NCAR Computing Facility. We learned that John had
developed a computer code to solve such a non-standard eigenvalue problem based on a Laguerre algorithm.

From our calculations using John’s code, we noticed that a different kind of instability exists at shorter wavelengths distinct from that found by Eliasen. A possibility of wave instability at shorter wavelengths in a two-layer frontal model had been discussed by Solberg (1928) who studied the stability property of wave motions at an inclined surface separating two layers of fluid having different densities. Bjerknes and Godske (1936) mention that there exist two types of unstable waves, one at a wavelength of the order of 2000 km and another at a much shorter wavelength.

The nature of the wave instability at wavelengths on the order of 2000 km appears to be the quasi-geostrophic baroclinic instability that was analyzed by Charney (1947) and Eady (1949). In fact, Riis (1956) demonstrated that the application of the quasi-geostrophic approximation in a two-layer model similar to Solberg’s alters the growth rate of long wave instability very little. The character of instability of long waves turned out to be similar to the one discussed by Phillips (1951).

The nature of wave instability at short wavelengths in a two-layer model was investigated in detail by Orlanski (1968). In his study he adopted the model originally considered by Kotschin (1932). In Kotschin's model the upper and lower boundaries are level surfaces and the interface between the cold and warm layers extends from the lower boundary to the upper boundary instead of intersecting the northern wall as in the Eliasen's and our models.

Orlanski found that in the case of very steep interface the instability at short wavelengths is attributed to the presence of large zonal wind shear between the warm and cold layers and is likely interpreted as the shearing instability of the Kelvin-Helmholtz type (Chandrasekhar, 1961). We ran calculations with our frontal model in wider parameter ranges than those in the Eliasen and Orlanski models. The results we found were indeed very complex. We obviously noticed the presence of two instability mechanisms of the quasi-geostrophic type for long waves and the Kelvin-Helmholtz type for short waves. In addition, it seemed that there existed another instability mechanism in between for medium-scale frontal waves.

Numerical works are great in exploring all sorts of cases and in finding interesting results, but when we try to understand the phenomena it is rather difficult to interpret. In this connection, we realized that the analysis of Stone (1966) was instructive. Stone considered the baroclinic model of Eady (1949) and investigated the property of instability without imposing the quasi-geostrophic approximation. Stone applied various approximations
valid to particular situations in the parameter ranges and obtained the analytical solutions to discuss the instability property. By examination of all of the results from the Stone’s model, which is continuous, and those from the frontal models of Eliasen and Orlanski and ours which are discrete, we could obtain valuable insights into the development mechanism of extratropical cyclones. More details are described in an article published in *Jour. Atmos. Sci.* (#41, 1972). However, a complete understanding of the non-geostrophic instability in shear flow is a difficult mathematical problem which has not been solved satisfactorily even today. It is perhaps best to investigate solutions of the generalized Eady model (without making the quasi-geostrophic assumption) in detail for a particular parameter range based on a specific synoptic situation on hand coupled with a numerical model experiment.
9. One-year visit to the Department of Meteorology, University of Stockholm

In January 1971 I received an invitation from Bert Bolin, University of Stockholm to visit the Institute of Meteorology, often called Rossby Institute, for nine months. Since my initial effort to develop the NCAR general circulation model (GCM) had reached a plateau as the GCM group was well established and Warren Washington was taking more responsibility in carrying out the further development, I felt that this was a good time for me to take a sabbatical leave from NCAR to make personal contacts with the scientists in Europe, and to explore a new area of research. After talking with my family and receiving approval from the Director of NCAR, John Firor, I accepted the invitation.

We left Boulder on August 19, 1971 and flew to Amsterdam. There, we picked up a four door 1971 Volvo which we ordered beforehand through a company called “Europe by Car”, Inc. So, we immediately drove the car from the Amsterdam airport. It was quite an experience for me to drive a new car in foreign countries where traveling distances are much shorter than those in the U.S. and cities were jam packed with people, cars and sometimes bicycles.

After spending over two weeks on the road starting from Amsterdam, we arrived at Stockholm on September 5, 1971 and we immediately went to the visitor housing quarter, called Wenner Gren Center. Our two daughters, Alice and Margaret, kept a diary during our trip. According to their record, we drove through Cologne (Köln), Bonn, Boppard, Frankfurt along the Rhine river, and then southward to Heidelberg and Baden-Baden. After that we drove southeastward to Ulm (west of Munich) and took the picturesque Romantic road northward to Bad Mergenheim (near Würzburg). We then went northward to Göttingen, Hamburg and to Lübeck. There, we left Germany and entered into Denmark. After spending some time in Copenhagen, we entered into Sweden and stayed overnight at Jonkoping. The next day we arrived in Stockholm.

It was an interesting trip, though the driving was strenuous because of narrow unfamiliar streets in the cities and speeding drivers on the autobahns. We enjoyed particularly our traveling along the Rhine and the Romantic Road through many old tiny cities. We also had time to meet people I know in Europe. For example, we visited Frank Schmidt at University of Bonn who was a dynamicist and had spent some time at NCAR. We met also with Peter Fabian and his family in Göttingen. Peter was an atmospheric chemist in Max Plank Institute and once I worked with him on modeling of the circulation of Venus together with Takashi Sasamori in the
late 1960’s. Although we enjoyed our travel, all of my family caught colds one after another during the travel and I was the last one to get it. As soon as we settled in our apartment in Stockholm, I had to stay in bed for a few days. Later, I learned from the staff of the Institute that they were wondering what had happened to us since I did not show up at the Institute as scheduled.

Our apartment in the Wenner Gren Center was located in the northern end of the city of Stockholm. The rooms were well furnished and comfortable. Both Alice and Margaret attended the Anglo-American School in the downtown. They liked the school and commuted by city bus. I walked to the Institute since it was located near the Wenner Gren Center.

The first work I did at the Institute was to write up the manuscript on the stability problem of frontal cyclones that I was working on with D.B. Rao at NCAR. I brought all the computer results with me and I was happy to finally finish this work. This joint work was published in JAS (#41, 1972). The next thing I did was to give a series of seminars on the NCAR general circulation model. Coincidentally, Joe Oliger who was working with us at NCAR on the numerical aspects of the NCAR GCM had been invited to visit the Mathematics Department of University of Uppsala. At the time I was visiting the Institute, Joe was writing with Heinz Kreiss of the University of Uppsala a monograph on numerical methods for atmospheric models as one of the GARP reports. I was involved in working for Research Coordination and Planning Group of Joint Organizing Committee (JOC) on real time data processing for GARP experiments in which I was a member and Bert Bolin was Chairman.

When I decided to take a sabbatical leave to visit University of Stockholm, I thought that I should take advantage of the long winter nights by reading some classics which I always wanted to study. I brought with me two such voluminous papers: One was a two-part article by S. Hough (1898) published in the 19th century and other was a contemporary article by Longuet-Higgins (1968). Both articles are concerned with how to obtain the normal mode solutions of Laplace’s tidal equations on a sphere which correspond to the eigensolutions of the horizontal structure equations of the primitive equation prediction model. I wanted to learn the basic properties of planetary scale waves and to construct the tools for representing observed global flows. Earlier, Flattery (1967, 1970) developed an operational objective analysis scheme for global data using the normal mode solutions of Laplace’s tidal equations over a sphere. Flattery was a Ph.D. student of George Platzman and his Ph. D. thesis topic was the construction of the global normal modes, known as the Hough functions. In those days, the Hough functions were something rather mysterious. Yet, they are important
to understanding the global dynamics. So, I wanted to understand them and to construct them by myself.

Among several classical papers on Hough functions, the paper by Longuet-Higgins (1968) was most prominent as he used an electronic computer to get the solutions. Naturally, I read his paper rather carefully from the beginning to the end. I was very impressed by the clarity of the article and I wanted to reproduce his solutions. The problem is to obtain the eigensolutions of a system of linear equations for 3 variables, the velocity components $u$ and $v$ and geopotential height $h$. I will write more about on this topic in the next section.

Our family took advantage of being in Europe and we traveled often. For example, during the winter break we signed up for a group package tour through a Swedish travel agent and visited London for one week in the beginning of January, 1972. Actually, we met the group only once at the Stockholm airport and after we arrived in London, we were all on our own and there was no organized activity. We stayed in a bed-and-breakfast kind of accommodation, which was very pleasant. We ate dinner at a specified restaurant and the menu changed daily. We went sightseeing every day in London by taking the Underground, called the “tube” and by a lot of walking. One day we took a train to visit Windsor Castle.

We also traveled by our car to Oslo and Dalarna during the spring break. Oslo of course is a major city in Norway. Although we noticed some difference in each country, all Scandinavian countries, including Denmark, are rather similar. Dalarna is said to be a Sweden’s well-known tourist place. The colorful costumes that local people wore were attractive. We bought as souvenirs dolls dressed in local costume and decorated wooden horses. Every time when I see these souvenirs in the show case in our dining room, they remind me of the pleasant memories of our family living in Sweden.

In May 1972 we signed up for a package tour to visit Moscow and Leningrad through a travel agent. Unlike the case of London tour this was a group activity with a Swedish guide. Because we were only non-Swedish speaking people among 20 or so members, the guide had to speak to us in English, but once we were sightseeing we often found Japanese tourist groups and listened their guides’ talking which was helpful. We went to usual sightseeing places such as Red Square, Kremlin, St. Basil’s Cathedral and so on, but we also had free time and we took the subway to go around the city. I remember that some of the subway stations had rather elaborate wall paintings which were impressive. We went also to see the Moscow circus and an opera in the Bolshoi theatre.
At Leningrad, now called St. Petersburg, the most magnificent place to see was the Winter Palace in the Hermitage Museum and if I remember correctly we spent a whole day at the Hermitage Museum. There were huge collections of art works including famous impressionist paintings. We also saw a ballet at a theater, but I don’t remember the name of the ballet group.

This family travel to Russia was my fourth visit there. In March 1972, just prior to our family trip, I attended a Study Group Conference in Leningrad which was organized by the Joint Organizing Committee (JOC) for GARP (Global Atmospheric Research Program). The theme of the Conference was “Parameterization of sub-grid scale processes”. I flew from Stockholm to Helsinki, Finland, with Bert Bolin and Hilding Sundqvist and we all took an over-night train from Helsinki which departed on the late evening of March 19, 1972 and it arrived at Leningrad on the following morning. During the morning tea time on the train, I heard a loud English voice that sounded familiar to me. To my surprise I met Doug Lilly and Chuck Leith on the same sleeping car! They had come directly from Boulder to attend the same meeting. Actually, Francis Bretherton and Jim Deardorff came from NCAR too as I met them during the meeting. Figure 4 is a group photo of participants of this Conference. I attended also a meeting of JOC Research Coordination and Planning Group on "Real Time Data processing for GARP experiments" which took place concurrently with the Parameterization meeting. Before coming to Stockholm, I was working on Observing Systems Simulation Experiments (OSSE) with Dave Williamson, Dave Baumhefner and Warren Washington. See, publications #38 (1971), #39 (1972), and #40 (1972).

So, I had a great time in staying 9 months in Stockholm and we all enjoyed living in Sweden, but now the time had come to make our journey home. According to the travel record kept by Alice and Margaret, we left Stockholm by our car on June 9, 1972 and traveled through various countries in Europe. Except for a hotel reservation at Amsterdam we had no reservation for accommodations during our travel of over one month. We drove a total of 8856 miles during our stay in Europe and arranged to ship our Volvo back to the U.S. We returned home on July 15, 1972 without any problem. It was a great trip in many respects.
10. Application of global normal modes for atmospheric analysis and prediction

While working on the general circulation model (GCM) development, I realized that it would be nice to have some tools for representing the flow patterns on the globe better than the spherical harmonics. Spherical harmonics have been used extensively for representing many geophysical quantities on the globe. However, they are not representative of atmospheric motions. In the case of global circulations we need to represent the flow patterns of at least the horizontal velocity components, u and v, and the geopotential height h. Therefore, it is desirable to have some vector functions to represent the wind and mass fields together rather than each separately. I was familiar with the work of atmospheric oscillations even before coming to the U.S. and I heard about the Hough functions as the eigenfunctions of Laplace’s tidal equations from George Platzman when I was in Chicago. While I was working on the GCM during the 1960’s, Dick Lindzen was at NCAR and working on the atmospheric tides. Again, I heard about the Hough functions, but I was too busy at that time to follow up his work. When the opportunity came in 1971 to visit the Meteorological Institute of Stockholm University, finally I was able to learn about the Hough functions as I described in the previous section.

During my stay in Sweden I developed a computer code to calculate the Hough functions using a simpler method than one used by Longuet-Higgins. I formulated the solutions of a system of linear equations for 3 variables, u and v for the horizontal velocity components and h for the geopotential height as a standard eigenvalue problem. I still remember my excitement when my results of calculations agreed with those of Longuet-Higgins. After returning from Stockholm, I continued to polish the matrix code of Hough functions at NCAR. Then, I started to work on deriving eigenfunctions in the vertical direction instead of empirical orthogonal functions as Tom Flattery did. Not knowing where I should start from, I followed the same approach as adopted by Wilkes, Siebert, Chapman and Lindzen for research on atmospheric tides. There is a notable difference between the problem of atmospheric tides and obtaining the normal modes of the atmosphere. In the case of atmospheric tides, we are interested in the response to forcing at particular wave frequencies. The normal modes are the free oscillations without forcing and there are many frequencies. In both cases the atmospheric system is separable so that we have two structure equations, one in the horizontal and the other in the vertical directions, but they are connected with a parameter called “equivalent height”.

In the case of atmospheric tides for a particular frequency, we first determine values of the equivalent height and associated horizontal
structure functions by solving the horizontal structure equation, specifying the frequency. Then the vertical structure equation, including the effect of dynamical and thermal forcing, is solved for the vertical structure functions corresponding to various values of the equivalent height. In the case of free oscillations the vertical structure equation is solved first to obtain values of the equivalent height and corresponding vertical structure functions. Then the horizontal structure equation is solved to determine the frequencies and associated horizontal structure functions corresponding to various values of the equivalent height.

For the hydrostatic atmosphere the largest value of the equivalent height is about 10 km and the free oscillation corresponding to this value is called the external mode. All others are smaller than 10 km and the oscillations are called internal modes. As a demonstration for the application of spectral expansion with the normal mode functions to real data, I used the height and wind data at 500 hPa from NMC (National Meteorological Center) Flattery analysis data for December 1972. This work was later published (#46, 1976). The vertical structure equation that I derived at that time was very primitive. So, I wanted to improve it, but I was occupied doing other things until Kamal Puri came to NCAR from Australia as a long-term visitor to collaborate with us. Kamal was interested in working with me and together we developed the method of spectral representation of 3-D global data by expansion in normal mode functions for an operational primitive equation forecast model. We demonstrated the utility of our expansion method using the Northern Hemisphere NMC daily data for the month of January 1977, including six internal modes together with the external mode (#57, 1981).

Since the normal mode (Hough) functions are eigenfunctions of the global shallow water equations, I thought that it is interesting to use the Hough functions for a prognostic (initial-value) problem as the expansion functions to perform time integrations of the nonlinear global shallow-water model just like using the spherical harmonics for solution of the nonlinear barotropic vorticity equation as Ferd Baer and George Platzman did. However, I encountered one puzzling problem in this endeavor. That was how to represent the zonal component of the global solutions by expanding them in terms of Hough functions. The case of wavenumber zero is special in that, while inertia-gravity modes are present, the rotational modes are nonexistent. So, I had to treat the time dependent solutions of the zonal component separately. However, except for this awkward treatment of the zonal part, the idea of using the Hough functions for solving the primitive equations worked out rather well as I compared the results with those obtained using a difference scheme (#48, 1977).
So, my task now was to create some vector expansion functions to represent the zonal flow for both velocity and height together that satisfy the Laplace tidal equations. How could I do this? It turns out that the Laplace tidal equations for steady zonal flow are reduced to the linear balance equation for zonal flow on the sphere which is a generalized form of the geostrophic equation. By solving the linear balance equation using a series of Legendre polynomials, I was able to construct a set of vector functions. The resulting functions, however, turned out to be not orthogonal. So, I had to transform them to a set of orthogonal functions using the Gram-Schmidt method. Furthermore, I needed to add a vector corresponding to a null wind field and a constant geopotential height to complete a function set which is orthogonal to expand the zonal wind and mass fields simultaneously. With this set of expansion functions, called “geostrophic modes”, I was able to formulate a clean Hough spectral model of the barotropic primitive equations over the sphere without a separate treatment for the zonal flow which I had to do in my previous attempt. So, I felt very good about it and published the article (#51, 1978) immediately. Let us call this type of geostrophic modes K-modes.

The need of expansion functions for zonal flows is not unique to a spherical problem. Joe Tribbia (1979) realized the same need in connection with his study on the normal mode initialization for the equatorial beta-plane shallow-water model and constructed a set of geostrophic modes based on the same procedure mentioned earlier. Meantime, a different approach to the construction of geostrophic modes for the equatorial beta-plane model was proposed by Pedro Silva Dias and Wayne Schubert (1979). They formulated an orthogonal set of geostrophic modes by taking the limit of the eigenfunctions of the rotational modes as the zonal wavenumber approaches to zero and applying L’Hôpital’s rule to derive the limit of eigenfunctions.

When Yosuke Shigehisa came to NCAR for one-year visit from Japan Meteorological Agency in the beginning of 1980’s, he asked me for a problem suitable to work on during his visit. So, I mentioned about the geostrophic modes as a hot topic and talked about the work of Silva Dias and Schubert for the equatorial beta-plane model. Then Shigehisa decided to find out what kind of geostrophic modes will come out by taking the limit of zonal wavenumber to zero for the Hough functions of rotational modes. Unlike the equatorial beta-plane model in which the zonal wavenumber is a real number, the zonal wavenumber in the spherical domain becomes an integer. Nevertheless, Shigehisa succeeded for the construction of geostrophic modes by taking the limit of the eigenfunctions of rotational modes treating the zonal wavenumber to be a continuous parameter and the ratio between the zonal wavenumber and the corresponding eigenfrequency to be finite. The latter condition ensures the phase speed remains finite.
Since a set of the geostrophic modes created by this procedure is different, we shall call the geostrophic modes thus created as the S-modes. This work was published by Shigehisa (1983) in the Jour. Meteor. Soc. Japan.

Now having two different sets of the geostrophic modes (K-modes and S-modes), I was curious about the mathematical nature of this problem and I talked with Paul Swarztrauber of the NCAR Computing Facility. He expressed a great interest in the problems of Hough functions including the question of geostrophic modes, as he had been working on the software to compute spherical harmonics. He even proposed to develop software for Hough functions for community use. I was delighted by Paul’s interest and we worked together to complete the software project (#65, 1985). As the geostrophic modes, Paul decided to adopt the Shigehisa's approach. He felt that the derivation of S-modes is mathematically consistent with the rest of the modes and the orthogonality of modes is preserved. Meanwhile, having the two sets of geostrophic modes, I was wondering which one is better to use.

Several years later, when I had an opportunity to collaborate with Hiroshi L. Tanaka who was, then, teaching at University of Alaska (later he moved to Institute of Geoscience, University of Tsukuba, Japan), I brought up to him the comparison of two types of the geostrophic modes and we decided to work together on this comparison. As described in our article (#76, 1992), there are merits on each type, but if one is interested in the question of which type can capture the majority of observed zonal energy with a smaller number of terms, the K- modes are better suited. The K-mode series converged faster than the S-mode series, especially for small vertical-scale components in the observed zonal fields. My interpretation on this finding is as follows: The S-modes have the characteristics of IG modes, while the K-modes are close to the rotational modes. Since the majority of planetary-scale energy reside in the rotational modes, it is advantageous to use the K-modes to represent the zonal flows.

Meanwhile, I started to examine various global dynamics problems using the Hough functions. One such problem was to understand the nature of traveling planetary waves at long periods of several days and longer as more observational data became available. One of the most well-known types is the wavenumber one disturbances which propagates westward with periods of about 5-days. Another regular transient wavenumber one propagates westward with periods of 16-20 days. Rol Madden (1978; 2007) noted that these westward propagating transient planetary waves are free oscillations of the external type with the equivalent height of 10 km. However, while a 5-day period agrees with that derived from the Laplace tidal equations, there is a discrepancy in periods between observed 16 days and “predicted” 12.3
days. Since the “predicted” period is derived from the theory without the background flow, I thought that it would be interesting to calculate normal modes of free oscillations including the effect of zonal flows and that the use of Hough harmonics as the basis functions for this purpose should work well. As discussed in the article (#53, 1980), it worked out indeed well and the results not only explain the discrepancy between the observed and “predicted” periods, but they also made it possible to understand the physics involved in the normal modes of a realistic atmosphere, at least in the framework of a barotropic atmosphere.

I would like to mention another global dynamics problem to which I applied the 3D Hough spectral expansion. It was to understand the dynamical mechanism of the so-called “teleconnection”. The notion that large positive anomalies of equatorial sea temperature give rise to remote influences on flow patterns at distant places in the world had been around for many years based on synoptic analyses of global weather changes by many researchers such as G. Walker, J. Bjerknes, and J. Namias. When equatorial waters are unusually warm, such as during El Niño, cumulus convection tends to stay over the warm water and releases anomalous latent heat of condensation. This heating thermally forces the atmosphere and generates planetary-scale waves that propagate into midlatitudes. This scenario of tropical-midlatitude teleconnection process had been well established based on a large number of observational studies such as by Horel and Wallace (1981) and many numerical model experiments to simulate the phenomena as done, for example, by Webster (1972), Hoskins and Karoly (1981) and Simmons (1982). These numerical investigations were accompanied by the theoretical studies of equatorial planetary waves by Matsuno (1966) and of their responses to heating by Gill (1980). Nevertheless, I wanted to learn more about its physical mechanisms from the standpoint of 3D normal modes.

So, the following is what I did. First, I calculated the linear response of a stratified global resting atmosphere to a specified tropical forcing to find out which normal modes are excited. For a parabolic form of heating in the vertical in the troposphere, the internal modes with the equivalent height of a few hundred meters are favorably excited (#64, 1984). This implies that the disturbances created by cumulus heating tend to show baroclinic structures consistent with observations. For stationary heating most of the excited energy goes into the rotational modes, but a significant portion also goes to the Kelvin modes, while other inertio-gravity (IG) wave modes are insignificant in general. For transient heating, the generation of IG modes, except for the Kelvin modes, depends strongly on the time scale of heating, while the rotational modes and Kelvin modes depend only weakly on the heating rate. This unique response of the Kelvin modes may result from the resemblance of the heating pattern to the horizontal structure of Kelvin
modes and the closeness of their frequencies to those of the rotational modes.

Secondly, I extended the previous work to find out effects of the basic zonal flow with meridional and vertical shear (#66, 1986). This work was carried out with Pedro Silva Dias under the US-Brazil Scientific Collaboration managed by NSF. The heating pattern is identical to that in the previous work, but it is stationary. When the basic zonal flow has no vertical shear, the response of the stationary heating becomes very similar to that of the previous case of no basic flow, namely the external modes are not much excited and internal IG modes are pretty much confined to the tropics. However, when the basic flow has vertical shear, the response extends globally to higher latitudes and “teleconnection” patterns appear. This is rather interesting. It turns out that the vertical shear of the zonal wind gives rise to the coupling of the external mode with the internal modes. As a result of this coupling, a significant response occurs in the external mode due to the excitation of the “baroclinic” internal modes by tropical thermal forcing. The meridional structures of the internal modes are equatorially trapped and their intensities are less affected by the basic flow. Since the meridional structures of the external mode are global, a significant response of the external mode is exhibited outside of the tropics to even higher latitudes, creating the appearance of “teleconnection” patterns. Of course, the direction of the basic zonal flow and its meridional shear have significant influence on details of how planetary wave structure in the mid- to higher latitudes shape up, but the principal mechanism of the teleconnection remains essentially the same. In fact, these modal interactions between the external and internal modes due to the presence of vertical shear in the westerlies explain the reason why the barotropic state of the atmosphere prevails in the midlatitudes (#66, 1986).

I should mention two more topics related to application of the 3-D normal mode expansion. The first one is concerned with the use of spectral method to solve the vertical structure equation. Earlier, we calculated the solutions of the vertical structure equation using the finite difference method (#57, 1981). Later, as described in the appendix of (#64, 1984), the Legendre polynomial was used to construct the orthogonal structure functions in the vertical. The other topic is to demonstrate the utility of the spectral vertical normal modes and to formulate a 3D spectral model of the PE equations using the normal mode functions. We then examined the problem of baroclinic instability on the sphere as an application. This work (#71, 1989) was carried out in collaboration with Hiroshi Tanaka who had worked on the atmospheric energetics analyses of the FGGE data using the 3-D normal mode functions as his Ph.D. thesis at the University of Missouri when our collaboration started.
11. One-year visit to the National Meteorological Center, NOAA in Washington, D.C.

After completion of the historic event called, “Global Weather Experiment”, formally known as the First GARP (Global Atmospheric Research Program) Global Experiment (FGGE) in 1979, our Large-Scale Dynamics Section in the NCAR’s Atmospheric Analysis and Prediction (AAP) Division received a research contract from the National Meteorological Center (NMC, now Environmental Modeling Center, NOAA). The objective was to evaluate the quality of global analysis data and to use the global data for improvement of medium-range weather forecasts. About the same time in 1981, Rick Anthes was appointed as Director of the AAP Division. Because his research interests at Penn State, which included hurricanes and tropical cyclones and atmospheric numerical modeling, coincided with mine, we got acquainted each other rather quickly and I have enjoyed working under Rick’s leadership since then. In fact, my association and our friendship have continued even when Rick became NCAR Director in 1986 and later became President of UCAR in 1988.

Now, back to our research contract from NMC, Bill Bonner, Director of NMC approached Rick Anthes about NMC’s interest to strengthen research collaboration between NMC and NCAR. Rick, then, organized our section’s retreat with members of NMC to discuss our mutual interest to improve numerical weather prediction in the fall of 1984 at Wallops Island near Rick’s vacation home in Chincoteague, Virginia. It was clear that Bonner’s intention was to expand NMC’s in-house research activities by inviting both long- and short-term visitors from the university community as well as NCAR rather than just giving research contracts to outside organizations. As the result, the idea of the UCAR Visiting Scientist Program (VSP) to NMC was born. The idea was that NMC provided funding to UCAR to select visiting scientists from the university community as well as NCAR to work at NMC with their staff. The objective of the Program is to strengthen NMC’s research and operational development activities in which the university research community has demonstrated expertise. Of course, the Program was designed to not only strengthen NMC’s operational activities, but also stimulate research in the university community. The selection of candidates was done by a UCAR VSP advisory panel and UCAR provided the logistics for visiting scientists at NMC. At this point, they were looking for a visitor to start this program. I realized that it would be an interesting opportunity for me to spend one year at NMC, one of the leading operational organizations in the world. Since I am a research person, I wanted to find out how I would do in an operational environment, something like a soldier engaged in a battle field. So, I applied to UCAR and was selected to start the Program. Because I felt that one person was not enough, I asked the Advisory Panel
to invite another visitor, Takashi Sasamori who was a professor at the University of Illinois and an expert in atmospheric radiation and boundary layer processes. So, Takashi and I spent the entire year 1986 at NMC, which was located at Camp Springs, MD near Washington, D.C.

Prior to our visit, Bill Bonner requested that we look into a way to improve weather prediction in the tropics. It was known then that, during the very few hours of a forecast run, predicted precipitation rates are unusually small particularly in the tropics. This is called the "spin-up" problem and was one of the unsolved problems in numerical weather prediction in those days. Among many possibilities that cause the spin-up problem, we decided to examine the quality of objective analysis in the tropics first. Since we shouldn't expect any precipitation where air is descending, we first decided to examine how well analyzed vertical motions correlate with precipitation rates. Since the analyzed precipitation rates were not available, we used outgoing longwave radiation (OLR) data as proxy. From this study we found that the analysis of vertical motion was totally inaccurate. Since the vertical motion is calculated from the divergent wind component, we inferred that the analyzed divergent wind component was erroneous. So, we cooked up a clever scheme to estimate the vertical motion field from the OLR data. Once the vertical motion field was estimated, we could determine the associated divergent wind component. Thus, the total wind was the sum of the divergent wind and the rotational wind, which was assumed to be unchanged as analyzed. Moreover, we demonstrated that these recalculated winds fit better with observed winds at observation stations. It was clear that our recalculated winds were better than the operationally analyzed winds. This work was written up together with two programmers, R. Balgovind and B. B. Katz (#70, 1988).

I very much enjoyed myself spending one year working at NMC, which I felt was an exciting place to visit. I often had lunch together and wonderful conversations with Norman Phillips who was Chief Scientist and John Brown, Director of Development Division, NMC. I knew John very well as he was at NCAR in our same group before moving to NMC. There were also several short-term visitors from the university community who were selected by the VSP Advisory Panel. NMC and the UCAR VSP Program arranged to hire four programmers in residence at NMC to assist the visiting scientists. Both Takashi and I stayed in the same apartment complex near NMC. Yuko joined me from time to time and I went back to Boulder often during the year. So, I didn’t have any problem even though we lived separately. During weekends and whenever Yuko visited me, I very much enjoyed visiting the museums and art galleries as well as attending cultural performances in Washington, D.C. It is my understanding that the UCAR Visiting Scientists Program to various NOAA laboratories continues successfully even today.
12. The spin-up problem - Diabatic initialization

I already mentioned the "spin-up" problem in connection with my one-year visit to the NMC. Namely, operational forecast models in those days were incapable of producing realistic precipitation rates during the first several hours. Our finding at NMC was that the objective analysis of horizontal divergence was erroneous and we proposed an adjustment scheme to the divergent wind component by incorporating satellite imagery data. I was hoping that application of this scheme would improve precipitation forecasts in the tropics. However, to our disappointment the forecasts still showed the spin-up problem. Apparently, having a good wind analysis was not enough to cure the ill. Namely, conventional diabatic initialization in those days suppressed gravity-wave noise, but it did not ameliorate the spin-up problem because (1) the diabatic information was insufficient for tropical initialization, (2) there was no initialization of the moisture field, and (3) no specific consideration was made to ensure that the initiation of cumulus convection reflected reality. Clearly, there were many questions to be answered to solve the spin-up problem, but I decided to look into those questions one by one as a new challenge --- one that eventually lasted almost 10 years!

During the fall of 1988, I had an opportunity to visit Japan to work at the Numerical Prediction Division of the Japan Meteorological Agency (JMA) in Tokyo and the Meteorological Research Institute in Tsukuba with funding from the Science and Technology Agency of Japan. Accepting this opportunity, I decided to look into the spin-up problem in the JMA operational global spectral model. Contrary to my expectation, the operational JMA model had practically no spin-up problem. Apparently, their success was due to the use of geostationary satellite imagery data for the moisture analysis. Instead of the spin-up problem, however, we noticed the presence of unusually intense precipitation cells, which were erroneous. Working together with my Japanese colleagues K. Tamiya, T. Kitade, and M. Sugi, we began to understand the cause of this problem. What was happening was that small cells in the divergence field which were present erroneously in the initial conditions grew rapidly by condensation heating. It occurred to me that the idea of cumulus initialization, similar to one proposed by Leo Donner (1988), might be applicable to "correct" erroneous horizontal divergence cells. In Donner’s case, both the temperature and water vapor fields were adjusted consistently with the model’s cumulus parameterization, while the horizontal divergence field was left unchanged. In the present case, however, the water vapor field did not need to be modified, but the horizontal divergence field had to be adjusted to be consistent with the model’s cumulus parameterization. Indeed, this idea worked well to suppress undesirably intense precipitation cells at the

Whenever I had opportunity, I always enjoyed visiting Japan, as I know the way around and meeting parents, relatives and school friends was a pleasure. But, this time was a little different, because my mother had been seriously ill and was staying in hospital. My mother was very pleased when I visited her, and talked about her pleasant days at our house in Boulder during her visit in the fall of 1981 and our trip to the Rocky Mountain National Park. While seeing her every weekend, I saw a gradual deterioration of her health and sadly she passed away on January 6, 1989, just before my return to Boulder. She was 89 years old. Incidentally, my father passed away a decade earlier in 1979 (February 3rd). He too was 89 years old. I was extremely busy in those days and was unable to take time off to visit Japan to see him. He looked frail when I visited him in Japan in July 1978, but I didn’t expect that his health would deteriorate so quickly. One year later I stopped over in Japan on the way back to U.S. from India to pay my visit to our family tomb where my father is resting.

After my return to NCAR, I talked with Leo Donner about my findings at JMA. Together with Arthur Mizzi who had been working with us to evaluate the quality of FGGE global data, Leo and I decided to carry out a comprehensive study of an initialization scheme that included diabatic heating effects due to mainly cumulus convection. In Donner’s original cumulus initialization scheme, the temperature and moisture fields were adjusted in such a way that a version of the Kuo cumulus parameterization scheme produced a precipitation rate close to “observed,” leaving the initial divergence field intact. This approach worked well when Donner and Rasch (1989) tested the scheme for simulated global data produced by a climate model, but was found inadequate when we tested it using analyzed FGGE data as input data for our global forecasting model that was adopted from the NCAR Community Climate Model-1 (CCM-1). This was because, not only the temperature and moisture fields needed adjustment for the initiation of cumulus convection, but also the input divergence field and diabatic information were both inaccurate and needed adjustment. Above all, we found that it was necessary to know the location and intensity of precipitation as a part of the initial conditions.

In the next several years, Leo, Arthur and I worked step by step to formulate the diabatic initialization scheme that included cumulus initialization as well as the traditional nonlinear normal mode initialization (NNMI) for a modified version of the CCM-1. [The original version of CCM-1, described by Williamson et al. (1987), used the convective adjustment scheme of Manabe et al. (1965), but we replaced it by the Kuo-scheme for
cumulus parameterization. We used the FGGE level IIIb analyzed data as the initial conditions together with a proxy data set of diabatic heating rate and observed precipitation data. After writing a series of three articles (#77, 1992: #78, 1994: #80, 1996), we finally arrived at a general approach to the diabatic initialization with a special emphasis on cumulus initialization. We now defined “cumulus initialization” to be an adjustment procedure for the temperature, specific humidity, and horizontal divergence of the input data to a prediction model in such a way to produce initial precipitation rates as close as “observed” without generating dynamical imbalance. So, the cumulus initialization was an optimization procedure with various constraints, but there is no single method to handle the adjustment because the detail of the optimization program depends on a particular scheme of the cumulus parameterization adopted in forecast models. For example, our modified CCM-1 adopted the Kuo scheme, which has a relatively simple functional relationship with the input variables. However, CCM-2 uses the Hack scheme which was more complex for use of the optimization for data adjustment. Since there are many different kinds of cumulus scheme, it became clear to us that developing a formal procedure of cumulus initialization based on the application of optimization for various cumulus schemes was a difficult task with little payoff. So we had to think about developing a more practical procedure of the diabatic initialization for a specific prediction model at hand, such as CCM-3 which used yet a different cumulus parameterization scheme. Later, we developed a much simpler idea of inverting the cumulus scheme, namely for a given precipitation rate we adjusted the moisture field only and applied the procedure to a forecasting problem as I will talk about it in the next section.

As an added note here, since the daily global analyses of precipitation rates were not routinely available in those days, we had to spend considerable effort to produce proxy precipitation data by combining the 5-day mean precipitation estimates of the Global Precipitation Climatology Project with the daily outgoing longwave radiation (OLR) data. It turned out that this proxy daily precipitation data set was very useful for application to diabatic initialization for forecasting problems.
13. Return to research on tropical cyclones

Now, I should describe why I happened to come back to research on tropical cyclones. At the beginning of the 1990s, I was asked by the management of the Central Research Institute of Electric Power Industry (CRIEPI), Japan to sponsor a CRIEPI scientist for getting information from NCAR on the impacts of global warming on the climate of eastern Asia, including Japan, to the extent that it affects the management of electric power industry. I said that I was not a suitable person for their purpose as I was neither a climate scientist nor a regional modeler and I suggested that they contact Filippo Giorgi who headed a regional climate modeling group at NCAR at that time. After negotiation, CRIEPI made an arrangement with NCAR that Hiromaru Hirakuchi work with Filippo for two years starting from 1992 on climate change research using a regional climate model (RegCM). However, during Hirakuchi’s stay at NCAR, Filippo accepted an offer from Italy and left NCAR. So, Hirakuchi came over to talk to me. I then found out that he was particularly interested in the effects of global warming on tropical cyclones, specifically typhoons in the Pacific. Because I was working then the problem of diabatic initialization for forecasting models and also familiar with forecasting of typhoons, I felt that this was an interesting opportunity for me to come back to research on tropical cyclones and I agreed to sponsor Hirakuchi’s stay at NCAR. This was how my collaboration for nearly a decade with many scientific long-term visitors from CRIEPI started. I should add here that the staff of UCAR/NCAR management, particularly Wayne Shiver, was very helpful in connection with preparing paper work for the CRIEPI-NCAR contact agreements and the logistics of inviting many visitors from CRIEPI.

Among many CRIEPI scientists who visited NCAR, I worked most closely with Junichi Tsutsui who came to NCAR for two years, 1994-96 and continued our collaboration after he left NCAR. Junichi was interested in the impact of anthropogenic climate changes on tropical cyclones. So, the first thing we did was to investigate how well a general circulation model could simulate the behavior of tropical cyclones. We analyzed a 20-year run from the standard T42 version of CCM-2 driven by climatological sea surface temperature (SST) and a 10-year (1979-1988) run driven by observed SSTs to find out how well tropical cyclones (TCS) are simulated. We were rather surprised to find that the climatology of simulated tropical cyclones (STCs) in terms of the statistics of regional formations as well as the total number of storms was well reproduced by the CCM-2. Of course, the model was clearly deficient in reproducing the size, intensity, trajectory length and time duration of observed TCS. This should be expected, since the grid mesh length of the T42 resolution model is approximately 250 km, which was too
coarse to resolve the size of real TCs, although the structures of STCs were in good agreement with those of observed. See (#81, 1996).

Encouraged by finding that the climate model was useful for simulating the behavior of TCs in today's climate, we wanted to analyze the statistics of simulated TCs in a 21st century global warming projection run with a gradual increase of CO₂ concentration in the atmosphere, but such a model run was not available at NCAR at that time. Consequently, CRIEPI scientists decided to assist NCAR scientists to run a 125-year future global climate projection by specifying a 1% increase of CO₂ concentration per year according to the IPCC (Intergovernmental Panel on Climate Change) emission scenario. NCAR's first CSM (Climate System Model) was used for this purpose and CRIEPI made an arrangement with NEC to run its simulation on the SX-4 supercomputer at the NEC factory in Fuchu, Japan in 1997. Each year of the model run took about 90 minutes on SX-4 and the NEC provided us the entire computer time of well over 200 hours without charge! This turned out to be the first NCAR global warming projection run. I was involved in this endeavor as a liaison between NCAR and CRIEPI and spent many hours of negotiation. Bob Serafin, Director of NCAR at that time, was very helpful in convincing the CRIEPI management of the importance of conducting this experiment.

The CSM used for the first global projection run at NCAR was a coupled model of the atmosphere, ocean, and land as well as sea ice. The atmospheric model was based on a T42 version of CCM-3. Junichi Tsutsui and I were wondering how well the CCM-3 can simulate TCs. The CCM-3 incorporated many improvements over CCM-2 mainly in physical parameterizations and in making the atmospheric model more suitable for coupling with the land, ocean, and sea-ice component models. Collectively these modifications greatly reduced systematic biases in energy balance that existed in the CCM-2. For example the large warm bias in July surface temperature over the Northern Hemisphere that existed in CCM-2 was reduced (Kiehl et al., 1996). However, Junichi and I were very surprised that CCM-3 did not perform well in the simulation of tropical transient phenomena such as TCs and tropical intraseasonal oscillations. We were wondering why? Since finding the reason for this result was a sort of detective story, I should explain what we found out.

Among many differences in physical parameterizations between CCM-3 and CCM-2, we focused on the difference in handling moist convection in the two models as this affects the simulation of tropics a great deal. While CCM-2 employed the Hack (1994) scheme for all types of moist convection, CCM-3 adopted a two-step adjustment. The Zhang and McFarlane (1995) scheme (ZM scheme) was applied first for deep convection, then the Hack scheme
took over for shallow convection. The ZM scheme is based on a cloud model of a plume ensemble similar to that proposed by Arakawa and Schubert (1974) and a closure formulation to determine the cloud base mass flux depending on convective available potential energy (CAPE). In contrast, the Hack scheme is based on a cloud model of a three-layer plume to adjust moist static energy. It is similar in principle to the convective adjustment scheme of Manabe et al. (1965).

The reason that CCM-3 produced so few TCs was that the ZM scheme handled most of the moist convection and the Hack scheme did not play much of a role. As the result, the tropics were dominated by drizzle-type precipitation without producing much intense convection. In fact, the ZM scheme kicked in wherever positive CAPE is present regardless of the moisture conditions. In order to reduce the contribution of ZM scheme, we decided to implement a relative humidity threshold ($RH_c$) as an additional condition for triggering the ZM scheme so that deep convection did not occur unless the relative humidity in the model’s sub-cloud layer exceeded a threshold value. For a larger threshold, more accumulation of moist static energy was required to generate deep convection. Since moist static energy is stored predominantly in large-scale convective zones, the threshold value would control the degree of moist convection necessary for generation of the TCs. We conducted four cases of one-year simulations of the T42 CCM-3 with the threshold value: $RH_c = 0, 80, 85, \text{ and } 90\%$ and examined the climatology of TC appearance. Climatological sea surface temperature was specified. Note that $RH_c = 0\%$ case of CCM-3 is nearly same as the original CCM-3.

Although virtually no TCs appeared in the simulation with $RH_c = 0\%$, the formation of TC dramatically increased as the threshold value increases. The case with $RH_c = 85\%$ produced almost the same TC frequency as in the CCM-2, which is in agreement with the observed frequency of TCs. What happened in this experiment was that the increase of relative humidity threshold reduced the “deep” ZM convection and increased the “shallow” Hack convection, while maintaining the global mean convective precipitation rate almost unchanged. Apparently, the increased contribution of the Hack scheme was beneficial for the formation of TCs. Junichi Tsutsui reported this finding at the AMS 24th Conference on Hurricanes and Tropical Meteorology, 29 May - 2 June, 2000 at Ft. Lauderdale, FL.

The above results imply that it may be possible to tune the CCM-3 so that the tropical transient disturbances can be simulated reasonably well without much degradation of overall climatology of the large-scale circulations. However, we began to think that the T42 resolution of model was too coarse to realistically reproduce the climatology of TCs. On the other hand, it was
too time and resource consuming to pursue the question of impact of global warming on TCs with a higher resolution climate model. So, we decided to refocus our research to the improvement of typhoon forecasts using a regional atmospheric model. Earlier, Hirakuchi and Giorgi (1995) studied the impact of CO₂ increase on the monsoon climate over eastern Asia and Japan using regional climate model (RegCM) nested in a GCM. So Junichi and I asked Hirakuchi to reconfigure the RegCM for numerical experiments of typhoon forecasts. The model’s grid increment was about 50 km and the size of forecast domain was large enough to cover the western Pacific. We specifically decided to make an in-depth case study of two typhoons Ed and Flo in September, 1990, which appeared over the western Pacific almost simultaneously for more than one week, but behaved rather differently in terms of their movement and intensity changes.

Earlier, I talked about our section’s effort to develop a diabatic initialization that consists of a combination of “normal mode initialization” and “cumulus initialization” using proxy data of precipitation intensity as a part of the input data to the CCM-1 and CCM-2 used as forecasting models. Since we now use the regional model RegCM as a forecasting model, we had to first construct the diabatic initialization package for the RegCM which had an option to use either the Kuo scheme or the relaxed Arakawa-Schubert (RAS) scheme developed by Moorthi and Suarez (1992), based on the original formulation of Arakawa and Schubert (1974). We used the nonlinear normal model initialization (NNMI) program for the RegCM developed by Ron Errico and his collaborators (1994). From our earlier experience in developing the cumulus initialization using an optimization algorithm, we learned that the most crucial aspect of cumulus initialization was to modify the input moisture field in such a way that the cumulus scheme produced the precipitation intensity that matched with the observed intensity. Since the cumulus scheme calculated the precipitation intensity for the input data of divergence, temperature, moisture and other model variables, the first step was to apply the NNMI with diabatic heating input to adjust the wind and temperature fields. Then, the inversion of cumulus scheme with “observed” precipitation intensity finds the smallest adjustment of input specific humidity that kept the shape of vertical profile unchanged. In fact this inversion scheme is straightforward to apply to any cumulus scheme as I described in an article written with Tsutsui and Hirakuchi (#82, 1996).

Because Typhoons Ed and Flo were present almost simultaneously yet they behaved rather differently, this case of September 1990 provided a good opportunity to investigate the efficacy of diabatic initialization and to compare typhoon behavior using the two cumulus schemes of Kuo and RAS. We used the T106-resolution uninitialized-operational-objective analyses of ECMWF as input data. Since the grid interval of the prediction model was 50
km while the resolution of the ECMWF analyses was about 120 km, the ECMWF data were interpolated onto the prediction domain. The lateral boundary conditions were also interpolated from the 6-hourly ECMWF data. We performed many 48-hour prediction runs for various synoptic cases.

The results of these forecasting experiments were published in #83 (1998): First of all, the importance of diabatic initialization including cumulus initialization on forecasting of TCs in their formative stage was clearly demonstrated. Since the intensity of cyclonic vortex at its incipient stage was weak, the prediction model was unable to produce moist convective activity as expected in reality unless the irrotational wind and moisture fields were suitably initialized. Therefore, the precipitation rates estimated from satellite data were clearly necessary. Secondly, although the benefit may be small, the diabatic and cumulus initializations were still effective for prediction of TCs, even in their mature stage. The benefit may come from the amelioration of precipitation spin-up problem. However, the diabatic and cumulus initialization was no substitute for inaccurate input analyses. For example, when a storm reached the mature stage but the input cyclonic circulation derived from the objective analysis was too weak, the initialization procedure was not helpful even though the estimated precipitation rates were accurate. Therefore, the improvement in data assimilation by taking into account precipitation estimates is clearly paramount for accurate prediction of TCs. Thirdly, the question of which cumulus parameterization scheme, Kuo or RAS works best for TC forecasting does not have a simple answer. The major difference in the performance of the Kuo and RAS schemes was found in the behavior of their precipitation rates: the RAS scheme was relatively insensitive to variations in the moisture profile, so that the precipitation rate (PR) increased gradually as the moisture in the vertical column increased. In contrast, the increasing trend of PR in the Kuo scheme, as the moisture in the vertical column increases, is rather slow until the moisture profile becomes very close to the saturation profile. Despite the presence of obvious differences in the two cumulus schemes, we found that both schemes performed reasonably well overall once the respective cumulus initialization was used.

Because the regional model was useful only for a forecast period of two days or so, we wanted to use of a high resolution global model for longer range predictions of typhoons. We learned that the CCM modeling group developed the T63 and T170 versions of CCM-2. We also knew that the NNMI program of Errico and Eaton (1987) is applicable to these CCM versions. The horizontal grid length of T170 model is approximately 80 km and it is comparable to that of the regional model we had used earlier. We decided to focus on two aspects of research. One was to investigate the behavior of typhoons from their formation to landfall/decay. The other was to examine
the synoptic environment of typhoon formation. So we added two more
global analyses of JMA (Japan Meteorological Agency) and NCEP (previously
NMC) in addition to the ECMWF analyses. We again examined the dual
system of typhoons Ed and Flo of September 1990 as done for the regional
model mentioned earlier. Typhoon Flo formed in the wake of Ed which
moved westward on the south side of subtropical high in the Western Pacific.
The forecasting procedure including the diabatic initialization (NNMI plus
moisture adjustment) was very similar to that used in the regional model.
Therefore, we were able to carry the knowledge gained from the regional
model study to the global case.

Because it was time consuming to run the T170 CCM-2 in those days, many
experiments were carried out using the T63 CCM-2 whenever possible. A
large number of 6-day forecasts were made for the cases of Ed and Flo with
the T63 and T170 versions of CCM-2 with the modified RAS cumulus scheme
using the ECMWF, JMA and NCEP analyses as the input data. It is too much
to present the results of many experiments here. Therefore, I just want to
mention my general impression of the TC forecasts based on the two
versions of CCM-2. It was clear that the diabatic initialization was very
important with the use of precipitation intensity estimated from the infrared
data from the Geostationary Meteorological Satellite (GMS). The
precipitation data were important particularly for adjustment of the input
moisture field, because it was poorly analyzed. The analyses of moisture
fields were surprisingly different among three global data sets. For instance,
the JMA analysis was much more moist compared with the ECMWF analysis
and the cumulus initialization had to reduce the overall moisture content in
order to produce the initial convective precipitation rate expected from the
“observed”. As far as the TC trajectory is concerned, when the TC vortices
were well defined initially, the TC trajectories were predicted reasonably well
up to six days. The higher resolution model, as expected, performed better.
However, the TC intensity forecasts were inferior regardless of the model
resolution.

Since TC forecasting is one of the major responsibilities of operational
forecasting centers, research on TC prediction is carried out at major
operational centers. Therefore, our efforts, in the style of cottage industry,
were no match to those of the operational centers. Knowing such a
limitation, I nevertheless did this research as a logical application of our
research on the diabatic initialization and as the focus of research
collaboration with the CRIEPI scientists. Because my career had started with
typhoon research, I had wanted to learn what the latest issues were in this
field and I felt that my curiosity had been satisfied particularly in connection
with the genesis of typhoons.
14. Normal modes of global nonhydrostatic atmospheric model

I already mentioned that when Filippo Giorgi left NCAR to take a position in Italy, the sponsorship of a visitor, Dr. Hirakuchi of CRIEPI, became in limbo. So, I became his host. It turned out that there was another visitor in Filippo’s group who was looking for a sponsor. He was Jian-Hua (Joshua) Qian, who received a Ph.D. from the North Carolina State University. Qian accepted an NCAR post doc in 1996 and started to work with Filippo on regional climate modeling. Actually, I heard about his Ph.D. thesis work even before his joining NCAR through Fredrick Semazzi, who was a professor at the North Carolina State University and Joshua’s thesis advisor. [Fredrick visited NCAR during 1970’s, worked with me writing a thesis on an initialization problem, and received a Ph. D. degree from Department of Meteorology, University of Nairobi, Kenya. I served as External Examiner of the same Department and visited Nairobi during the summers of 1977 to 1979.]

Joshua’s Ph.D. thesis was on the development of a nonhydrostatic semi-Lagrangian global atmospheric model, which I felt was a solid piece of work. Thus, Joshua and I thought that it was a good idea to understand more the properties of nonhydrostatic global model. So, we decided to work together on the problem of normal modes of the nonhydrostatic compressible spherical atmospheric model. This problem had been treated during 1960’s by Dikii (1965) and Eckart (1960), but it had left unexplored further until recently. Here, I should mention that the term nonhydrostatic referred to the inclusion of the vertical acceleration term in the vertical equation of motion in the frame work of (shallow atmosphere) primitive equation system.

The most recent work on this subject was done by Roger Daley (1988). He was interested in finding out how small the motions on the sphere had to be for the vertical acceleration to become important. Roger solved the normal modes of a global nonhydrostatic model by the separation of the variables, but he formulated it in such a way that the separation constant, known as the equivalent height, appears only in the vertical structure equation. However, by doing so, the horizontal structure equation becomes no longer the Laplace tidal equation and a new algorithm had to be developed to calculate the eigenfunctions and their associated frequencies. Since the solution of the horizontal structure equation requires a great deal of effort, we thought that a better approach would be to reformulate the problem in such a way that the horizontal structure equation is the Laplace tidal equation as in the case of the hydrostatic model. By doing so, the horizontal eigenfunctions are still Hough functions and the wave frequencies are determined by specifying the equivalent height (EH). It turned out that this formulation makes the vertical structure equation depend not only on the
EH, but also on the wave frequency just like the horizontal structure equation. Namely, both structure equations contain two unknowns of the EH and the wave frequency. Therefore, the eigenfunctions of the vertical structure equation and corresponding eigenvalues (EH and wave frequency) must be determined as a coupled system.

Actually, it was rather interesting to solve this coupled eigenfunction-eigenvalue problem which depends on two unknown parameters, EH and wave frequency. After examining how the wave frequency varies as a function of EH in each structure equation, we realized that this simultaneous system could be solved by an iteration method. The success of this iteration method depends on how good the initial guess can be chosen. Anyway, we succeeded solving this simultaneous eigenfunction-eigenvalue problem as described in our findings in the article # 85 (2000).

What did we learn from this study of calculating the normal modes of a nonhydrostatic global model? First of all, in the hydrostatic model, there are two kinds of normal modes: One is the first kind, which consists of fast eastward and westward propagating inertio-gravity (IG) oscillations. The other is the second kind, which consists of slow westward propagating rotational (Rossby-Haurwitz) oscillations. These two kinds of oscillations are associated with various values of the equivalent height (EH). The largest value of EH is approximately 10 km in an Earthlike atmosphere and it is called the external mode. The remaining values of EH are all smaller than that of the external mode and they are called the internal modes. The external IG oscillations, consisting of westward and eastwards propagating gravity waves, have a special name and are referred to as Lamb waves, which propagate with approximately the speed of sound. The external rotational oscillations have the same characteristics of 'equivalent barotropic planetary waves.' These two kinds of external mode oscillations are very special, because vertical motions are nearly vanished.

In the nonhydrostatic model there is the third kind of oscillations as the eastward and westward propagating inertio-acoustic (sound) modes in addition to the two kinds of oscillations mentioned earlier. Their frequencies are much larger than those of the first and second kinds.

The inertio-acoustic (IA) modes are distinguished from the other two kinds of the modes having large values of EH greater than the external mode EH. That is why the IA modes, known as sound waves, propagate with large speeds. Moreover, sound waves propagate vertically as well as horizontally due to the acceleration term in the vertical equation of motion. Otherwise, the properties of oscillations are similar to those of the first kind. The IG waves in the nonhydrostatic model are correctly represented as the wave
frequency tends to the Brunt-Väisälä frequency as the scale of motion becomes smaller, while that does not happen in the hydrostatic model. Therefore, the IG modes of the nonhydrostatic model are significantly different in both the frequency and the meridional structure from those of the hydrostatic model for smaller-scale motions with a large aspect (vertical/horizontal) ratio. That is why the nonhydrostatic effects are important to describe small-scale motions. In fact, the IG mode frequencies of two models for infinitely deep motions start to deviate from around the zonal wavenumber 400, corresponding to roughly 80 km in wavelength in the mid-latitudes, in agreement with the result of Daley (1988).

Finally, I should add that Qian and I also worked together on the analysis of nonhydrostatic normal modes in an isothermal atmosphere using midlatitude and equatorial beta planes to complement with our spherical study. We found that the use of these two beta-planes can provide good approximations to describe the characteristics of nonhydrostatic normal modes on the sphere (#88, 2003).
15. Roles of the horizontal component of Earth’s angular velocity

In the fall of 2000 I was invited to visit the Frontier Research Center for Global Change in Yokohama, Japan and I gave a talk on the normal modes of nonhydrostatic global model described in the previous section. After my talk I received a question why I didn’t include the Coriolis terms due to the horizontal component of Earth's angular velocity. I replied that with the shallowness approximation it is traditional to neglect those cosine Coriolis terms by quoting the work of Phillips (1966). Actually, I was not quite satisfied by my answer because the role of the horizontal component of Earth's angular velocity is a legitimate one even though its magnitude could be small. I remember that the theory of relativity was born by recognizing that a negligibly small value of the ratio of material speed over the speed of light had an important physical significance.

After returning home I started thinking about how I could understand the physics behind this neglected role. In fact, I remembered that L.F. Richardson (1922) in his book of NWP expressed uneasiness about neglecting the cosine Coriolis term in the vertical equation of motion and Carl Eckart (1960), in his book on hydrodynamics, attempted to understand its physics. Anyway I thought that there was no chance of understanding the role of the cosine Coriolis terms in a nonhydrostatic model of spherical geometry and instead I decided to examine the tangent-plane version of a linear nonhydrostatic compressible model. After a failed attempt to solve the system by the separation of variables, it occurred to me that the coefficients of the variables in this linear model are all constant. Therefore, the solution of this model can be expressed by using harmonic functions.

I assumed that the domain is periodic in the horizontal direction, but the vertical velocity vanishes at the bottom and top of the domain. It turns out that the imposition of the vertical boundary conditions had a fundamental impact on understanding the unique role of the cosine Coriolis terms in this system that is not present in the traditional system. In the traditional linear nonhydrostatic model without the cosine Coriolis terms, the wave frequency is obtained as the roots of a quartic (4th degree) dispersion equation, which appear as two pairs with the positive and negative signs. These two pairs of roots represent the inertio-gravity (IG) waves and inertio-acoustic (IA) waves, respectively, which move eastward and westward. However, in the non-traditional model with cosine Coriolis terms, the wave frequency is obtained as the roots of a sextic (6th degree) dispersion equation. Therefore, we had to solve it numerically. However, by examining the solutions of its special cases, it became clear that there is one pair of positive and negative roots which represent unknown oscillations having frequencies very close to the Coriolis frequency, but with magnitudes that
are smaller than those of the Coriolis frequency. At this point, I was not aware of any reference to this kind of near-inertia waves, except I could recall a short article of Egger (1999), who pointed out that cosine Coriolis terms gives rise to the oscillations whose frequencies are very close to the Coriolis frequency.

At this point I didn’t know what to do, as no one I talked to gave me any definite opinion, though Joe Tribbia and George Platzman encouraged me to go on. So, I decided to write up what I had done and submitted to the Monthly Weather Review as a sequel to the work of the normal modes of nonhydrostatic global model described in the previous section. Unfortunately, the manuscript was rejected with little explanation from the editor. After a little bit of disappointment, I began to realize that I should have done a more careful literature search on this problem and I needed to focus more closely on an understanding of this particular solution. Meanwhile, Pitor Smolarkiewicz of NCAR’s meso-scale section showed me two manuscripts, written by Thuburn, Wood and Staniforth (2002a; 2002b), to be published in Quarterly Journal of the Royal Meteorological Society. Pitor said that I may be interested in reading it. Sure enough, I couldn’t stop reading the two papers and I was shocked to find what I read. One article was concerned with a numerical study on the normal modes of deep nonhydrostatic atmosphere in the spherical geometry. Realizing that the results of numerical calculations are difficult to interpret physically, they undertook in the other article an analytical study of the same system on the tangent-plane. Both articles, of course, dealt with the dynamical models that include the cosine Coriolis terms in the equations of motion. In this latter study they found a “new mode” of oscillations whose property is very much like what I found as the near-inertial mode. They were not sure what it is, but seemed to be convinced it was worth reporting.

Now, I was determined to undertake my effort to understand the nature of this “new mode” and decided to do two things: One was a literature search to see whether anyone had done anything earlier on this question. The other was to analyze a hierarchy of atmospheric models that include the complete Coriolis terms, starting from a simple incompressible and homogeneous model that includes only the rotation effect, then a Boussinesq model that includes both the buoyancy and rotation effects, and finally a compressible and stratified model that includes the compressibility in addition to buoyancy and rotation. Surprisingly, the essence of the nontraditional effect showed up even in the simple incompressible and homogeneous model. Recall that this system without the cosine Coriolis terms has been used to explain the inertial waves whose frequencies are quadratic with plus and minus signs. However, the inclusion of cosine Coriolis terms in this system with the boundary conditions that the vertical motion vanishes at the top and bottom
gives rise to the 4th order dispersion equation for the frequency. Namely, there are two additional roots of frequency with plus and minus sign together with those of usual inertial waves. In fact, these extra roots seemed to represent the “new mode” mentioned earlier as the magnitude of these roots is very close to the Coriolis frequency. I should mention that Tolstoy (1973) in his book Wave Propagation considered the inertial waves with cosine Coriolis terms included, but because he simplified the system by assuming no meridional variation (zonal propagation only) he missed the presence of this near inertial mode. This is because the meridional wavenumber is coupled with the cosine Coriolis parameter to produce the near inertial mode.

I continued to look for the references on this kind of near inertial mode as I just could not believe that no one has ever noticed it before John Thuburn and his colleagues mentioned earlier. (Later I found few earlier works on this near inertial mode while I was preparing a manuscript for publication. See #87, 2003.) Anyway, I thought that I need some name for this near inertial mode as calling this as “a new mode” sounds like almost no name. Because this particular mode appears only when the vertical boundary conditions are present, I decided to call it as “boundary-induced inertial (BII) mode”. The BII mode not only appears in the incompressible and homogeneous model, but also in the Boussinesq model and the compressible and stratified model. It turns out that Eckart (1960) investigated earlier the case of compressible and stratified model with the boundary conditions and discussed the solution of Lamb waves as the external mode, but left the solutions of internal modes unexplored. Eckart (1960, p.134-135) wrote that “These calculations are by no means complete. It would be possible to discuss the properties of the simple waves -- their phase and group velocities, the impedances, etc., -- but the algebraic complexities would be great. These two sample calculations indicate one thing, however: there are effects that depend on [cosine Coriolis terms], and these can be very marked for frequencies in the neighborhood of [inertial frequencies].”

Since I had enough materials for the results and past references, I wrote a manuscript and submitted to the Journal of Atmospheric Sciences. Partly because two papers of Thuburn, Wood and Staniforth (2002a;2002b) were already published and there was not much doubt about the validity of these unfamiliar findings, my article (#87, 2003) was accepted for publication. One of the reviewers, however, did not like my use of the terminology of “BII mode.” In fact, Dale Durran and Chris Bretherton (2004) published a note as a comment to my article. They essentially objected naming of this “new mode” as “BII mode”, because this mode appears as the linear superposition of two plane waves that exist in an unbounded domain. Thus it is not like an edge wave such as a Kelvin wave which cannot be exactly
constructed from the linear superposition of a finite set of sinusoidal wave solutions to the unbounded problem. I thought that their criticism was fair. Moreover, they showed that when the Earth’s rotation vector is tilted with respect to the vertical, there are two possible ways of superposing two plane waves, which have different vertical wavenumbers, but the same horizontal wavenumber and frequency, at imposed boundaries to satisfy rigid conditions. Since I agreed with their comments and analysis, in my reply (#90, 2004) I complemented their presentation by referring to the past studies that examined the normal mode problem of this nontraditional Boussinesq model in order to place the emergence of this near-inertial mode in a historical context. I also pointed out that this near-inertial mode may have some connection to the observed near-inertial oscillations in the sea that had been analyzed by Webster (1968) and others.

I should add here that as soon as I read the article of Thuburn et al. (2002a) who used a finite-difference method, I embarked on the calculation of normal modes of a deep nonhydrostatic global atmosphere using a spectral approach as an extension to the work I had done with Joshua Qian on the shallow nonhydrostatic global model. Although I could not apply the method of separation of the variables in the horizontal and vertical directions in the deep global model, I could still solve the problem numerically by setting up an eigenvalue-eigenfunction matrix program. Namely, a spectral approach was used in the horizontal direction using a spherical harmonics expansion and a finite-difference scheme was used to discretize the variables in the radial direction. The test was conducted with terrestrial conditions similar to those used by Thuburn et al. (2002a). Despite differences in the numerical methods, my results were in agreement with theirs in many respects, including our inability to detect the presence of additional normal modes similar to BII modes. I don’t think that this failure of finding the type of “new mode” is not due to the method of solution, but due to the lack of enough grid resolution in the vertical used in my test calculation. The same thing could have been said for the finite-difference method of Thuburn et al. (20002a). Because the vertical variability of the BII modes is very high, any numerical calculations would fail to detect them unless a sufficiently high vertical resolution is used. Anyway, I wrote up the details of numerical method as I felt that the solution method by itself had a merit, though the test calculations did not produce any new result. Later, I published the work as an NCAR technical report (#91, 2004).

Meanwhile, I thought that I should ask oceanographers about a possible connection between the observed near-inertial oscillations in the sea and the theoretical near-inertial mode. Bill Large of NCAR encouraged me to pursue this connection as he said that “Near-inertia oscillations are a big business in oceanography and there are many papers”. Thus my literature search was
expanded into oceanography. In fact, I was reading only oceanography papers in those days, pretending to be myself an oceanographer. I found many interesting articles related to the topic of near-inertial oscillations or currents. In fact, the November 1995 issue of the *Journal of Physical Oceanography* contains a collection of papers from a recent field experiment. One of the articles I was particularly interested in was the one by Chris Garrett (2001) with a provocative title of “What is the ‘near-inertial’ band and why is it different from the rest of the internal wave spectrum?”
16. New theory of inertio-gravity waves in the sea

From my literature survey in oceanography, I learned that inertio-gravity waves produce a vital part of internal wave energy in ocean currents, far more than their counter part in meteorology. Moreover, the variability of static stability (Brunt-Väisälä frequency, $N$) in the vertical has a significant influence on the behavior of internal waves. Therefore, I felt it necessary to study the normal modes of inertio-gravity waves in the Boussinesq model having a variable $N$ in height including the cosine Coriolis terms and I thought that it was best to tackle this problem numerically. Obviously, I was very much interested in how the solutions corresponding to the BII mode would turn out. At the same time, I realized how I could convince myself that the numerical results were believable. One way to make sure was for two persons to work on the same problem with two different numerical approaches. If those two solutions agreed, there would be no room for doubt. When I had a chance to see John Gary, I talked about what I was doing. I had known John since my Courant Institute days. John received a Ph.D. from Courant on applied math and joined the NCAR Computing Facility as a numerical analyst early 1960’s.

John Gary and I ended up working together and used two entirely different numerical methods both of which produced converging solutions. We examined the normal-mode solutions as an eigenvalue problem for an exponential form of $N$ as used by Garrett and Munk (1977) together with the case of constant $N$ whose analytical solutions were used to check the accuracy of the numerical methods. The results from the case of variable $N$ were very interesting. While the vertical variability of the traditional kind of IG modes dominated where the value of $N$ was large, the vertical variability of BII modes dominated where the value of $N$ was small. Since the value of $N$ decreases downward from the top, the vertical variability of the BII mode was concentrated near the bottom. Since John’s results also showed the same thing, we were confident of our findings. I thought that these results were very informative. Anyway the appearance of the IG and BII modes is complementary, indicating the inclusion of cosine Coriolis terms is absolutely essential for understanding of the internal wave structure under a realistic profile of $N$. Later, we published our findings in *Tellus* (#92, 2006).

Convinced by the importance of cosine Coriolis terms in understanding the near-inertial oscillations in the sea, I wrote to Chris Garrett at the University of Victoria, Canada asking about his opinion on the connection between the BII mode and the near-inertial oscillations, explaining to him what I did with a copy of my 2003 JAS article (#87). He replied, saying that he is wondering whether what I have done was similar to that described in the article written by Theo Gerkmema and Victor Shrira (2005a) published in the *Journal of Fluid
Mechanics and sent me a copy. I still remember very well that my heart fluttered with excitement as I read their paper. They analyzed the solutions of a linear Boussinesq model on the tangent plane without the traditional approximation. Thus, the problem was identical to that described in the Section 3 of my 2003 JAS article (#87). However, I was very impressed by their thorough analyses. Obviously, they are capable mathematicians as well as oceanographers. They stressed that “non-traditional effects profoundly change the dynamics of near-inertial waves in a vertically confined ocean.” Yes, I was right and felt relieved, but I wondered why an article of this kind had not appeared much earlier than mine.

I should note here the way Gerkema and Shrira analyzed this nontraditional linear Boussinesq model, which cannot be solved by the separation of variables. The model consists of 5 variables of harmonic waves with frequency \( \sigma \). By elimination of the variables, a partial differential equation (PDE) for the vertical velocity is obtained. This PDE is of a mixed type and a criterion can be derived to ensure that the PDE is hyperbolic. This condition leads to a possible range of wave frequency and there are two regions in the frequency domain divided by the inertial frequency (Coriolis parameter). In one region, the frequency is greater than the Coriolis frequency, but less than the Brunt-Väisälä frequency, \( N \). This branch can be called the super-inertial range, corresponding to the traditional inertio-gravity waves. In the other branch, the frequency is smaller than the Coriolis frequency and the minimum values depend on the value of \( N \). This branch can be called the sub-inertial range. Clearly, this branch of sub-inertial frequency corresponds to the “new mode” since this range is not present in the traditional analysis. Therefore, according to their analysis the “new mode” is simply an extension of traditional inertio-gravity mode beyond the Coriolis frequency, as revealed from the general property of this PDE without use of the boundary conditions explicitly. Thus, I understand that it is not appropriate to call this sub-inertial range the BII, boundary-induced inertial mode, even though this mode does not arise when the vertical boundary conditions are not taken into account. It sounds paradoxical, because a full understanding of this subtle point became clear only after solving the problem with and without the boundary conditions. I now realized that the question of what is the normal mode does not have a clear-cut answer. Anyway, I now referred to the “new mode” or the “BII mode” as simply to the “sub-inertial mode” in contrast to the traditional “super-inertial mode”.

Gerkema and Shrira (2005a) discussed in detail the normal-mode solution of this PDE for harmonic waves in a vertically bounded and horizontally periodic domain in the same way as we have done earlier. Therefore, they derived essentially the same results as ours. I should add, however, that their analysis was very extensive, covering many aspects, and I consider it as an
important contribution on this topic. One novel aspect of their discussion was that they assumed the Brunt-Väisälä frequency $N$ being variable in height as far as the analysis permits. They predicted that sub-inertial inertio-gravity waves are trapped in regions of the weakest stratification. This is exactly what happened in our normal-mode calculations with John Gary (#92, 2006). So, I felt very good about what we have done. One interesting point is that they suggested that the trapped sub-inertial IG modes may contribute to deep-ocean mixing due to their intrinsic short vertical scales.
17. A new mechanism of deep-ocean mixing

This last point on the possibility of sub-inertial inertio-gravity (IG) modes as a mechanism of deep-ocean mixing reminded me of a talk by George Platzman who discussed the energy dissipation in oceanic tides. I remembered during his seminar in the 1990’s at NCAR that Platzman pointed out the need for a new physical mechanism to explain the global budget of energy dissipation of ocean tides. I thought that the sub-inertial IG modes could be a good candidate for this missing physics. Although I had no intention of getting into research on ocean tides, I thought that it might be worthwhile to investigate the generation of sub-inertial IG motions as a time-evolution problem with the linear nontraditional Boussinesq model complement to the eigenvalue problem. After all, I have read many articles on the wind-generated inertial oscillations such as by Kroll (1975) and Gill (1984), but I haven’t yet seen a single article discussing the problem including the cosine Coriolis terms. So, I spent some time to write a computer program to solve the initial-value problems based on the nontraditional Boussinesq model.

Wave motions are assumed to be horizontally periodic, but bounded vertically at the top and bottom. The evolution of the vertical structure of wave motions is calculated from given initial conditions with or without forcing and dissipation by assuming a specified value of the Brunt-Väisälä parameter, N. So, this program was intended to be used as a simple numerical laboratory to study the time evolution of IG waves. Because this was a numerical program, again I faced the problem of how to demonstrate the believability of numerical solutions. As a test I chose a constant value of the stratification parameter N, and I demonstrated free oscillations using normal mode solutions as initial conditions. I also investigated the generation of waves by two-types of wind-stress forcing by separating the forcing term into the vorticity and divergence components. By carrying out many experiments with a simple forcing and constant stratification N, I learned a great deal on how the near-inertial oscillations developed in the ocean. Since I felt the program was working properly and I hadn't seen any article describing the initial-value approach to study IG waves without the traditional approximation, I wrote an article and published it in the Journal of Computational Physics (#93, 2007).

I mentioned earlier that near-inertial oscillations can be very prominent in the ocean unlike in the atmosphere. Consider the situation in which a steady wind forcing is switched on and internal waves develop in time. Since the imposed forcing essentially balances the pressure gradient force near the surface, the Coriolis force due to the vertical component of rotation nearly balances the flow acceleration. Therefore, the frequencies of generated
oscillations tend to become near-inertial. Since the thermal stratification $N$ is large in the thermocline, the near-inertial waves thus created are more or less confined above the thermocline, though the generated waves eventually penetrate down to deeper depths. This process takes time of more than 10 days (Gill, 1984) which is much longer than the duration of episodic storm disturbances. Nevertheless, near-inertial waves of fairly large amplitudes have been observed often in greater depths (Webster, 1968: Van Haren and Millot, 2004). Thus, how can we explain the occurrence of near-inertial waves in deep oceans?

We can imagine that if an adequate forcing is present near the bottom of ocean, internal waves would be generated just as in the case of wind-driven waves in the upper ocean. For example, tidal flows over varied bottom topography can produce internal waves as shown in the extensive references listed in such a text-book Baroclinic Tides by Vlasenko et al. (2005). So, I decided to launch a new numerical experiment with my initial-value model of nontraditional IG waves built earlier to investigate the generation of near-inertial waves in deep oceans. Two aspects needed to be changed. One was to use the variable thermal stratification $N$ in height, for which the exponential form of Garrett and Munk (1972) was adopted. The other was to add a bottom forcing through the boundary condition of vertical velocity based on the same idea of up-lifting motion due to prevailing flows in lee-wave problems. The bottom topography was represented by a harmonic shape. The tidal currents of specified periods are considered as the prevailing flow.

Many numerical experiments were carried out by assigning various tidal periods and specified latitudes of the model center. As one example, I describe the results from the experiment with diurnal forcing and the model center at 30º N. This is particularly an interesting case, because the forcing and local inertial periods coincide, resulting in the possibility of resonant motion. Because I was interested in finding the effect of non-traditional Coriolis terms, I ran two cases with and without the cosine Coriolis terms. I will describe, therefore, the results of two contrasting cases.

With a forcing period of 1 day, in both cases it took about 10 days for wave patterns to emerge from the state of rest. Then, I began to see the two wave regimes. One was in the upper domain with large vertical scales and this feature appeared in both cases. However, the other flow regime near the bottom was totally different in two cases. In the case of no cosine Coriolis terms, I could see near the bottom a line-up of regular spikes, like the teeth of sharks, which oscillated with the inertial period. In contrast, in the case with cosine Coriolis terms, rather intense wave patterns with very short vertical scales emerged near the bottom. Moreover, a periodogram
analysis indicates that the frequency of these intense waves near the bottom was distinctly sub-inertial. This contrast of two different flow features was consistent with our normal mode analyses, namely the cosine Coriolis terms yielded the sub-inertial mode having a large vertical variability near the bottom where the value of stratification parameter N is a minimum, while the usual super-inertial IG mode having a smoother vertical variability dominated near the upper part of the model. The regular wave pattern near the bottom in the run without the cosine Coriolis terms was not the sub-inertial mode, but it was the forced solution.

The result of the experiment just described is a resonance case as mentioned earlier. Therefore, when I located the model at 60ºN with the diurnal forcing, I got a weaker response. Nevertheless, the non-traditional effects were still creating one-order of magnitude larger response to the wave generation in comparison with the traditional case. In the case of the semi-diurnal forcing with the period of 12 h 25.2 min., locating the model at 75ºN resulted in another resonance case. However, because the magnitude of the cosine Coriolis term was smaller than the 30º N case, the response of forcing to wave generation became smaller. Nevertheless, resonance was still at work.

In summary, it was clear through numerical experiments that the sub-inertial mode of IG waves played a remarkable role in generating intense internal waves when forcing existed near the bottom of ocean where the buoyancy frequency N is small. I was very much excited by the results and wrote an article (#94, 2010) for *Dynamics of Atmospheres and Oceans* (DAO) as an oceanographic article.

During the process of publishing the article just mentioned, I received a comment from one reviewer concerning the effect of beta (Rossby parameter) which is missing in the tangent-plane Boussinesq model. In fact, Gerkema and Shrira (2005b) had investigated the near-inertial IG waves on the “non-traditional” beta-plane. In their analysis they focused on the beta effect for the horizontal propagation of the IG waves. However, I wanted to approach the problem from the standpoint of normal modes as an intermediate step toward understanding the normal modes on the sphere. I already mentioned the difficulty of analyzing the normal modes of the global atmosphere with the inclusion of complete Coriolis forces. It is often said that understanding of the free oscillations on the sphere can be achieved by approximating the spherical problem by examining the solutions on the equatorial and mid-latitude beta planes.

My first task, therefore, was to derive the mid-latitude beta-plane non-traditional Boussinesq model. It turns out that Rossby-waves appeared in
addition to the sub-inertial and super-inertial IG waves, which were slightly affected by the inclusion of beta. In other words, the model could deal with wave motions from large-scales down to small-scales in the mid-latitudes. I performed a detailed normal mode analysis with this new model and verified that the Rossby modes appeared in the low frequency domain, while all the features of the IG modes were preserved. Thus, the usefulness of the non-traditional Boussinesq model was extended to handle wave motions in longer time and larger space scales.

Normal-mode solutions obtained from the eigenvalue approach contribute to understanding the nature of wave motions as intrinsic properties of the model. However, what happens in reality must be calculated by time integrations as an initial-value problem starting from given initial conditions. Therefore, I first modified the initial-value program of the non-traditional Boussinesq model described earlier in the JCP article (#93, 2007) by adding the beta effect. Then, I repeated the case of generating internal waves by atmospheric forcing as described in JCP article (#93, 2007). It was rather interesting that Rossby waves were generated by the atmospheric forcing and the energy of the Rossby waves was substantial relative to the amount of energy in the IG waves. In fact, in this case as far as the generation of wave energy is concerned, the cosine Coriolis effect played a minor role compared with the beta effect. However, in the case of bottom forcing as described in my DAO article (#94, 2010), the results were opposite. Namely, when the period of bottom forcing approached the local inertial period, the generated waves became resonant and intensified due to the cosine Coriolis effect. In this situation the cosine Coriolis effect overwhelmed the beta effect. Therefore, to see the beta effect I needed to pick a non-resonant situation.

In summary the beta effect generated the Rossby mode in addition to the IG modes as long as forcing was present. Details of the Rossby waves so generated depended on the mechanism and location of forcing as well as the profile of thermal stratification N(z) among other factors. Of course, the beta effect does influence the low-frequency part of the IG waves, though the generation of IG waves is more affected by the cosine Coriolis effect. I wrote up this work with John Gary who assisted me to perform the normal mode analysis and the article was published in the Quarterly Journal of the Royal Meteorological Society (#95, 2010).

It is clear that the beta effect should be included in the theory of IG waves for its application to the atmospheric and oceanic multi-scale global motions in the mid-latitudes. Now, what about the beta effect in the IG waves in the equatorial region? We have already mentioned about the difficulty in analyzing the normal modes of atmospheric model in spherical geometry.
without the traditional approximations. Therefore, it would be very helpful if we could investigate the solutions of the non-traditional Boussinesq model on the equatorial beta-plane. I worked on this problem, but it turned out that, while it was possible to obtain the simple wave solution without vertical boundary conditions, I was unable to construct the normal mode solutions when the boundary conditions were taken into account. The only recent reference I am aware of regarding the research on cosine Coriolis effect in the framework of equatorial beta-plane model is an article in *Dynamics of Atmospheres and Oceans* by Harlander and Maas (2007), who used both analytical and numerical (eigenvalue) approaches to investigate the solutions of a kind of Boussinesq model examined by Stern (1963). It is not immediately obvious to me, however, how their theory fits to our understanding of the equatorial waves. Rather their study might be considered as a certain approximation to the oscillatory solutions of a rotating fluid in a thin spherical shell, the subject of which was actively perused during 1970’s by applied mathematicians, such as K. Stewartson, as a class of hydrodynamic problems.

Of course, if the need arises to find out how important the effect of cosine Coriolis terms is in the equatorial region, the time-dependent approach would be useful as long as one focused on a specific phenomenon such as the Madden-Julian oscillation. I shall discuss next why the cosine Coriolis terms are important in the next-generation atmospheric modeling for weather and climate.
As it is well known, the dawn of the numerical weather prediction era was the epoch-making result of efforts from the mid-1940s to 1950s by a group of meteorologists, mathematicians, and computer engineers at the Institute for Advanced Study in Princeton, led by John von Neumann and Jule Charney. They proposed to attack the problem of numerical weather prediction (NWP) through a step-by-step investigation of models designed to approximate more closely the real properties of the atmosphere. In the spring of 1950, the first successful NWP was performed with the barotropic vorticity equation on the Electronic Numerical Integrator and Calculator (ENIAC) at Aberdeen Proving Ground, MD.

One important factor of the Princeton group’s success in NWP, beside the availability of electronic computers such as ENIAC and IBM 701, seems to be the recognition of different spatial and temporal scales of motion governing weather systems and a suitable simplification of the basic thermo-hydrodynamic equations by rational approximations to describe motions of a specific weather phenomenon. This is a valuable lesson learned from the heroic attempt of Lewis Fry Richardson in 1920s, who formulated a mathematical method of NWP and performed a hand calculation using meager observational data then available. It was no surprise that his “forecast” (actually, a pressure tendency calculation) was unsuccessful and discouraged any followers. Explaining why the Richardson’s rational approach failed seems to describe the history of NWP. Several major breakthroughs were necessary to produce today’s successful weather forecasts. The remarkable innovation of Richardson, however, was that he had to consider many essential aspects of NWP that future workers in this field had to face and he laid groundwork for their success. It was very fortunate for me, as I described in Section 6, that I was able to test, working with Warren Washington on the development of a general circulation model, the soundness of Richardson’s dynamical formulation which is based on what is now called the primitive equation model.

In 1955, the National Meteorological Center, Washington, D.C., began to issue numerical forecasts on operational basis equipped with the IBM701. I described in Section 3, my experience of using their computers located at the Federal Office Building No. 4 in Suitland, MD. Their early forecast models were based on, along with the barotropic vorticity equation, various quasi-geostrophic approximations to filter out the fast-moving inertia-gravity (IG) motions whereby a longer time step could be taken. The effect of baroclinicity was included by allowing the vertical variation of flows at two or three levels. However, they began to discover certain deficiencies in the
forecasts and started to improve the model formulations. One improvement was to replace the quasi-geostrophic assumption by the non-divergent assumption, deriving the horizontal wind from a stream-function field calculated from the geopotential field by using the so-called “balance equation.” But, to solve the balance equation, it is necessary to satisfy its ellipticity condition. The problem was that the geopotential field sometimes had to be adjusted beyond a realistic range. In fact, forecasts based on the balanced model were no better than those from the quasi-geostrophic models.

During 1950’s I was working on how to solve the balance equation and curious about the nature of ellipticity condition. It turned out that, during 1980’s the question of the ellipticity condition came back in connection with the nonlinear normal mode initialization of primitive-equation models. The quality of objective analyses in 1950’s was too poor to find out whether non-elliptic regions exist or not, but I thought that the FGGE level IIIb analyses were good enough to examine this question. As I described in my MWR article (#59, 1982), non-elliptic regions are ubiquitous in the tropics, indicating that the assumption of non-divergence is not valid at all for tropical motions.

Another deficiency of the quasi-geostrophic (QG) formulation was that the QG models gave rise to too rapid westward movement of planetary-scale motions with longitudinal wave numbers, say 1 through 3, and a large latitudinal extent. As noted by Burger (1958) and Phillips (1963), the dynamical explanation for this excessive westward retrogression of ultra-long waves is the following. For ordinary long-waves of order 1000km in scale, the quasi-geostrophic wind is essentially non-divergent. However, the ultra-long waves with order of 10,000 km in scale, the divergence has the same order of magnitude as the vorticity due to the variation of Coriolis parameter appearing in the geostrophic wind formula. Thus, the assumption of non-divergence in the formulation of the QG models is simply invalid for ultra-long waves.

Toward the end of 1950s it became clear that the only clean way to get out of the dilemma of QG (or quasi-nondivergent) model formulation is to go back to the primitive equation (PE) model. Actually, Jule Charney had proposed the use of PE model in 1955, and also experimental developments were being carried out by Karl Heinz Hinkelmann, Joe Smagorinsky, and Fred Shuman and others in late 1950s. However, there were many hurdles to overcome in these efforts. Because a short time step was needed to integrate the PE models, more computing power was necessary. New numerical schemes had to be developed to integrate the PE models. Table 1 shows a quick look at the transition from the QG to PE eras.
The early experiments with PE models were carried out without consideration of moist processes, because once the effects of condensation heating were included, the model integrations kept blowing up. In fact, I was one of the researchers who experienced the difficulty in running a moist version of PE model. The solution to the question of how to run the PE models stably with condensation heating was made by researchers in two camps, one in the formation of tropical cyclones and the other in the development of atmospheric general circulation models. Because I feel this is an important topic worthy of in-depth discussion, as I have already mentioned in Section 3, I have written about it in an essay “On the origin of cumulus parameterization for numerical prediction models” (#84, 2000).

The other important topic regarding a successful transition to PE models for NWP is the question on how to set up initial conditions in such a way that predicted flow patterns are well-behaved (free from extraneous noise). The process of adjusting the input data to ensure dynamical balance between the mass and wind fields is called *initialization*. Interestingly, this question never came up during the development of atmospheric general circulation models, because the time integrations for climate simulation were started from the atmosphere at rest. However, an operational solution of the initialization was central to the practical transition in forecast models from QG to PE models during the 1970s.

Generally speaking, atmospheric disturbances described by the PE models fall in two categories. One is the high-frequency IG motions and the other the low-frequency meteorologically significant motions of Rossby-wave type. Therefore, depending on the input data, if the energy contained in the IG motions is too large, the meteorologically significant motions would be masked by the IG motions while the time integration proceeds. This phenomenon was first analyzed theoretically by Hinkelmann (1951). In fact, the discouraging result of Richardson (1922) had a root in this phenomenon as he obtained the surface pressure tendency of 145 hPa in a 6-hour interval!

During approximately 10 years in 1960s to 1970s, at the National Meteorological Center (NMC) in Washington, D.C. the PE model developed by Shuman and Hovermale (1968) was used for NWP of the Northern Hemisphere. What was unique about their forecast procedure was that the initial conditions were obtained from the so-called Hough analysis. Tom Flattery (1971) developed an objective analysis scheme for observed data using the 3D expansion functions that consist of the product of the normal modes of the Laplace tidal equations in the horizontal and empirical orthogonal functions in the vertical. I have already mentioned the Hough
functions in Section 10, but I would like to talk about the practice of Flattery analysis at NMC as an interesting history of data initialization.

Hough functions are the normal mode solutions of Laplace tidal equations (global linear shallow water equations). They were first investigated in the late 18th century by Margules and Hough, but the complete picture of the solutions became only clear through the use of a high-speed computer by Longuet-Higgins (1968). Independently, Tom Flattery in the mid-1960s worked on the Hough functions as his PhD thesis at University of Chicago under George Platzman. Flattery finished his thesis in 1967, joined NMC, and applied his knowledge to design the Flattery analysis.

One problem with the Flattery analysis was that the divergence calculated from the wind field was practically zero. This was because Flattery used only the external rotational mode solutions as the expansion functions, with no linkage in the vertical due to the use of empirical orthogonal functions. Thus, observed data were often altered beyond observational errors by this procedure and forecasters were often not happy with the quality of analysis. This led to the need for an improved data assimilation technique at NMC and a new method based on the multivariate statistical interpolation was developed (McPherson et al., 1979) in preparation for global analyses of data collected during the World Weather Watch (First GARP Global Experiment) in 1979. An irony of this change in the analysis procedure, however, was that the balance between the wind and mass fields became worse compared with that in the Flattery analysis and the need of improved data initialization became a critical issue.

While various initialization schemes were developed at NMC that reduced mass-wind imbalance, an imaginative solution was proposed by Bennert Machenhauer (1977) and, independently, by Ferd Baer and Joe Tribbia (1977), which is now known as the nonlinear normal mode initialization (NNMI). The basic idea of NNMI is the following. Since our interest was to predict slowly varying large-scale weather, we clearly needed to prepare the initial conditions describing slowly varying motions such as Rossby waves. However, since the PE prediction equations are nonlinear, we can’t avoid the generation of fast-moving IG motions. Therefore, it was better to leave some components of the fast-varying motions in the initial conditions in such a way that those small components don’t grow in time. The trick, however, is how to achieve this result for a particular forecast model on hand.

A question arises at this point as to what relationship exists between the NNMI and the classical balancing based on QG assumptions mentioned earlier. Since the principle of the NNMI was established based on the spectral (normal mode) form of prediction equations, we need to choose a
particular prediction model to see how this procedure works in physical space. For a Boussinesq tangent-plane baroclinic model, Leith (1980) clarified a connection between the NNMI and the classical balance procedure based on QG theory. Some of his notable contributions are the introduction of concepts such as “slow and fast manifolds” for the collections of fast (IG waves) and slow (Rossby waves) frequency normal modes and the use of graphical interpretation of NNMI procedures.

The development of NNMI was an important factor in a successful transition from the QG to PE era in the evolution of forecast models, in addition to another important factor -- the ability to handle condensation heating, that I mentioned earlier. Obviously, those two factors had to be merged as diabatic initialization as I described in Section 12. Related to the history of NNMI, I should mention that the 10 year period of 1970s to 1980s was an exciting time for me at NCAR, having discussion with such able colleagues, Roger Daley, Bob Dickinson, Ron Errico, Joe Tribbia and Dave Williamson as well as Chuck Leith who all had engaged in research on various aspects of the NNMI.

The application of NNMI requires the construction of normal modes for a particular prediction equation system. While the construction of normal modes is feasible for a global or hemispherical PE model, the task is not straightforward for limited-area models. In this respect, I should add here my collaboration with Heinz Kreiss of University of Uppsala and Jerry Browning of NCAR Computing Facility. I knew Heinz, an applied mathematician, from my time visiting Stockholm during 1970s. I worked with Jerry for the construction of a limited-area model and the question of data initialization came up. So, Jerry and I talked with Heinz about how we should design an initialization scheme for the limited-area model. As a result, three of us ended up working together to develop an initialization scheme for a limited-area shallow-water model by applying the bounded derivative method (BDM) of Kreiss (1980). We summarized this work in an article, published in *Journal of Atmospheric Sciences* (#55, 1980).

The basic idea of the BDM is the following. The solution of atmospheric hyperbolic PDE such as the shallow-water model contains fast and slow time scales. Unless the initial conditions are suitably adjusted initially, the fast time-scale motions that develop during the time integration would overwhelm the slow time-scale motions. Therefore, it was necessary that the time derivatives of fast time-scale motions be reduced initially to the same order of magnitudes of the time derivatives of slow time-scale motions. The requirement of reducing the time-derivatives of fast time-scale motions gives rise to constraints that should be satisfied in the initial conditions.
After learning about the BDM, I became curious about the relationship between the approaches of NNMI and BDM for initialization. I knew that Leith (1980) showed that the NNMI is equivalent to the classical balancing based on QG assumptions and I thought that the BDM must be connected to QG modeling too. And, in fact, the application of the first-order NNMI and the second-order BDM to a baroclinic PE model in beta-plane geometry gives rise to initialization schemes which are identical (to the degree of certain approximations) to the classical balancing procedure expected from QG formulations. Because the both NNMI and BDM are successive approximation procedures, higher-order initialization schemes can be developed and provide a physical understanding of the principle of initialization. I discussed the details in a review article and published in Reviews of Geophysics and Space Physics (#58, 1982).

I began writing this section with the intention of describing the transition of the atmospheric models for NWP to the PE baroclinic model from the barotropic vorticity and QG baroclinic models. As indicated in Table 1, during 1970s-1980s the global PE prediction system became the principal tool for both operational forecasting and general circulation research after overcoming two major hurdles: handling moist convection and data initialization. Incidentally, I co-authored with Warren Washington an essay describing how general circulation models were developed and what are their future research goals as Chapter 3 in the book The Development of Atmospheric General Circulation Models edited by Leo Donner et al. (#96, 2010).

During the decade of 1980s-1990s, we saw a good deal of improvement in the performance of PE models with increased computational grid-mesh resolutions together with advanced parameterizations for physical processes. The maturity of PE models enabled their use in a variety of forecast applications and this trend continued to another decade of 1990s-2000s. As a summary, I list in Table 1 the topics of application without going into details. Although I have not worked on any of the topics listed, I was fortunate to have had an opportunity to write a narrative on these topics with Masao Kanamits of Scripps as a contributed article for Encyclopedia of Physical Science and Technology (# 86, 2002). Unfortunately, Masao passed away in 2011 and I miss him.

Now, what comes next as the dynamical model for describing the atmosphere beyond the PE formulation? Actually, even within the framework of PE models, the vertical acceleration term should be included for high-resolution grid mesh calculations. However, any computational short-cut, such as implicit or semi-Lagrangian method to overcome the need of a short time step in the vertical, may negate the benefit of including the vertical
acceleration. It is my impression that unless the ratio of vertical versus horizontal scales is very large, the other non-hydrostatic term due to the horizontal component of Earth's rotation is more important than the vertical acceleration term.

Looking ahead to designing future global atmospheric models that include the whole atmosphere, it is best to return to the Euler form of the original equations of motion from which the PE model was derived through the use of the shallowness approximation. While it is not straightforward to calculate the gain in accuracy of dynamical model by switching from the PE version to the deep nonhydrostatic version, it is likely that we gain at least on the order of 1% accuracy in energy in various physical aspects. Although the 1% gain may not sound very much, the cumulative effect of various 1% errors could be substantial. As I have discussed in Section 15, the most obvious gain seems to come from the inclusion of cosine Coriolis terms, but there are many others, such as allowing the variation in height of horizontal cross-section area and of the value of gravity, which improves the assimilation of satellite observational data and angular momentum calculations. Thus, for the design of the next-generation Earth system model for weather prediction and climate assessment, it seems imperative to eliminate both the shallowness and traditional approximations and adopt a dynamical core based on the nonhydrostatic deep (NHD) formulation. Global atmospheric circulation models based on the NHD formulation have been developed at the U.K. Met Office (White and Bromley, 1995: Staniforth and Wood, 2008) and the Frontier Center for Global Change in Japan (Satoh et al., 2008).
19. Epilogue

It was 2:46 p.m., Japan time, on March 11, 2011 when a record-breaking huge earthquake of magnitude 9 on the Richter scale occurred at the seafloor 130 km off shore of Miyagi prefecture in the eastern Japan. As soon as we heard the news, we phoned our relatives in Tokyo. We found that they were safe, but they were really in a stage of shock. Tokyo is about 400 km away from the epicenter. So, the damages were relatively minor there due to their diminished intensities at long distances. However, greater physical damages had occurred in many cities along the east coast of northern Japan. Moreover, tidal waves induced by the earthquake brought an unprecedented devastation with many thousands of dead and injured and even a larger number of missing people. Unbelievable hardships were brought by water damage in the nearby nuclear facilities of electric power generation.

Sad news of this earthquake once again reminded me of the difficulty of predicting earthquakes in contrast to weather forecasting. During my school days in the late 1940s at the Geophysical Institute, University of Tokyo, the discipline of seismology had been well established based on theories of elastic wave propagations and Earth dynamics. I even thought that the prediction of earthquakes might become possible in the future. In contrast the curriculum of meteorology at that time was a fledging branch of geophysics, because, despite a long history of meteorology, weather forecasting was more an art than a science. The dramatic emergence of modern meteorology since then that made reliable one-week weather forecasts possible seems to have benefited by the following three factors: One was the discovery of planetary waves from upper-air observations and a successive refinement of dynamical models for their description starting from a rather simple model as I described in Section 18. I am not aware of any history of a similar modeling hierarchy in seismology that describes a slow movement of the Earth's crust leading to the build-up of stress that eventually causes faulting.

The second factor behind the advance of numerical weather prediction (NWP) was the advent of electronic computers, although any quantitative study of science could have taken advantage of these computers. However, for the development of NWP the good timing of their invention was no accident. It is well know that John von Neumann at the Institute for Advanced Study in Princeton, NJ had set up Electronic Computer Project in 1947 with a specific objective of applying electronic computers to weather forecasting. The first successful barotropic forecasts were made in 1950 using the Electronic Numerical Integrator and Calculator (ENIAC) as I described in Section 18. I also mentioned in Section 3 my own experience of using a commercial computer IBM701 installed in 1954 at the Federal Office
Building No.4 in Suitland, MD. The capacity and speed of IBM701 were no match with those of today's even cell phones, while the physical size of the machine with a large number of vacuum tubes was enormous. We had to squeeze the instructions and data into a small size of core memory for maximum use of the machine capability. It is interesting to point out, however, that the same practice of maximum use of computer capability has not changed in time as the scientists undertake bigger and bigger problems and squeeze their programs and data into ever growing machine capacities.

Since NCAR created the Computing Facility in 1964 equipped with the Control Data Corporation (CDC) 3600, the organization maintained state-of-art supercomputers, upgrading periodically to a CDC6600, CDC7600, CRAY 1-A, CRAY X-MP and so on. I believe that the last CRAY computer I used extensively was CRAY C90, which was replaced by IBM computers in 2002. Those NCAR CRAY supercomputers were designed by Seymour Cray who dedicated himself to build the machines for atmospheric scientists to fulfill von Neumann’s dream of advancing weather forecasting. I always thought, as did by a noted atmospheric general circulation modeler, Joseph Smagorinsky, that supercomputers should be collocated with the numerical modelers to encourage close collaborations with the staff of the computer operations. But, the next-generation supercomputers at NCAR finally grew to the point that it was no longer physically possible to install and maintain the computers at NCAR's Mesa Laboratory, where they had been housed from the beginning. To continue to provide advanced supercomputing to NCAR and the university atmospheric scientists, UCAR and NCAR developed the NCAR-Wyoming Supercomputing Center located in Cheyenne, WY.

The third factor in support of the advance of NWP is the development of satellite observations and large-scale field programs. The first weather satellite, TIROS I, was launched in 1960 and revolutionized the way we observe the atmosphere. Prior to the satellite era, atmospheric observations were sparse and there were large gaps in our view of global circulation systems. However, the meteorological observations from satellites are different from the traditional synoptic and radiosonde observations so that the satellite data were not fully exploited until recently, when advanced 3-D and 4-D variational data assimilation techniques became operational. Because these data assimilation techniques require accurate short-term forecasts, there is a synergy between improvement in the data assimilation and refinement in the prediction model used for data analysis.

The current global observing system is the outgrowth of networks established in 1979 as the epoch-making Global Weather Experiment (GWE) under the auspices of the Global Atmospheric Research Program (GARP). GARP was an international scientific research program initiated in 1967 and
led by the World Meteorological Organization (WMO) and the International Council of Scientific Unions (ICSU). The purpose of GARP was to increase our understanding of the transient behavior of global atmospheric circulations that control changes of weather. GWE was the largest scientific international experiment ever attempted to observe the atmosphere, using every available instrument and platform. GARP also organized many large-scale international field experiments such as the GARP Atlantic Tropical Experiment (GATE). The objective of GATE was to conduct detailed atmospheric and oceanic observations in the tropics to understand the mechanism of transferring energy released in small-scale cumulus convective systems as condensation heating to the global circulations. NCAR scientists played the major role in planning, organizing, and conducting the field program, working with the scientists of the universities and laboratories. These international efforts were necessary and beneficial, because the atmosphere has no boundaries. Thus, international cooperation has always been the hallmark of modern meteorology.

I have been at NCAR more than 50 years. NCAR has been a wonderful host to my research career. Since I observed the progress of modern meteorology all along as I have described, I feel that I should comment on how NCAR has grown from a start-up organization to become the national center of atmospheric research. When I joined NCAR in the summer of 1963, there were already more than a dozen of staff members in the group of dynamical aspects of atmospheric circulations. Our offices were located in Cockerell Hall, a dormitory on the University of Colorado campus. I soon met with many newly joined staff members, Jim Deardorff, John Brown, Chester Newton, Warren Washington, Harry van Loon, Henry van de Boogaard, Yoshi Nakagawa, and many short- and long-term visitors, including Ragnar Fjørtoft from Norway who was well-known as the inventor of a graphical method of solving the barotropic vorticity equation. In the following year, many scientists joined Laboratory of Atmospheric Sciences (LAS), including Doug Lilly, Dave Houghton, Ned Benton, and Walt Jones. Also, Will Kellogg joined as LAS Director succeeding Phil Thompson. Phil initiated the Advanced Study Program (ASP) and became its first Director. Soon Bernhard Haurwitz joined ASP and I got to know him well too. The ASP is one of the most successful programs in UCAR, fostering many visitors, managing various fellowship programs, and conducting colloquiums. In fact, Dave Williamson visited NCAR every summer since 1963 under the Graduate Fellowship Program and studied at MIT. After receiving a PhD, Dave joined NCAR as a staff member in 1969 and I began a long history of collaboration with him. Until the NCAR Mesa Lab building was completed in 1967, our offices kept moving to various locations, including the upper floors of Joslin's department store building on Walnut Street in downtown Boulder.
Within 10 years the build-up of core scientific staff at NCAR was well established and many scientists were working on various research topics, leading in a variety of ways to meet the objective of GARP in collaboration with the scientists of the universities and laboratories. Behind this striking success, I heard some criticism toward NCAR. At the occasion of the 25th anniversary of NCAR, Henry Lansford, a science writer, wrote in the August 1985 issue of Weatherwise that "When federal support for scientific research began drop off in the late 1960s and early 1970s, some university scientists saw NCAR as a competitor for federal funds." In 1972 a Joint Evaluation Committee (JEC) was formed by UCAR and NSF to examine the operations of NCAR. Upon receiving a directive from the committee in 1973, Director Firor decided to make a complete reassessment of the NCAR staff. John formed the Interim Appointments Committee (IAC) to review the senior scientific staff. Up until that time, the research scientists had no differences in their ranks (appointment levels). Although such a system worked well within the organization, it was rather difficult for outsiders to judge the quality and seniority of NCAR scientists. Since the NCAR scientists work closely with university members who have various titles (e.g. Assistant, Associate, and full Professor), it seemed appropriate for NCAR to have an appointment system and criteria for promotion somewhat equivalent to those of the leading member universities of UCAR. The charges to the IAC were to assess the quality of professional scientists in terms of their creativity, productivity, integrity, leadership and future potential and to represent them in terms of numerical ranking. The results were then used by the NCAR administration for assignment of various types of appointments to individuals by taking into consideration of references from outside evaluators. I was appointed as one of the six IAC members and we all worked intensively during the first six months of 1973. Chester Newton, as its Chairman, worked extra hard. This was the most difficult committee work I ever engaged in, but I learned a great deal on many fine points in performance evaluations. Today I understand that the NCAR appointment system is one of the most stringent ones compared to other research institutions. All senior-level scientific members take turns participating in the Appointment Review Group (ARG) that examines the nominations of candidates from various divisions of NCAR for recommendation to NCAR Director.

In 1974 Francis Bretherton was appointed as the new President of UCAR when the founding President Walt Roberts stepped down. Concurrently, Francis was also appointed as Director of NCAR, with John Firor serving as Executive Director. Francis was assigned to examine all aspects of NCAR science and facility by himself as NCAR Director. In fact, Francis also took the reign of directing our Atmospheric Analysis and Prediction Division (AAP) for the first year before he appointed Chuck Leith as its Director after reorganization. Before the reorganization, the AAP comprised of three
projects; GARP, Climate, and Small-Scale Analysis and Prediction. I was working in the GARP project and partly involved in the development of NCAR atmospheric general circulation model (GCM). By that time Warren Washington was taking the lead role of GCM development. Because the GCM development required large resources of computer time and manpower, Francis was particularly keen on learning about the details of the project, and called upon a committee with eleven principal staff members to review all aspects of GCM operations. It was called the GCM Steering Committee under Francis’s chairmanship. Warren Washington was Executive Secretary and I served on it as a member.

Unlike typical committee meetings, GCM Steering Committee meetings consisted of a series of seminars to review all aspects of the GCM operations and we met frequently many times over six months. At that time Francis was a highly regarded theoretician in fluid dynamics, but I had an impression that he didn’t work much on numerical time-integration problems such as the GCM and NWP. Because of his unique background, Francis asked us rather refreshing, but often pointed questions and he frequently caught us off guard. For example, one day I talked about the use of GCM for experimental global weather forecasts and I presented our five-day forecasts from the second day. Francis then asked me why I didn't show the results of the first day. I replied to him that there were large forecast errors due to the initial dynamical imbalance between wind and pressure fields, known as a data initialization deficiency. I still remember vividly that Francis was literally upset by my answer and said that "Why don't you work on fixing the problem!" Evidently he wasn't aware that the problem of data initialization was an urgent issue in operational NWP at that time. Nevertheless, it so happened that I took his advice literally and spent almost the next 20 years in understanding the nature of diabatic initialization problem, as I have described it in Section 12.

Actually, I very much enjoyed attending the GCM steering committee because it was strictly scientific in nature and no administrative matters were involved. As we went through the various meetings over time, I was impressed by Francis' ability to get the essence of problems that were relatively new to him and to make constructive suggestions. The details of our discussions were compiled and published in 1975 as an NCAR technical report entitled "Development and use of the NCAR GCM." Warren Washington ran the meetings effectively as Executive Secretary and he often tried to make the atmosphere of meetings more relaxed. At one meeting Warren brought three fireman's hats and put them in front of Francis' table. There was a label on each hat as UCAR President, NCAR Director, and Division Director, respectively. Then, Warren asked Francis which hat he would put on today. I was a bit nervous to see what would happen next, but
Francis burst into a loud laughter. The atmosphere of often tense meetings became more relaxed and friendly afterward.

In August 2007, UCAR/NCAR organized a special symposium to honor Warren's distinguished services to five U.S. Presidents, working as a member of the National Science Board which he chaired from 2002 to 2006 as well as his outstanding scientific achievements. I was asked to give a talk about my collaboration with Warren in early days of NCAR. In my talk I thought that it would be fun to recreate the Warren's three-hat scene during the GCM Steering Committee. So, I brought three fireman's hats and talked about this unique story. Figure 5 shows a picture taken after the Symposium. The three hats were worn by UCAR President, Rick Anthes, NCAR Director, Tim Killeen, and CGD Director, Jim Hurrell, respectively, as of that time. Incidentally, other than Francis Bretherton, the only person who served in these three positions was Rick Anthes, though not at the same time.
**Table 1. Landmarks of numerical modeling for weather and climate since 1950**

<table>
<thead>
<tr>
<th>Time Period</th>
<th>Events and Developments</th>
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<tbody>
<tr>
<td>1950</td>
<td>Charney, Fjørtoft, and von Neumann’s experiment at Princeton, NJ</td>
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<td>1950-1960s</td>
<td>Dawn of numerical weather prediction (NWP)</td>
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<td>NWP activities in many countries in the world</td>
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<td>Quasi-geostrophic prediction models</td>
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<td>Operational NWP in Sweden (1954), U.S. (1955), and Japan (1959)</td>
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<td>General circulation experiment (Phillips, 1956)</td>
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<td>1960s-1970s</td>
<td>Foundation of NWP and climate simulation</td>
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<td></td>
<td>Primitive equation models</td>
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<td>Numerical methods for solving prediction models</td>
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<td></td>
<td>General circulation modeling for the atmosphere and world oceans</td>
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<td></td>
<td>Concept of predictability</td>
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<tr>
<td>1970s-1980s</td>
<td>Global Atmospheric Research Program (GARP) period</td>
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<td></td>
<td>Establishment of European Centre for Medium Range Weather Forecasts</td>
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<td>First GARP Global Experiment (FGGE) -- Global Weather Experiment (1979)</td>
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<td></td>
<td>Global domain prediction models</td>
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<td></td>
<td>Multivariate statistical interpolation of objective analysis</td>
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<td>Data initialization -- Nonlinear normal mode initialization</td>
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<td>1980s-1990s</td>
<td>Improvement in physical parameterizations for the Earth system</td>
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<td>Dynamical extended-range predictions</td>
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<td>Coupled atmosphere-ocean-sea ice-land models</td>
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<td>Predictability of the atmosphere</td>
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<td>1990s-2000s</td>
<td>Severe storm predictions</td>
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<td></td>
<td>Expanded earth observing systems -- Advanced data assimilation methods</td>
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<td>Ensemble forecasting -- Estimate of forecast reliability</td>
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<td>Whole atmosphere prediction models</td>
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<td>2000s-beyond</td>
<td>High resolution numerical models - nested regional modeling</td>
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<td>Detailed physical processes for NWP and climate simulation</td>
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<td>Theories of Earth’s climate variability</td>
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<tr>
<td></td>
<td>Non-hydrostatic deep global modeling for the atmosphere and oceans</td>
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</tbody>
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A1
Figures 1 – 5

Fig.1.
Prof. J. Holmboe and I posed in front of the building of Department of Meteorology, UCLA. Date: June 18, 1954.
Fig. 2.
My family members with wife, Yuko and daughter Alice on the baby carriage met George Platzman and Robert Simpson (far right) on the campus of University of Chicago, dated July 6, 1959.
Fig. 3:
A dinner party during the First International Numerical Weather Prediction (NWP) conference in Tokyo, November 1960.
Fig. 4.
Group picture taken during the Study Conference on Parameterization of Sub-grid Scale Processes, March 20-27, 1972 at Main Geophysical Observatory, Leningrad, USSR, arranged by the Joint Organizing Committee (JOC) of Global Atmospheric Research Program (GARP). From front to back and left to right:
Fig. 5.
From left to right three fireman's hats were worn by then NCAR Director, Tim Killeen, CGD Director, Jim Hurrell, and UCAR President, Rick Anthes together with Warren Washington and myself. This picture was taken after a special symposium in honoring Warren in August 2007.
List of principal publications by Akira Kasahara


31.--------, and D. D. Houghton, 1969: An example of non unique, discontinuous


52.--------, 1979: Toward achieving the goal of atmospheric scientists-understanding our atmosphere. *Atmospheric Technology*, National Center for Atmospheric Research, **11**, 4-10.


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