AUTOBIOGRAPHY

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Formative Years—Early Love of Weather

As I look back on my life and career of the past 75 years, I feel that I have been especially lucky. Perhaps the most fortunate event was being able to meet and later to become a student, colleague and friend of Carl Gustav Rossby, whom I consider to be the man of this century in meteorology. He set me on a path of excitement and gave me a philosophy that led to achievement, and equally important, set my standards of behavior and ethics.

My earlier years through high school were spent in Fall River, Massachusetts, a city of about 125,000, which was known chiefly as the scene of the Lizzie Borden axe murder and for the vast number of textile mills. French Canadians, Englishmen from the Lancastershire district, Poles, Portuguese from the Azores, and a number of Irish worked at these mills, often for 12-hour days. Many of the local population had come from other U. S. cities, hoping to succeed among the textile workers. My father was one of these. He came to Fall River seeking to make a better living by being an optometrist for the factory workers. Although many of them spoke little English, my father had no trouble communicating because of his knowledge of Portuguese, which he had learned from his father, a merchant in the Azores. The local Portuguese thought my father spoke their language well.

Being in such a cosmopolitan group in school had advantages, especially since the Fall River schools and teachers at that time were dedicated to excellence. Our high school was Durfee, a school of about 1600 students. I was fortunate to have superior math and physics instructors who instilled a curiosity for scientific knowledge. My English, Latin and romance language teachers were also among the best. My older brother had been an excellent student a few years ahead of me, and I was continually reminded of my responsibility to carry on his outstanding work. Perhaps the most stimulating of all my instructors was a physics teacher, who made special efforts to explain some of the intricacies of meteorology, along with his theories and the suggestion that I might seek a career in the U. S. Weather Bureau. This lead to a day-to-day search of our public library for anything and everything bearing on meteorology. The town's amateur meteorologist, who was an unpaid cooperative observer for the U. S. Weather Bureau, as well as a wealthy broker, learned of my interest and spent considerable time advising me. It wasn't long before I had set up my own weather station, using instruments bought with hard-earned cash. I began keeping records and drawing weather maps from reports published in the daily newspaper. I soon made forecasts for my friends, occasionally comforted by the fact that the official forecasts were also wrong from time to time. Fortunately, I had not yet learned about probability and statistical verification, otherwise I might have left the pursuit of meteorology as a career.

Later on, during high school, I found out that an American Meteorological Society existed, having been founded by Professor Charles F. Brooks in 1919. Since the requirements for membership were modest—a sincere interest and annual dues of $2 per year—I joined and thereby began to receive monthly Bulletins which kept me informed of developments and outstanding figures in the field.

While high school friends thought I was getting into this field because of the money (the image of the cooperative observer as a millionaire), my father was worried because he saw little or no chance of my making a living. He tried to persuade me to take up optometry, his profession, but this did not appeal to me—besides, I couldn't speak Portuguese! He did persuade my brother, who went on to become a nationally known figure in the academic and clinical field. Later on, my father changed his low opinion of meteorology as a bread-winning profession, and became one of my best public-relations protagonists.

It still was an enigma as to how I could enter this field. The Weather Bureau was about the only organization involved in meteorology, although a first new department of meteorology had just been established at M.I.T. by Rossby—but this was a graduate department. Although I had been offered a four-year scholarship to Wesleyan University in Connecticut, a protracted illness of my father made it wiser for me to help out at home. My mother was so dedicated to our family, that I felt obliged to stay with her, especially since my brother was studying and working in New York. My earning power was restricted to house-to-house selling, and making some money as a drummer in a jazz band. About a year after graduation from high school, I became ill with tuberculosis and had to temporarily abandon the idea of going to college. However, the confinement during my illness gave me time to study and especially to take many correspondence courses including college physics, math, and an excellent course in meteorology given by Clark University under Professor Charles F. Brooks. Later on, I took courses in German, English composition, and even chemistry—all by correspondence! My voracious study and reading encouraged me to write letters to the experts in the field, asking for a job, citing my study of their papers and books, and even enclosing my picture. Polite letters came back with many excuses why nothing could be done with the limited funds available. After all, this was the middle of the Great Depression. Who wanted a student meteorologist, especially one with no formal qualifications? Finally, a famous man, H. H. Clayton, wrote that I might come to see him at his home near Blue Hill Observatory outside Boston. I took the first available train, and there I was—talking to a real, live meteorologist in his home.

He told me about the research he was conducting for the Smithsonian Institution and asked if I was interested in helping uncover and collect some data in Washington. After the obvious reply, he said he would have to test me. He proceeded to supply a number of weather maps from which I had to extract certain pressures from the isobars. This done, he compared his own values with mine, and found that we had each made one mistake—one success for him and one for me. Could I leave for Washington on the weekend?

I returned to Fall River where my father obtained tickets for me—an overnight boat to New York and then by train to Washington. For the few nights before the trip, I got very little sleep—after all, I hadn't been more than 50 miles from Fall River, and Washington, D.C., was the Nation's Capital!

In my very best wool suit, I arrived in the midst of a June heat wave, was put up in an excellent boarding house by a good friend of Clayton's, Prof. S. P. Fergusson,

and proceeded to go to the Smithsonian Institution on the Mall. Armed with a letter of introduction to Dr. Charles G. Abbot, Secretary of the Smithsonian, I approached one of the guards who, on hearing my request to see Dr. Abbot, tried to get rid of me. With perseverance, I got to see the head of the guards, who decided my letter was kosher and took me to Dr. Abbot, who treated me most warmly, discussed with me the scientific nature of the work, and helped me find a place in which to work. Actually, it had to be at the Weather Bureau Central Office, because most of the data were there. This gave me an unparalleled opportunity to search the fine library and discover work that I hadn't known existed, and also to meet in the flesh many of the great names whose work I had read—among them, Dr. W. J. Humphreys, author of the famous American textbook "Physics of the Air." Most of my time was spent compiling data for Clayton's research, and I found many volumes of foreign weather maps in the process. The data were to be employed for two purposes: compilation of world weather records, an internationally known series put out by the Smithsonian; and secondly, for solar-weather studies, which were foremost in the minds of Clayton and Abbot.

Later on in my career, knowing the limitations of the data I had helped gather in Washington, I made the decision to stay out of solar-weather work—a decision I have not regretted.

Besides the discovery of many meteorological volumes in the library, I found the many scholarly papers of the Norwegian or Bergen School. These opened my eyes to physically based explanations of nature's wind and weather-making processes. Meteorology was no longer merely the geometry of squiggly lines on maps! I also found the initial set of scientific reports issued by Rossby's newly founded department of meteorology at M.I.T. I spent many a night trying to wade through these reports with my limited knowledge of math, physics and meteorology. Frustrated at one point, I wrote a letter to Rossby politely questioning a couple of his statements in a classical paper, "Thermodynamics Applied to Air-Mass Analysis." Shortly thereafter, I received a response from him saying that I was correct on one point but in error on another, together with a full explanation. In a P.S., he asked in essence who I was and would I drop in to see him the next time I was in the Cambridge area. I needed no urging, and in a month or so, had the pleasure and opportunity of meeting the person who was to influence my entire life thereafter.

The Euphoric 1930s at M.I.T.

With my unorthodox background, Rossby had to make a "special case" of me so that I could concentrate on subjects germane to meteorology—mainly in the graduate department, but also in the physics and math departments. He also made it possible for me to earn money evaluating the recordings of the research aircraft instruments used by the department at the East Boston Airport; occasionally I was allowed to fly as an observer. This meant getting up at 4 a.m., travelling to the airport by subway (4 changes), evaluating records made in flight, and having breakfast while listening to the fascinating stories told by Dr. K. O. Lange, an instrument specialist who had been brought over to M.I.T. from Damstadt, Germany (their equivalent of M.I.T.). Lange had studied the best of the German methods of instrumentation, and was an expert on their new meteorographs, which were designed to probe the upper layers of the atmosphere up to 5 or 6 kilometers. He also was an expert in gliding activities, which the Germans had developed after World War I because they were forbidden by the Treaty of Versailles to employ motorized aircraft. Later on, the vast

supply of glider pilots formed the nucleus of Hitler's Luftwaffe. Lange, himself, and a colleague from Darmstadt, were National Socialists and strong advocates of Hitler. I was often given propaganda talks slanted to my own background. Obviously, my Jewish upbringing didn't make me a good target! Nevertheless, I learned a great deal of Germanic scientific precision from Lange, though I felt that his political endeavors were enigmatic, to say the least.

After my first year at M.I.T., Rossby asked me to assemble some data for research by taking pilot balloon observations at East Boston Airport for about four weeks. This involved the well-known trek to and from the airport and essentially 14-hour work days taking balloon runs with the help of a theodolite to determine the wind directions and speeds at various altitudes. Rossby told me that this data would throw light on the well-known but little understood sea breeze, although I knew his primary interest was in getting data for his studies on mixing length and the Ekman layer. On one occasion, a home town friend came to visit at the airport, and I encouraged him to watch me send up the pilot balloons. He came into the shed where the balloons were inflated. Shortly thereafter I noted that he was smoking—while I was inflating the balloons from a tank of hydrogen! After the series of runs was completed, I found that on only one day did the sea breeze arrive—it turned out to be 200 feet thick.

I turned over all the data to Rossby, assuming that it would help in his research, and then returned to Fall River to continue more computations for H. H. Clayton and earn money to enable me to carry on at M.I.T. in the fall. While I was in Fall River, a telegram arrived from Rossby, who was now at the Woods Hole Oceanographic Institution for the summer. My mother was the person who opened the telegram and she almost fainted, because at that time, a telegram usually meant a death in the family. In the telegram, Rossby asked me to come to Woods Hole as soon as possible, if Clayton would agree. So I took the bus to New Bedford, Massachusetts, and then the boat to Woods Hole, arriving very early in the morning. No one was there yet at the only oceanographic building. Finally Rossby arrived, and after inquiring about my trip, told me that he couldn't make sense out of my extensive tables of wind velocities gathered from the balloon runs. This did not surprise me, as I had never been known for neatness, and my organizational ability for research was yet to be developed. I then proceeded to rework and recopy all the data in an organized and neat way. After about a week, I proudly took them to Rossby for his research. However, in the process of reworking the data, I discovered some interesting properties involving the fine structure of the winds in thin homogeneous layers. Rossby seemed to be intrigued and asked if I would like to stay around and continue working on the data. This enabled me to get to know and talk with a number of scientists at Woods Hole. Equally important, I became familiar with the Marine Biological Library (for biology and geophysics) which had recently been organized into fine shape by Ray Montgomery's mother. There were few meteorologists at the Institution then—Althestan Spilhaus, Ray Montgomery, Gardner Emmons, and of course, Rossby. At times I felt like a fish out of water, but the stimulation provided by others and the seminars made it most worthwhile. I was to return to Woods Hole in later summers; and even when stationed in Washington in subsequent years, managed to spend part of my summers in this wonderful scientific environment.

In the fall, I returned to my studies at M.I.T., and for a few hours a day, did computing for H. H. Clayton. He seemed to like my work and was happy to assist me in earning a few dollars. He was particularly impressed with some short-cut graphi-

cal methods I had developed for obtaining fast but satisfactory solutions to complex equations—methods I had learned from my earlier correspondence courses. I also carried on some early morning work at the airport before going to class.

Later in that school year, I became especially interested in the structure of air masses and fronts as determined by the rapidly expanding aerological network of airplane soundings and pilot balloon wind soundings. Occasionally we made several soundings through cold fronts that passed East Boston airport. These made it possible to construct cross-sections through the fronts by combining ascents in time and space made at other places to the west, like Detroit and Chicago. Some of the central ideas for doing this stemmed from the work of J. Bjerknes, who had pioneered aerological studies of cyclones over Europe with swarm ascents of sounding balloons; and also from the excellent research carried on at M.I.T. by Prof. Hurd Willett, an authority on American air masses and fronts. This research lead to my first major publication—a monograph in the M.I.T. Professional Notes Series. The monograph contained many cross-sections and analyzed maps detailing synoptic conditions of an active meteorological period in February 1932. Not satisfied with only synoptic-scale phenomena, I investigated the meso-scale structure of an especially strong cold front that passed over New England, leaving interesting upper-traces recorded in three airplane ascents made over Boston in one day. Hour-to hour variations in the speed of the front were explained on the basis of changes in the underlying topography. In addition, strong subsidence of air above the forward portion of the front was documented.

This monograph established my reputation as a capable analyst and raised my stock with Rossby and Willett. Shortly after the monograph was published, I began to get offers of positions by some of the airlines that were trying to establish meteorological departments. Rossby himself in the mid-twenties had already developed the first airways meteorological network in California (the Guggenheim Network), and Horace Byers had recently taken a postion with TWA to introduce modern synoptic methods. Rossby felt that I should get more practical experience by working with TWA, and of course, carry the M.I.T. banner. So I joined TWA, first at Newark, then at Kansas City. In these assignments, I got a taste of the real world—round-the-clock workshifts and stressful forecasting for early Ford trimotor transcontinental flights. Problems involved icing on aircraft, low ceilings, zero-zero visibility due to fog or even blowing dust over the Dust Bowl area, and hazardous winds. Although the job had its fascination and certainly pointed up many important research problems, there was no time for scientific work because the company meteorologists were continually on an operational treadmill. Besides, I got the distinct impression that airline meteorologists were considered second-class citizens around the air terminals and often served as scapegoats for weather-related accidents. Therefore, when TWA had to temporarily suspend operations following government curtailment of airmail contracts, I was happy to return to M.I.T. with part-time work at Blue Hill Observatory. Here I could devote time to scientific matters—even though the salary was a fraction of what TWA paid. Prices for meals and lodging during the Depression were so low that I did not require much money to get along. As for recreation, Willett introduced me to hiking in the Blue Hills and canoeing on the Charles River. In fact, he, Harry Wexler and I often took young ladies on dates together.

My research now focused on the topic of subsidence within the atmosphere, and it wasn't long before I presented papers on this subject at national meetings. A presentation at the AGU in Washington helped my image, as did a trip to Caltech where

Dr. Krick and president Dr. R. A. Millikan held a special conference on modern methods of meteorology. This was my first transcontinental trip; I was a passenger in a new Buick convertible that Willett had just bought with cash! At C.I.T., he and I presented papers upholding the reputation of M.I.T with extensive aerological studies. My topic, "Subsidence," drew a great deal of interest and also led to more offers of positions with airlines. I returned east by air, because Rossby asked me to help out at the National Gliding and Soaring contest at New York. M.I.T. had a scientific contingent there, consisting of Lange, as leader, instrument expert Chris Harmantas, radio operators Murphy and Arsenault, aircraft-glider pilot Hank Harris, and me. My job was to analyze maps, evaluate airplane soundings and present daily briefings to the glider pilots about the thermals and other factors important to the day's gliding activities. It was most exciting, especially when a distance record for the United States was made by Dick Dupont, of the famous Dupont family, and a most enthusiastic gliding pilot. The distance record resulted in large part from a forecast of a strong frontal passage in advance of which Dupont glided all the way to Boston. I still marvel at his courage—and at my colossal nerve in proposing such a dangerous flight path! But I had read about the German pilots doing this and Lange had told me many stories of this kind.

The social amenities at Elmira, the scene of the National Gliding Contest, left little to be desired. The townsfolk threw parties almost nightly, and those of us with the M.I.T. supporting team were always invited. On one occasion, there was to be a big outdoor affair complete with drinks and plenty of food, to be served on large tables on the vast lawn of one of the wealthier families of Elmira. Before the party was to start, sounds of distant thunder worried the hosts and the caterers. So the question was raised as to whether all the tables, chairs, food and other things should be moved inside the house.

Naturally, the decision was referred to me as the resident forecaster. After raising my finger to determine the wind direction, surveying the threatening sky, and substituting my watch crystal for my crystal ball, I recommended abandoning the outdoors and transferring everything inside. Not long afterward, the thunder decreased in intensity and the thunderstorm cell moved off, producing not a drop of rain in the party area! That little episode didn't help our image as meteorologists, and I had to work especially hard to counterbalance it. Perhaps unconsciously this experience may have influenced my decision in later years to opt for long-range forecasting.

The work on subsidence led to a monograph in the Harvard Meteorological Studies series, a piece of work that C. F. Brooks distributed widely throughout the meteorological communities around the world. During the first stages of World War II, the Air Force purchased almost 1,000 copies of this for their cadets. During the period I worked on it, very little was known, so I had an open field.

During this period, I found it necessary, or at least desirable to have more money. Realizing that the ideas being taught at M.I.T. were new to most American meteorologists and that they, like I, were seeking a more physical picture of weather phenomena rather than geometrical descriptions of isobaric patterns, I decided to write a series of introductory articles. The substance of these would be some of the material I had learned at M.I.T. and from extensive scientific reading, particularly papers of the Norwegian School. An ideal medium for publication was the Bulletin of the American Meteorological Society, one of the few American journals at the time. The Monthly Weather Review, a U.S. Weather Bureau journal, would not have reached the same

audience, and perhaps its editor would not have published such introductory material. At any rate, C. F. Brooks, secretary of the AMS, thought it an excellent idea, and Rossby concurred. In fact, Rossby was to take such an interest that he read over each article of the series and made helpful suggestions and corrections.

When the first articles dealing with stability and air mass properties appeared, their reception was gratifying. Apparently, I had touched upon a popular topic. Readers thought that the pieces were written in understandable language for practitioners who wanted a conceptual framework, not mathematical derivations. Soon Brooks received requests for hundreds of reprints from many groups, including the Weather Bureau and the military services.

The meteorological branch of the Army Signal Corps offered me a well-paying job to teach and expand upon the work for their personnel. In addition to new offers from the airlines, I was offered a teaching post at the newly established Parks Air College. As flattering as these offers were, I felt that in the long run, it would be better to remain at M.I.T. and Blue Hill Observatory where I could interact and learn from topnotch scientists with genuine research interests.

However, it was often made clear to me that my formal background was not adequate for a successful career in science—particularly in the academic arena. This was painfully clear because at that time, I did not have even a bachelor's degree. Thus, I decided to enroll as an undergraduate at the University of Michigan. This would be easier and less expensive than M.I.T., for tuition at Michigan was relatively low—about $400 a year. Michigan gave me advanced standing whereby I could take advanced courses in math and physics, as well as courses in German, English, astronomy, and climatology.

To help with finances, I obtained some work in map analysis under a new government contract of student aid. As a supplement, I gave some lectures to people interested in flying; on one occasion, I gave forecasts and advice to the Piccards in connection with a high altitude flight. Then again, I had to write occasional articles for the series in the AMS Bulletin. Actually, I had taken on more work than I should have. As a result, near the end of the school year, serious physical problems (pleural effusion) forced me to return to Fall River and once again proceed with correspondence course work and self study.

At this time, I became further interested in atmospheric inversions in general—not only those produced by subsidence. The work of Brunt and others on radiative transfer was especially appealing, so I reasoned that certain inversions might be maintained by discontinuities separating moist air below and dry air above the inversion, together with vertical mixing. After a great deal of work, I prepared a paper on this topic and submitted it to the Monthly Weather Review, where it was readily accepted and soon published. Former colleagues at M.I.T. and in Washington seemed impressed—even more so when a favorable review of the paper by C. K. M. Douglas appeared in the Quarterly Journal of the Royal Meteorological Society. This was highly unusual inasmuch as reviews were generally reserved for books, not papers. Shortly thereafter, Rossby offered me an assistantship beginning with the fall term at M.I.T. Nothing could have made me happier, for I was to embark on my truly professional career.

The particular opening in the M.I.T. department was partly due to the fact that Rossby had secured a government contract through the Bankhead-Jones Act—an act designed to assist agriculture. M.I.T. was to search for understanding of causes and maintenance of the Dust Bowl drought that was ravaging the American heartland.

Ray Montgomery, Irving Schell, and Larry Page (from the Bureau of Agricultural Economics) had already written a good review of long-range forecasting practices in the world. Only one school of research seemed to show promise, namely that of Dr. Franz Bauer in Germany. Thus it was decided to attack extended forecasting for the time scales of the order of a week, the period treated by Bauer. I was asked to join the team, along with Rossby, Willett, Larry Page, and Roger Allen (Weather Bureau). The walls of one of the offices were soon covered with hemispheric maps, mainly sea-level charts, but also sections of upper charts, and we began attempting forecasts of mean temperature and precipitation patterns over the United States. It soon became clear that none of us knew what we were doing, other than coloring charts with red and blue crayons. Nevertheless, we held map discussions once or twice a week, in which Rossby and Willett played the more active roles, while some of us, including kibitzers, tried to make intelligent comments. Rossby had just begun working on his theory of long waves in the westerlies and the associated positions and displacements of the great Centers of Action.

This theory involved the concept of redistribution of vorticity, in which the motion of the long waves as a function of zonal wind speed and wave length were principal parameters. Rossby would often try to explain alterations in the general circulation with the help of these ideas, gradually convincing Willett and me of their validity. But the ideas were largely used in a vague descriptive manner, except for occasional computations made by Rossby on the back of an envelope. Once this central idea was established, progress in five-day forecasting of average patterns of wind and weather over the United States developed rapidly.

Shortly after I arrived at M.I.T. in September 1936, I was asked by Dr. C. F Brooks to make forecasts in connection with the Harvard Tercentennial celebration to be held that month—forecasts in particular for the big outdoor event, and other outdoor festivities. To do this, I analyzed two sea-level maps each day, as well as some limited-area 10,000-foot charts. As luck would have it, a tropical storm system appeared off the southeastern coast a few days preceding the big day at Harvard, and I began to worry. In fact, my nights became sleepless, thinking about possibilities. These thoughts were confided to Dr. Brooks, who encouraged me to do the best I could.

Well, "the best I could" turned out to be not good enough. My prediction called for some light rain in the morning gradually increasing mainly at the conclusion or after the ceremony. While the term "light rain" might have been verified technically, at least for part of the period, what happened was a ruinous rain for the Tercentennial. Dr. Brooks commiserated with me and tried to buoy up my devastated spirits. Maybe it was light rain for awhile, but certainly my timing was wrong—an error not without parallel when a northeaster occurs, even in these days of sophisticated instrumentation and methods. Even though most of the forecasts for other days and events of the Tercentennial worked out well, I didn't win many brownie points with the Harvard authorities. I am grateful, however, that I am not involved with the forecasts for the 350th reunion coming up in 1986!

One of the main difficulties in applying Rossby's ideas involved the lack of data aloft, particularly over the oceans. Thus, we were working with a limited section of the hemisphere, with perhaps a half of one long wave. Later on, at Rossby's suggestion, I constructed a trial map by judicious extrapolations, estimating quantitatively the flow patterns aloft over the North Pacific and North Atlantic, as well as the United States, and the work of Rossby took on new meaning and application. His wave

equation could now be applied quantitatively, displacements computed, and stationary wave lengths determined. Translation of the wind patterns into associated quantitative specifications remained to be achieved a couple of decades later, largely through the indefatigable work of Bill Klein, with some ideas proposed by a number of us at the Weather Bureau's Extended Forecast Section. Nevertheless, a breakthough had been achieved in devising a system for predicting upper air flow patterns, revealing the core of the extended forecast problem. A couple of decades later, Rossby told me that his early period at M.I.T. (the 1930s) was the most satisfying and exciting of his entire career. Ever since, I have been happy that I was one of the players on this team, and that I am included in the "et al" contributors in the classical paper Rossby wrote in 1939 for the Sears Journal of Marine Research.

During the last half of the 1930s, my time was divided between teaching two classes at M.I.T.—advanced aerology and weather forecasting—and carrying on research relating to extended forecasting. Among the latter, isentropic analysis consumed a great deal of time and effort. As frequently was the case, this research stemmed from central ideas provided by Rossby. The concept of working with isentropic surfaces was a direct outgrowth of the earlier work done at M.I.T. with conservative properties of air masses, especially potential temperature and specific humidity, and as such, was the culmination of what might be called "the thermodynamic era." Work of a similar nature was being done in England with the wet bulb potential temperature, especially by Sir Charles Norman. This latter work served to stimulate Rossby to go full speed ahead with isentropic analysis. He always liked and perhaps needed a stimulus in the form of competition. After constructing hundreds of isentropic charts and associated cross sections of potential temperature, specific humidity and wind, along with the Montgomery stream function, I reasoned that isentropic analysis was an exceptionally valuable tool for precipitation forecasting, particularly when the moist and dry tongues were clear and easily identified. This condition occurred chiefly in the warm season. It soon became obvious that swarms of thunderstorms were embedded in the moist tongues where the deep moist air enhanced convection, unimpeded by entrainment with dry air aloft. Careful analysis of upper-air data led to ideas about the role of static stability, radiative balance, and particularly the question of deployment of the moist and dry air tongues. These studies led to an operational system for thunderstorm forecasting and, importantly, to the unorthodox idea that summer thunderstorms over the great plains of the United States did not occur haphazardly as "air mass" thunderstorms, but frequently moved in clusters within upper-air moist currents that flowed in great anticyclonic systems in midtroposphere. These winds often carried the high-level moisture southward into the southern states—even though the surface winds were blowing from the South. The publication of this and other related work won me the first Meisinger Award of the AMS in 1938.

It was in the mid-1930s that I began to go on dates with Edith Paipert, who was to become my wife in the fall of 1938. Harry Wexler, my best friend for many years since grammar school, had married her sister Hannah, so the introduction was quite natural. A few years earlier, I had introduced Harry to Rossby and the field of meteorology. As things turned out, this made for a nice relationship through many years, wherein we shared many trips and good times, including several bicycle trips to Cape Cod. Edith had been to art school, studied sculpture and painting, and thus had a wonderful feel for symmetry, balance and aesthetics—something which I certainly did not have. As I analyzed isentropic charts, sometimes in her presence, she would make comments as

to the artistic or often the artistically-jarring features I had produced. It soon became clear that parts of my analysis that she did not like were incorrect and could be made both more artistically satisfying and scientifically correct by modification. It was then that I realized the close association between art and science. In fact, in a couple of courses I taught at M.I.T., this philosophy was stressed, much to the chagrin of a few of my contemporaries. At present, there are societies in the world devoted to this concept. The American Academy of Arts and Sciences has just set up a small group of scientists and artists to find areas of commonality.

In September of 1938, Edith and I were married and took off for Bermuda on our honeymoon on the popular cruise ship "The Queen of Bermuda." Shortly after we set sail from New York, seas became rough, and it wasn't long before we went down to our cabin—not for love making but to rest out the storm! When we arrived in Bermuda, being a good meteorologist, I suggested that we ride our bikes (there were no automobiles allowed on Bermuda at this time) out to St. George's, the seat of the British meteorological station. There I saw on the weather map a vast and intense hurricane over southern New England—something I had not anticipated, and unfortunately, neither had the official forecasters. It was not until we listened to the radio that we became aware of the great devastation of that September 21, 1938 storm—an event that was to result in the establishment of the first Boston Weather Bureau Office and also offshore buoys in the western Atlantic. Lest the reader get wrong ideas, we had a wonderful time in our two weeks in Bermuda. However, on our return to Boston, no one had time to listen to our Bermuda tales; we had to hear out the unending stories of people's experiences during the hurricane.

Other work with isentropic concepts threw light on the nature and causes of the dust bowl drought. Harry Wexler and I discovered that the dry air invading the dust bowl area frequently arrived in great anticyclonic swaths flung off the midtropospheric westerlies along the United States-Canadian border. Some of this work also appeared in the published reports we submitted in connection with the Bankhead-Jones Contract. These reports contained a wealth of material relating to extended forecasting, and were written by a team composed of Rossby, Willett, Allen, Holmboe and me. Among other things, these reports demonstrated that our five-day forecasts had definite skill over and above climatological probability. In doing this, we had been among the first group to employ rigorous statistical techniques to verify weather forecasts.

Washington and the War Period

In the last years of the 1930s, it became obvious that war was probable and that preparedness measures were essential. For this reason, the military services became especially interested in our extended forecast work and assigned a few officers to the project at M.I.T. Among these was Captain Tommy Moorman, later General Moorman of the U. S. Air Force. The officers attended all our discussions and also made practice five-day forecasts.

In May of 1940, it was decided to shift the project to Washington, where it would be closer to defense preparations. This was also decided because the forecasts that had recently been made operational at M.I.T. for the Weather Bureau, could be more easily transmitted to Weather Bureau offices from Washington. I was asked to head up the project in Washington and take a one-year leave of absence from M.I.T. Hurd Willett came down for a few months to help insure a good start. Our reception by some of the Weather Bureau personnel was not exactly cordial—indeed, some seemed

hostile. It was pointed out that we "first had to learn to walk before we could run," so that attempting forecasts for a period of five days in advance was utterly foolish. After many encounters with reactionaries, but with the greatly appreciated help of the Chief, Dr. F. W. Reichelderfer, we got off to a reasonably successful start and attracted some young enthusiasts. Greatly helping in the first practical forecasting effort was Kenneth Smith, formerly with us at M.I.T. and a born forecaster.

The military officers attended our map discussions, and their team was expanded with additional air force and navy officers. Some of these had studied at C.I.T. under Dr. Irving Krick. Twice each week, I gave map discussions lasting about an hour before a large group and also conducted "post-mortem" discussions reviewing the forecasts made a week ago. Looking back, I regret that tape recorders were not then available, because the discussions were most illuminating and sometimes even hilarious. Some of the most vociferous people at the post-mortems had very skewed and selective ideas about what had been said about forecasts made the preceding week. The only prognostic charts drawn for these forecasts were by our own five-day forecast group, so it sometimes became a shouting match as to who said what. In spite of all these difficulties, we were able to turn out predictions of value, both for the national economy and for military purposes.

With the advent of Pearl Harbor, we were ready to supply forecasts for the military services, and expanded our forecast domain to the North Atlantic and Europe. The Air Force and the Navy occupied some of the rooms at the old Weather Bureau at 24th and M Street where we were housed, so there was good collaboration. Of course, much of the material, both data and forecasts, was classified and not furnished on direct circuits to Weather Bureau stations. The Air Force and the Navy decoded weather information over the oceans and in the theaters of war. Though this material was by no means ideal, it sufficed for our type of large-scale work.

There were several important developments in the early wartime period. First was the tremendous increase in upper air observations over areas which had been only sparsely covered or not covered at all. Secondly, the pressure of having to make forecasts for many parts of the Northern Hemisphere forced attention to interactions between weather and circulation on at least a hemispheric, if not global, scale—interactions that lie at the heart of short-period climate fluctuations. Thirdly, a large number of climatological studies were undertaken under the sponsorship of the military services, studies which formed the base for extended forecasts. All the above considerations led to the idea that it would be desirable to have an extensive project to complete a long series of hemispheric analyzed maps at sea level. While hemispheric maps at sea level were constructed routinely in connection with the M.I.T. Extended Forecast Project, no such charts were readily available for earlier years, other than some incomplete analyses done in Germany and perhaps elsewhere. For the most part, only sectional charts based on fragmentary data were to be had. It was realized that a more complete file of reliable maps would provide a basis for many studies germane to the war effort, including use as analogues.

This idea soon had a great deal of support by administrators in the Weather Bureau, Navy, and Air Force, as well as the support of many in the academic community and scientific branches of the Weather Bureau and the military. It may come as a surprise to many meteorologists today to learn that the total hemispheric concept for these maps was at first not fully endorsed. For example, the Navy was primarily interested in the North Atlantic sector, not in completing the vast work for continental

areas, for at that time, much of the war activity was over the oceans. On the other hand, the Air Force was in favor of completing the maps for the European theatre of war, and was not particularly interested in ocean meteorology. Some of us, particularly Harry Wexler, Rossby, Willett and I argued strenously for entire hemispheric coverage, not only because of further probable extension of the war, but especially because of the required consideration of interconnections between circulations over remote areas. Finally it was agreed that the complete Northern Hemisphere would be analyzed for a ten-year period ending around 1939—the time when observations began to deteriorate and be unavailable because of the European war.

Six top-notch analysts around the country were asked to start the project at New York University, including Jorgen Holmboe, Gardner Emmons, Jim Andrews, Harry Hawkins. Harry Wexler and I were the overseers of the project, and frequently went to N.Y.U. to advise and check up on the analytical work and the progress toward our goal. At first an experiment was conducted to see if analyses done by different meteorologists were in fair agreement with each other as to fronts, isobaric configurations, and the like. After a week's trial, it was decided that while the charts were not alike in detail, they all caught the essential features; therefore, the project could proceed. Furthermore, an extensive plan had to be worked out whereby certain teams dug up the hundreds of thousands of reports from various countries, from ships, from airplane flights, and other sensing platforms. A team of map plotters then transferred the observations to weather charts. Next, the plotted charts were turned over to analysts to put in fronts, isobars, precipitation areas, air mass designations, etc. This procedure also carried with it the detection of erroneous reports and their elimination. The charts were then given to the drafting unit for preparation for final printing, binding and collating. It became almost a factory procedure, and soon the scientific community was to see the first volumes of "Historical Weather Maps." As soon as produced, these volumes became helpful for the war effort—in studies of climatology, in the selection of analogues, and for testing proposed forecasting concepts. The Chief of the Weather Bureau, Dr. Reichelderfer, an authority on map analysis, not only took an active administrative role, but even personally inspected many maps to see "if they were analyzed right." Later on during the War, it was decided to expand this project by completing 40 years of past charts, back to 1899. The work was farmed out to C.I.T, where speed-up factory methods were introduced. Dr. Krick was especially interested in the final products, since they would be useful in employing his analogue methods.

Aside from having technical supervision of some of the work of the historical map project, I devoted much of my energy during the War to making extended forecasts for the military services, lecturing to Air Force cadets, Navy officers, and civilians at various university training centers, and carrying on special research projects bearing on the war effort. Among these were forecasts for the North African Invasion, where predictions of sea state off Africa were vital. These forecasts were made using the wave and swell techniques developed by Sverdrup and Munk, which depended on estimates of the wind systems over much of the North Atlantic for periods several days in advance. Other predictions for many wartime events were made, including favorable periods for the transfer of disabled vessels to other ports for repair. Research involved preparing new climatological charts of upper air winds, estimates of the likely course of incendiary balloons from Japan, favorable and unfavorable conditions for the possible invasion of Japan, and certain aspects of the meteorology for bombing raids. My duties frequently brought me to the Pentagon. On one occasion, I participated in a Top Secret series of

a month's meetings with Soviet scientists to share information regarding forecasting methods. Our meetings were held in a most secure office of the Pentagon, where the windows were blacked out so that no one might spy on us. Of course, there were key Air Force Officers and translators. As a civilian, I was told by the commanding officer that "if any of the discussions leaked out, Namias was as good as dead!" Needless to say, I kept my mouth shut.

Around 1943, I felt that our extended forecast methods should be detailed in a monograph. Thus I wrote "Extended Forecasting by Mean Circulation Methods"—a monograph promptly stamped CONFIDENTIAL. A few years later, this monograph was brought up to date and printed for general distribution after declassification.

The monograph summarized much of the newer methods developed since the work at M.I.T. and also gave some of the historical scientific background of the extended forecast problem. The monograph was to become well-known in the United States and in Europe just after the war, in part because the Europeans had been deprived of American scientific literature and descriptions of U. S. methodology for several years.

Also, toward the end of the war period, my colleague Phil Clapp and I completed a paper detailing our studies of the motion and development of long waves in the westerlies. It was in this period that I wrote up some results showing that very strong anticyclones affecting North America were at times the result of the superimposition of a high cold substratosphere on a cake of abnormally cold lower tropospheric air that arrived from Siberia. Decades later, this peculiar trajectory of Arctic air was dubbed "The Siberian Express."

Phil Clapp and I also published a fairly comprehensive paper explaining a kinematic method we had developed for use in five-day forecasting. This was objective in nature and helped make five-day mean circulation forecasts easier to prepare. In computing tendencies for mean maps, one of our bright young colleagues, Jim Walsh, had discovered the nature of the Markoff process, which characterizes variations in the pressure and temperature fields aloft and at the surface, a property that was to become more fully documented and discussed by scientists in later years.

Expanding Horizons—the World Domain

Just after the war, American meteorology entered a still more active era. Young men fresh from military training and service were looking for jobs in civilian life, and frequently took advantage of the G.I. Bill of Rights to complete university work for their Ph.D. degrees. Many had learned of our work in extended forecasting and came seeking positions at the Weather Bureau. We were in the enviable position of being able to select the cream of the crop and, indeed, were to take on those who had a genuine interest in both operational and research work. Many of these new recruits to the Extended Forecast Section would later be selected for new key positions in rapidly expanding Weather Bureau activities—in satellite meteorology, numerical forecasting, the IGY, and in development of new forecasting techniques in general. Some left for high positions in the Air Force. Years later, Dr. George Cressman, who succeeded Bob White as Chief of the Weather Bureau, said that the Extended Forecast Section was an ideal training ground for meteorologists entering the important new fields of meteorology in the Bureau.

During the late 1940s, some of the most exciting meteorological work was being done at the University of Chicago, where Rossby and Horace Byers had established a new department; and also at the Institute of Advanced Study in Princeton, where a

most important project in numerical forecasting with the use of high-speed computers was initiated. Some pioneers in this effort were Jule Charney, Phil Thompson, Paul Queney, and the great mathematician, Johnny von Neumann—along with others, some of whom are shown in an accompanying photo. Although I frequently visited Princeton, my role was chiefly that of a kibitzer to see if the computer-generated forecasts resembled the real atmosphere. I was well-equipped to do this, since the first model developed there, principally by Charney, was the barotropic model, the essence of which I had been employing with primitive methods for several years following Rossby's classical 1939 work.

Princeton became a center for visiting scientists, and a number of experts were called upon for advice during short or long stays. Among these were Johnny Freeman, Hans Panofsky, Ragnar Fjørftoft, Arnt Eliassen, Joe Smagorinsky, and Norm Phillips. Much credit must be given to the group of programmers who worked with the best computer at the time, the Eniac, particularly Herman Goldstein. The sponsorship of the three groups, Navy, Air Force, and Weather Bureau, and particularly the Office of Naval Research, was crucial. Key administrators in this endeavor were Harry Wexler, Reichelderfer, General Yates, and the Navy Monitor of the project, Dan Rex. In passing, it is interesting to note that at the first meeting to discuss the new Princeton endeavor, to which about 35 of the nation's top-notch meteorologists were invited to give advice, no one suggested as a starting point, the barotropic model! This in spite of the fact that many of those present had worked in this general domain.

Although I took great interest in the Princeton work and attempted to apply the first simple one-dimensional models to extended forecast practice, my main efforts were in trying to explain the causes and maintenance of the atmospheric centers of action and the associated jet streams, which had by then become popular, not only in the scientific community, but even with the general public. As a government spokesman, it frequently was my lot to interact with the media by giving interviews describing the abnormal weather regimes and their relationship to long wave trains and the jet stream. It has always been my philosophy that public information of this sort is very helpful to science, although one who holds this view is occasionally subject to criticism from some of his contemporaries.

Among a large number of papers I wrote at this time, perhaps the most prominent were the work with Phil Clapp on the confluence theory of the jet stream, and an article on the Index Cycle.

In 1949, Rossby, who had a year or so earlier returned to Sweden to found a new International Institute of Meteorology, invited me to Stockholm. Reichelderfer agreed, and my family and I were soon excitedly preparing our trip—the first outside the continental United States. After a ten-day voyage on the S.S. Gripsholm, a Swedish ship, we disembarked in Sweden to be greeted by Rossby and meteorologists from foreign countries who were working at the new Institute. Friendships were easily made and it was not hard to get into harness in the splendid research atmosphere. Rossby insisted that I take part in the daily map discussions (he didn't have to insist), and I soon was his principal salesman for barotropic reasoning and forecasting concepts. On more than one occasion, the experience gained with barotropic concepts paid off at these map discussions. For example, forecasts for a long Easter celebration were requested from the Swedish Meteorological Service by the media. The Director of the Service recommended that they contact "the American expert" in extended forecasting who was visiting in Stockholm. Fortunately, the synoptic situation prior to the holidays

was a clear cut type in which strong cyclonic activity had developed over the North Atlantic, and the indicated vorticity flux from the developing trough strongly suggested that a quasi-permanent ridge of high pressure would soon dominate northern Europe. This forecast was given to the press and shortly verified by a fine extended weather spell. This one prediction seemed to "sell" barotropic concepts to all the forecasters and perhaps the administrators of the Swedish Service. Their best-known forecaster entreated Rossby to permit him to spend an extended period of study in Washington.

I had believed that barotropic concepts would be well-adapted to the European area because the climatological situation there is characterized by a relatively small meridional temperature gradient, and thus the upper air patterns would be responses to the influence of strong baroclinic systems over the Atlantic to the west. This simple idea was indeed verified numerically later on in Sweden and elsewhere when the first computer–generated barotropic forecasts were produced.

While at Stockholm, I presented a series of well–attended lectures on extended forecasting concepts and practices. Some of these lectures were repeated at the University of Uppsala where Bergeron was located, and also at Oslo and Bergen where I spent a couple of weeks. At that time, my hosts were most cordial and eager to hear about the new ideas developed in America—ideas from which they had been cut off. From contacts in Sweden and Norway, it soon was apparent that I was considered a foreign expert and VIP—a circumstance which, while pleasing to my ego, carried with it feelings of responsibility and sometimes inadequacy to live up to my billing. After all, I never had been especially proficient in dynamic meteorology, and carrying on discussions with people of the stature of Fjørtoft, Eliassen, Høiland and others was not easy. However, my hosts everywhere respected me for what I was—a good synoptic meteorologist who was fortunate enough to have been on the scene when great advances were being made—and one who had participated in some of the advances. It wasn't long before the Norwegians considered me a member of the "Norwegian School," a most gratifying honor because of my early study and admiration of their work.

My research at Stockholm continued with investigations of upper air flow patterns and their causes—always with the practical idea of forecasting in mind. It was here that I embarked on a careful study of the index cycle—the slow wintertime phenomenon when the westerlies first slowly decline and then recover in a cycle of about four to six weeks. These variations carry with them great alterations in the positions and intensities of the centers of action. Among a number of conclusions from this paper were that the average speed of the regional slow-down in the westerlies, involving great blocking activity, was about 60 degrees of longitude per week, and the inception of these blocks was frequently over Europe. A hypothesis was presented that the index cycle represented a great condenser—first storing vast masses of cold air over the Arctic, and then, as a relaxation, releasing them to temperature and low latitudes to provide the necessary heat exchange between pole and tropics.

After about seven months in Scandinavia, my family and I began an extensive trip to many European countries, but were unable to get a visa to Russia, probably because of the atmosphere of the "cold war" at that time. Visits to other countries were very rewarding as meteorological and cultural education. At each place, I gave one or two lectures which, as in Sweden and Norway, were well-attended and received. I was, in a sense, a "one-eyed man among the blind," since I had been privy to hemispheric data and many new and effective ideas found during and just after the war years. For the most part, lectures and discussions with meteorologists were in English, although my

broken German sometimes helped; often interpreters were made available. Our living style was greatly assisted by the fact that I had the equivalent rank of Colonel in the Air Force, and this together with a special passport gave me access to the military PX, which was to be found in most cities. In some of the war-devastated countries, especially Germany, these amenities were most helpful. They also made it possible for me to share food and drink with fellow meteorologists. I was well treated by the heads of the weather services and by university people, so that this was one of the most pleasurable European trips we have ever made—our color slides still resurrect nostalgic memories!

At the conclusion of the trip, we sailed from England for home on the S.S. America. As usual, I immediately introduced myself to the Captain and navigation crew, and from time to time went to the Captain's quarters to check the weather reports and gave instruction and encouragement to the officers who took weather observations and filed reports.

After arrival in Washington, I was "debriefed" by some government officials and gave a couple of seminars detailing my experiences and making evaluations of European methods. For a few weeks after my return, I dictated extensive reports of my trip to my secretary, for I had taken copious notes while in Europe. The final typed report, about three-quarters inch thick and single spaced (the "ETR," European Trip Report), was sent up to the Chief of the Bureau and other presumably interested people at the Bureau. After a dead silence of about six months, I presumed that (1) it was of no interest, (2) it was poorly written and boring, or (3) the Weather Bureau people had other more pressing things to do (they were always fighting fires—real or imagined). Then came a telephone call from someone familiar with the report, saying that he and a colleague would like to talk over the contents of the report. I was ecstatic to find an interested meteorologist, but he turned out to be with the CIA. In our Extended Forecast Section, there was, however, a good deal of interest in the report, and together with a number of enthusiasts, we revised some of our map analysis and display procedures and introduced better techniques for forecasting and research. Thus the long report was valuable after all.

In 1950, Dr. Reichelderfer, the Chief of the Bureau, asked me to make an extensive trip around the country to all the principal forecast centers, spending a week or two at each, carrying on map discussions, giving lectures, and making suggestions for forecast improvements. This trip gave me a much better idea of the work of the Bureau in "the field" and also some of the deficiencies in education and service. It was discouraging to find that few forecasters at that time were familiar with new developments or with the scientific contributions coming on line. In particular, the concepts of long waves, vorticity redistribution and related processes were hardly known and were sometimes even ridiculed as impractical tools. However, there were always some keen and up-to-date young men who were sympathetic and made my trip easier. Much to my consternation, I was known mostly for the much earlier work, "An Introduction to Air Mass and Isentropic Analysis" and hardly at all for the other research. I am sure the current scene at the Bureau is much more scientifically oriented.

Among the high points of this trip was the stay in Hawaii. We had just embarked on "The Hawaiian Project"—a research effort sponsored and funded by the Hawaiian Pinapple and Sugar Association. It was designed to develop methods for weather forecasts, particularly precipitation, for various plantations. The Director of the Institute was Dr. Auchter, a former head of research at the Department of Agriculture and

a most progressive and likeable man. Not surprisingly, it was Rossby who originally had gone to Hawaii and was instrumental in starting this project, along with some sponsored research at the University of Chicago. My family and I received red-carpet treatment by the pineapple and sugar interests, including the assignment of an airplane and a car so we could visit a number of plantations on the islands. Fresh pineapples were delivered outside our hotel door each day!

Together with the tasks at the Weather Bureau and some lectures at the University, days were full and exciting. Bob Simpson, then in charge of the Weather Bureau there, helped make our stay most pleasant and efficient. The Hawaii trip and research on their problems made me more aware of the great importance of North Pacific weather and wind systems, an increased awareness which was to influence my work on teleconnections to United States weather and climate patterns, and on air/sea interactions.

Starting with studies of index cycles, my attention became increasingly focused on long-period evolutions of atmospheric systems. It became clear that events over a month and even over several months were not random nor were they the accidental results of ensembles of variable day-to-day and week-to-week circulations. One of the most striking cases of a remarkable evolution of atmospheric events over a four or five month interval transpired in late fall of 1949 and the first quarter of 1950. During this time, great weather anomalies had dominated the coterminous United States—first with very cold air over the West and extremely warm air in the East; but in late winter, this pattern was completely reversed. In other words, winter arrived late in the East, with the dramatic reversal taking place in about a week. Analyses of monthly mean mid-tropospheric height patterns showed the slow and regular movement of a great Pacific anti-cyclone from an area east of Hawaii northwestward to the Bering Sea and then into Alaska and Northwest Canada. This peculiar motion and the teleconnections therefrom were responsible for the abnormal wind and weather patterns observed over the United States.

In view of this evolution, the long-range forecasting problem for time scales of a month to a few months might therefore be stated as an attempt to understand how and why such patterns emerged and developed. The day-to-day weather maps gave little hint of this long-term development, but rather seemed to be commanded by the mean developments. Thus my major effort from then on was to research and analyze such evolutions with the ultimate aim of obtaining forecasting clues. This particular case suggested that changing seasonal influences, involving insolation and dynamic interplay between centers of action (teleconnections), could explain and clarify some of the apparently chaotic evolution. There also appeared to be some influence of the underlying sea temperature anomalies on atmospheric developments. Perhaps this was the time when my search for air-sea interactions as causal mechanisms began. However, this coupled air-sea attack was not to become organized in my mind until the late 1950s. At any rate, I believed that an attack could be made on the problem of monthly forecasting with some hope of success, and that such an attack might also throw light on some five-day forecasting problems, as well as profit from the five-day work.

Implicit in this and other work was the fact that synoptic-scale systems often went through a cycle in about a week only to return in similar form in the following week or so—suggesting quasi-periodicity. At about this time, Dr. Irving Langmuir, the Nobel Laureate, had tried to show that his seeding of clouds in New Mexico was responsible

for establishing a weekly periodicity in many meteorological elements as far away as the Ohio Valley. These claims encouraged me to carry out further studies of natural periodicity, leading to two papers on this work. Langmuir became greatly interested and invited me to spend a few days with him at the G.E. Knoll's Laboratories near Schenectady. Although he worked hard to convince me that the periodicity found over the Ohio Valley was due to seeding in New Mexico, I was able to demonstrate in this and in other cases, that periodicity could be explained in terms of the evolution of the general circulation on the appropriate time scales. While Dr. Langmuir was a most capable scientist who made major contributions to meteorology, including the statistics of periodicity, I have always felt that he was not well-equipped to draw conclusions in this particular branch of meteorology. His stature is not diminished by this one misjudgment.

After completing a monograph on 30-day forecasting experiments written in the relative peace of Woods Hole, I embarked on the routine preparation of 30-day forecasts with the consent of the Weather Bureau Administration. Although there was some hostility in some quarters of the Bureau, the verification of five years of forecasts convinced many that the predictions were worthwhile—particularly if cautionary notes were appended. Although these notes did not say that use of the material might be dangerous to one's health, the caveats were almost legally worded. Reception by the general public and by industry was favorable. The scientific community showed a great deal of interest, although in many cases, a healthy scepticism. Many of the meteorologists in our Extended Forecast Division became interested participants in both the research and the forecasting. The operational 30-day forecasts provided a good image of the Bureau as a progressive public service organization. As a byproduct, it projected me into the TV limelight as one able to explain abnormal weather regimes in terms understandable to the layman. Thank God for the "Jet Stream" and all the possibly related phenomena!

Another of my interests during this period was in hurricanes—particularly families of these storms which seemed to be closely associated with the abnormal forms of the general circulation. This was the time (the 1950s) when the eastern seaboard as far north as New England was vulnerable. After a presentation at a symposium in New York dealing with hurricane vulnerability, an unexpected bonus occurred when two reporters—one from the New York Times and another from the Herald Tribune—heard my talk and gave it front page coverage the next day. The reason was that I had pointed out that large-scale wind patterns involving the Bermuda High had developed over the past few years so as to favor northward movement of hurricanes along the coast, rather than eastward recurvature, thereby making the entire eastern seaboard vulnerable. Some members of Congress saw these articles, and this had something to do with the establishment of the proposed National Hurricane Research Center in Florida. Fortunately, or unfortunately, this particular hurricane pattern was to hold for a few years. During this period, my colleagues and I developed a system for estimating areas of hurricane vulnerability along the Gulf and East Coasts for the coming month. The Chief of the Bureau gave me permission to issue a few cautious statements, and the first of these was issued on the first of June in 1957. It indicated that in general, the entire Atlantic seaboard would be free of the threat of hurricanes for the month, as would the Florida peninsula. However, the mid-portion of the Gulf coast might be vulnerable to one or two hurricanes, although it was not possible to say when or exactly where they would strike. The experimental nature of the forecast was stressed.

In the middle of the month, I had to make a business trip to the National Hurricane Center in Miami. Shortly after I arrived, a tropical storm developed into a hurricane in the Gulf after appearing in the Caribbean, and as luck would have it, made landfall in Louisiana. This storm produced great damage and some loss of life. The meteorologists at the Hurricane Center were amazed that an indication of this storm track was announced around the first of the month, and frankly, so was I. When I returned to Washington, I was somewhat euphoric that the system had performed so well, although I was sorry to see the devastating effects of the hurricane. The morning I arrived in Washington, the Chief of the Bureau asked to see me as soon as possible. In my euphoria, I thought he was going to congratulate me on a job well-done, but that was not to be. Rather, he told me that the forecasts of hurricane possibilities so far in advance would have to stop, because two southern senators (both on the Weather Bureau's appropriations committee) were pressuring him to cease issuing these predictions. Their constituents felt that any mention of hurricanes would affect the tourist trade. Being a good soldier, I acquiesced to the Chief's wishes (what else could I do?) and that was the end of the hurricane vulnerability experiment. Twenty-five years later, some bright young meteorologists are pursuing the problem. If they succeed technically, I hope they will not find insurmountable obstacles to disseminating their statements.

During the 1950s, the numerical forecasting work at the Institute of Advanced Study was in full swing, and I was fortunate to have been able to see firsthand some of the remarkable progress being made. From time to time, I traveled to Princeton with a few others; among them, Harry Wexler. It was felt that my presence would insure that realistic solutions were being ground out by the computer, and that from time to time, I might be able to contribute an idea or two. Early on, it became clear that since Jule Charney had appeared in Princeton, the project was headed in the right direction and practically important results might take place within a few years. Like everyone else, I sat in awe of the great Johnny von Neumann, and did my best to follow his rapid-fire delivery of ideas and top-of-the-head calculations. Johnny occasionally would ask me pertinent questions about the behavior of the real atmosphere, a circumstance which really bouyed me up. As far as I know, my presence, largely as a kibitzer, may have helped. At Princeton, I was able to meet and talk with the great Norwegian meteorologists, as well as with many other famous dynamicists. The Princeton group was a wonderfully organized team—a pleasure to see in action.

In 1955, I was to embark on yet another important era in my career. My earlier research in extending the time range of forecasts and in problems of the general circulation won me the highest award of the American Meteorological Society. This was presented at the Annual AMS Meeting in New York. After the meeting, I returned to Washington to announce the news to my wife, who told me that I had received another award in a letter from Princeton University—the Rockefeller Public Service Award, given to only ten civil service workers of the entire two or three million. Only two were scientists, so naturally I was elated. This award made it possible for me to spend a year at a place of my choice. Not surprisingly, I chose Stockholm, where Rossby had fully established an international center for meteorological research, which attracted many distinguished world scholars.

Thus my family and I, including my ten-year-old daughter, Judy, embarked for Sweden, taking a Swedish ship, and soon were in the heart of Stockholm again. As a side note, a few bureaucrats in the Weather Bureau attempted to stop this trip because

they said my presence would be needed for the soon to be established climate research group being formed under the aegis of von Neumann at Princeton. The Rockefeller Award Committee and Princeton University, administrators of the awards, paid no heed to the desires of the Weather Bureau, nor did I. I felt that my contribution to the Princeton work would be enhanced by spending a year in Europe.

At Stockholm, I completed a number of papers, some begun at the Weather Bureau in the preceding year or two. I became familiar with more of the aspects of numerical forecasting with the barotropic model. This new work gave me hope that ultimately, a first approximation to 30-day and longer forecasts might come from a numerical-dynamic model. It has now been thirty years before full-fledged attempts are being made, but I still feel that the new cadre of capable dynamicists are on the right track. However, I still cling to my conviction that in order to succeed, this effort must embrace empiricists as well as dynamicists, and of course, state-of-the-art computers.

Among the papers written at Stockholm were studies on drought, connections between warm summers sandwiched in between cold winters over Scandinavia, and a short note to the effect that barotropic models could capture mainly large-scale waves, not the smaller synoptic systems. In the drought paper, I showed that the surface moisture in the soil of the Great Plains of the United States played an important role in drought by varying the heat input to the overlying atmosphere. That is, heat could be used for sensible heating of the soil or for evaporating the soil moisture, and this modulation could have important influences for producing long-period lags in the general circulation. This paper also stressed that the drought-producing upper-level high pressure cell over the Great Plains is dependent upon similar anomalous cells over both the North Pacific and North Atlantic, operating through teleconnections. Once this triple cell pattern was established, soil moisture deficits could feed back to help maintain the continental high cell. At Stockholm I also became interested in the general circulation patterns of the Arctic, for this area was now reasonably well-covered by upper air soundings. Thus, two studies were prepared and published, and one was presented at an Arctic symposium held by Sverdrup at the Norwegian Arctic Institute in Oslo.

After leaving Stockholm in the early summer of 1956, our family traveled extensively in Europe, where I spent a week or two at the main meteorological centers, discussing topics of mutual interest with foreign colleagues, observing forecasting practices and giving lectures on general circulation and on long-range forecasting. The contacts made good friends who have been most helpful and friendly through the years. These travels also projected me in the role of diplomat without portfolio, helping to foster international collaboration. This experience was to be most helpful later on in dealing on the international level, and at international scientific meetings.

The summers of the 1950s were spent at Woods Hole, where I was able to get away from the telephone and administrative duties sufficiently so as to write up research results of work usually done in Washington. This material provided substance for a lecture or two at the Oceanographic Institution. The research was eclectic in character, involving the upper air general circulation and its weather concomitants and lag effects.

While at Woods Hole in 1957, word of Rossby's untimely death arrived. This came as a great shock to us there and around the world. It was a very depressing period, for we felt the loss of an irreplaceable leader, a great humanitarian, and friend. I was not overly surprised by Rossby's demise, because in my work and social contact with him in Stockholm in 1956, it was clear that he was not in the best of health. Looking back

on that period, I now see some of his remarks to me in a foreboding light, whereas at the time, I attributed them to Rossby's occasional pessimism. I feel most fortunate to have spent many periods of my life with him.

Air-Sea-Continent Interactions as a Unifying Focus

It was not until 1958 that my research took another major turn—this time into the realm of air-sea interaction on time scales of the order of a month to seasons. The stimulation for this was the now-famous Rancho Santa Fe Calcofi Conference, held by Scripps Institution of Oceanography, at the idyllic Rancho Santa Fe Inn, not far from La Jolla. The organizers were John Isaacs of Scripps and Oscar Sette of the National Marine Fisheries at Monterey. Other activists were Roger Revelle, Walter Munk, Warren Wooster and a host of biologists, chemists, and geologists from Scripps. In addition, some outsiders were invited as experts, among them, Jule Charney, Henry Stommel and me. It turned out that, as the first speaker, I addressed the problem of the meteorology of the anomalous period that had sparked the conference, one in which remarkable oceanic warming had occurred over the eastern Pacific. Southern fish were being caught in northern waters, El Niño was in progress, unusual typhoons were observed, and, in general, both atmosphere and ocean were far from normal. This abnormality also included the marine biota, the California current and some marine chemical properties. Armed with a large file of charts gathered in Washington, I proceeded to describe the weather and wind patterns of the period, then spent the next few days listening intently to scientists from other disciplines talk about impacts of the abnormal ocean and atmosphere. The inter-associations quickly became clear, and it struck me that some of the secrets of long-range weather forecasting might lie in the coupled air-sea system. It was especially noteworthy that the mismatch of time scales in the two media, air and sea, could account for the frequently observed long-term memory required for long-range problems. Of course, this central idea had been proposed in earlier decades, particularly by Helland-Hansen and Nansen and by others, but their work was inconclusive. They were greatly hampered by lack of data and high-speed computing devices, and especially by lack of a coherent theory of the general circulation. I felt that science had now advanced so that a fresh start was in order.

Immediately upon my return to Washington, I began work on the large-scale air-sea interaction problem, and have never since ceased this effort. Although much more complex than I originally thought, the subject has provided an avenue of approach to short-period climatic fluctuations that shows no sign of diminishing. In fact, the present extensive efforts to study El Niño and the Southern Oscillation (ENSO) have catapulted the air-sea problem to the forefront of meteorology and oceanography. Perhaps my 1959 AGU paper and subsequent papers on atmosphere and ocean interactions over the North Pacific helped spark the renewed surge of interest. At any rate, the new ideas obtained by attacking these problems have kept me going for the last few decades, and have afforded the opportunity to participate in dozens of national and international conferences.

In 1961, I was invited to spend the summer as a visiting professor at the University of Mexico at Mexico City. This was a U.S.-Mexico collaborative arrangement under the Smith-Munn Act. My hosts were the Geophysics Department at the University, which was headed by Julian Adem, a former colleague at the University of Stockholm in 1955. My family and I were excited about this new venture, and soon we were in

the hectic Mexico City area. At the University, I gave a series of lectures to staff and students, and also discussed scientific topics with a number of staff researchers. I made the mistake of giving two-hour, back-to-back lectures on many days, and I soon became aware of the rarified air (7000 feet elevation) and the toll on my stamina. Nevertheless, my enthusiasm enabled me to carry on.

Edith, Judy and I took this opportunity to see other parts of Mexico, taking trips to Oaxaca, the Necaxa watershed, the east coast, and other spots including a couple of weekends in Acapulco. Our hosts were most obliging, providing a car and chauffeur whenever necessary. It was VIP treatment! Julian Adem took a lively interest in long-range forecasting and was developing a thermodynamic model for practical use. Later on, I invited him to spend a year with us at the Extended Forecast Division in Washington. He stayed for several years, developing his model, which, while controversial as to its performance, has shown considerable promise.

In the winter of 1962, I was asked by Weather Bureau Chief Reichelderfer to spend about six weeks at the newly established Antarctic Analysis Center in Melbourne, Australia. This was an international center set up after the IGY program that took place in 1957-58, its purpose being to prepare Southern Hemisphere weather map analyses to assist in interpreting events in that hemisphere. The Center was headed by Mr. Philpot of Australia, and among the participants was Tom Gray, a former employee of the Extended Forecast Section who had recently spent a couple of years in the Antarctic. I was prepared to face a difficult task, for the observational network was relatively poor, especially compared to that of the Northern Hemisphere, and research in that area was rather sketchy.

Satellite cloud observations were not yet a reality, so the analysis had to be constructed from sparse surface reports and some vague notions of how maps should look in that hemisphere. My own knowledge and experience in the Southern Hemisphere was minimal, and to be honest, it took me some time to orient myself and get used to the opposite rotation of the winds around cyclones and anticyclones there. After some trial and error, this was reasonably mastered, but I soon became frustrated with the almost complete lack of data in the moat separating Australia and New Zealand from Antarctica. It was at this time that I fully realized the foolishness of the oft-repeated statement in America that "the secrets of long-range forecasting are locked in the Antarctic." If this was indeed true, I felt that someone provided a cruel hoax by throwing away the key.

In spite of this frustration, it was an interesting assignment in many ways. It enabled me to make many new friends with the people "down under," including the excellent group of scientists at Aspendale, at that time under the fine leadership of Bill Priestley. I also visited New Zealand, where I renewed my friendship with the Meteorological Office Director, Richie Simmers, who was a colleague of mine when he was a Rhodes Scholar at M.I.T. I gave lectures in both Melbourne and Wellington. In Melbourne, I showed some data indicating possible connections between the circulations of Northern and Southern Hemispheres, and suggested that the ongoing drought of that period in southern Australia might be partly a reflection of some anomalies over Asia and Indonesia. I did not follow up on this study.

I took advantage of the wonderful beaches in Australia, and was also privileged to visit the Snowly Mountains and many of the man-made lakes. Similarly, in New Zealand, Richie Simmers arranged for me to see many of the sights, including Rotorua, the village of the Maoris.

On my return, I had to fly back in a propellor plane because New Zealand did not yet have airport facilities to handle the new jet aircraft. I checked the weather maps just before the flight to Fiji, and saw that a major typhoon was in our flight path. When I pointed this out to the Captain of the plane, he politely thanked me—and then proceeded on the original flight plan, which put us right into the heart of the typhoon. This was during the dinner serving—quite an experience! After this, a few days of "R and R" in Hawaii was most welcome.

Although the early 1960s held much activity in the form of research, the period was one of considerable stress and physical problems. My best friend and brother-in-law, Harry Wexler, passed away in 1962 from severe heart problems. As head of research at the Weather Bureau, he was technically my boss. In reality, he and I shared many hours discussing our research, and he was always most helpful. His own efforts were expended over a wide range of activities, including the IGY, the World Weather Watch Program of the WMO, the first satellite programs, etc. In retrospect, it seems incomprehensible that one person could successfully carry out such a program of domestic and international activities. When he died, it required at least three key scientists to fill his shoes.

In the following year, 1963, after a hectic period of international participation, domestic lectures, research, and many TV and media interviews, I, too, had a coronary heart attack, which took me out of circulation for a few months and slowed me down appreciably thereafter. However, as with other bouts of illness, I managed to work on some unfinished papers at home during my convalescence. These papers dealt with interconnections of circulation between northern and southern hemispheres, wherein strong anomalous flow patterns of the Northern Hemisphere seemed to be associated with and perhaps cause displacements of the zonal wind belts of the Southern Hemisphere through modulation of the tropical Hadley cell's position and intensity. At this time, the ENSO problem had not yet emerged as it has in the past decade. Undoubtedly, ENSO was also associated with the events I discussed in these papers.

It was also in the early 1960s that I became more and more convinced that economically valuable seasonal forecasts could be made. After a trial period of five years and some encouraging verification, I published an article detailing the methods and giving results. There were, of course, objections to the possible release of such predictions, and for many years, they remained on the shelf as fascinating experiments. Finally in 1968, the Weather Bureau decided to release them publicly. The reception, particularly in industry, has been encouraging, even though skills are low. It has always been my feeling that even small or modest skills in the right hands can be useful. After I left Washington, my successor, Donald Gilman, developed a system to express these and monthly forecasts in terms of probabilities. While this procedure has much to recommend it, I have felt that since the probabilities are based in large part on the past years of track record, the usefullness is restricted because individual situations vary considerably in strength of premonitory signals. Hence, some big economically important events may be missed by diluting the output with vague probabilities. Of course, this is a controversial stand.

As usual, most of my research during this period dealt with air-sea-land interaction problems and several papers on this subject were published. One of the most interesting to me was a careful study of multi-year recurrence of intense blocking over Scandinavia, which led to drought over much of the area, including the normally wet Norwegian coast.

After a visit to Woods Hole in the summer of 1964, I was involved in a bad automobile accident on Route 1, south of Boston. Ironically, I was not the driver, never having driven a car, but I was in the "suicide seat." The terrific impact led to a broken femur and hip, which called for an operation complete with prosthesis. Fortunately, I had a surgeon who did an excellent job, especially for that period and in a rather small hospital. The lengthy period of traction and convalescence enabled me to catch up on my literature. The moral support of my colleagues in the Weather Bureau and elsewhere was most heartening.

After returning to work in the fall of 1964, I began to restrict activities, to delegate more authority to capable staff members, and to spend more time on research on short-period climate fluctuations. The new Chief of the Weather Bureau, Bob White, who was always keenly interested in this subject, was most helpful. Meanwhile, we in the Extended Forecast Division had built up an excellent group of practitioners and researchers—many of whom are identified in an accompanying photograph.

Two years before going to Scripps, I was invited to New York University to work on the problem of the Northeast drought that had plagued New England and the middle Atlantic states for several years in the early and mid-sixties. Jerry Spar had secured funds from New York State to carry on this work, so in September of 1965, I went to NYU and with some assistants, began exploring the great drought, utilizing some ideas gleaned from my other studies of drought over the Great Plains. It soon became clear that drought in this area was different from droughts of the Plains. Rather than being associated with an upper level high pressure cell, the Northeast drought was associated with prevailing northwesterly winds aloft, with a trough not far off the Atlantic seaboard. The net result was frequent subsidence and the accompanying dryness, as storms developed and moved off rather than along the coast. But what factors produced this abnormal wind pattern during the spring and summer periods when the multi-year drought occurred? Here I again resorted to an explanation suggested by sea surface temperature anomalies. The offshore waters were quite cold and the gradient between these and the Gulf Stream was strong. I perceived that this oceanic baroclinicity could from time to time spread into the overlying atmosphere and thereby encourage cyclone movement and development, with attendant rainfall off the coast. These synoptic systems would in turn help generate the Northwesterlies behind the storms, strengthen the SST gradient to the east and thus feed back to perpetuate the drought. Numerous factors involving wind, humidity and associated heat fluxes seemed to confirm this theory.

However, the funds for the drought project were to dry up (no pun intended) in late 1966 because the drought had ended. This circumstance seems to be frequent with drought research, because by the time the authorities become concerned and begin funding research, the drought is apt to end. A similar demise occurred after the Dust Bowl drought and also after the great Southwest drought of 1952-54. As a further ironical note, the first major rains to break the drought started as a big thunderstorm on the very day, September 1, that I arrived in New York!

As an aside to the 1952-54 Southwest drought, it so happened that I was invited to partake in a drought conference held by the Eisenhower administration in the cabinet room of the White House for an entire afternoon in 1954. Cabinet officers and many other high officials were there (including governors of the five most affected states) to hear some of us experts describe and ponder the drought in progress. At the mid-afternoon coffee break, a man who had been sitting across the table from me came

over and spoke about many things I had mentioned in my talk. He sympathized with me in stressing the great complexity of the drought problem and suggested that what was needed was another "Manhattan-type project." Not realizing with whom I was speaking, I agreed. On return to my seat, a colleague informed me that I had been talking to Sherman Adams! Naivité of this sort has plagued me all my career—the inability to take advantage of good funding sources!

Getting My Second Wind at Scripps—Retirement from NOAA

During the mid-1960s, my work drew the attention of many diverse groups of scientists, and there were many opportunities to lecture on air-sea-land interaction problems. Among the most interested group was the Scripps Institution of Oceanography, where I had earlier been stimulated at the Rancho Santa Fe Conferences. In particular, Professor John Isaacs was active in these problems, and he invited me to La Jolla on several occasions to speak and participate in symposia. He suggested that I spend some time at Scripps, but it was not until 1968 that I felt that I could do this in good conscience. For one thing, I felt indebted to the Weather Bureau, and then it was not easy to pull up stakes from the rapidly growing culture of Washington, where my wife was achieving a good reputation in the art world. Thus, I decided to try La Jolla for a six-month period. It turned out that the six months, beginning in January, had amazingly good weather—even for La Jolla—with only two rainstorms, both in the night hours—otherwise, plenty of sunshine. To help sweeten things, John Isaacs succeeded in getting a foundation to provide a nicely furnished house with garden and gardener near the ocean and close to Scripps.

The many contacts I made at Scripps were among the most stimulating I have ever had. I learned about many oceanic, biological and geological topics, of which I had been ignorant. My new friends were most receptive to fresh ideas about low frequency phenomena in the upper ocean and lower atmosphere. Consequently, I returned each of the succeeding three years for six months at a time before deciding to retire from NOAA and live in La Jolla, a decision I have never regretted. It was made at the right time, because budgetary problems in the Government were getting worse, my superior seemed to have little interest in our extended forecast problems, and many of our best scientists were being offered and were accepting higher paying jobs in other branches of meteorology. One of my principal collaborators in research, Phil Clapp, was retiring, also pointing up my decisive action.

The transition from government work to academia at Scripps was easy. My experience in the rough and tumble with bureaucracy stood me in good stead at Scripps, even though I had few administrative duties. Professor Issacs provided a small staff and saw to it that most matters like contracts, budget, and the like were handled efficiently with a minimum amount of drainage of my time. One of the most helpful in handling the details of administrative affairs was Dick Schwartzlose—a man of low profile who could manage many necessary affairs pertinent to the scientific work and still make friends in the process. My work on air-sea interaction seemed to provide further incentive to Isaacs and the Marine Life Group at Scripps, and with other factors, this enthusiasm helped secure funds for further studies. The principal funding agency was the Office of Naval Research—an organization for which I have always had the highest regard since the days when they were the principal sponsors of the Numerical Forecasting Project at Princeton. From time to time, some of us would confer with officials of this group, both in Washington and La Jolla; the relationship was cordial

and efficient. Feenan Jennings was the main administrator. Feenan was a man of uncommon ability to strip away nonessentials and get down to substantive material. Later on, he went to the NSF and performed similar functions.

Early on at Scripps, my scientific work was devoted largely to exploring in greater detail the coupling between the upper layer of the sea and the atmosphere. My earlier work had often stressed the synergynistic processes wherein ocean and atmosphere collaborated to maintain anomalies in both sea surface temperature and atmospheric flow patterns for periods of months and seasons, but it was clear that there must also be circumstances when the feedbacks were negative in character. These processes would thus at times operate to reverse anomalies or to force both media back to normal. It was not until my early years at Scripps that one of the main situations of this kind became evident—the degree of development of the sub-polar cyclones (the Aleutian and Icelandic Cyclones) in the transition period between summer and fall. From a number of carefully analyzed case studies, it became evident that if abnormally warm water was generated at high latitudes during summer, the Aleutian Low in the subsequent fall would be intense, while cold water in summer led to abnormally high pressure in fall. This concept was anticipated from physical considerations which involved diabatic heating of Arctic air masses over warm water south of the Aleutians, but stable vertical stratification with cold sea-surface temperatures leading to anticyclogenisis in fall. It was thrilling to see some validation of this concept particularly in the Pacific and to some extent, in the Atlantic, south of Iceland. A few years later, Professor Russ Davis of Scripps, who had previously discounted influences of the ocean on the atmosphere, statistically analyzed my results with sophisticated techniques and confirmed the results. While I have always been less than impressed with purely statistical results, particularly those lacking physical concepts, I found it especially encouraging that a man of Davis' stature could find confirmation of my empirical results. Unfortunately, this report of his appeared in the literature well after his first negative findings and is much less well-known.

In the years following my arrival at Scripps, many air-sea problems attracted my interest. Most of these were concerned with macroscale phenomena involving the atmosphere, surface layers of the sea, the character of the land surfaces (whether dry or moist), and the extent of the snow cover. In much of this work, I had the assistance of Bob Born, who was not only a top-notch programmer, but one who could collaborate on new ideas and help extend them. He coauthored a number of papers published in the 1960s and did much of the work involving the preparation of atlases of sea-surface temperatures, sea-level pressure, 700 mb heights, and hundreds of teleconnection charts. All these atlases have been used in research around the world and appear to have ushered in a whole new attack employing teleconnections for both operational forecasting and in research. Of course, this possible outcome was the primary reason the atlases were prepared in the first place.

Other research dealt with the time and space scales of meteorological and oceanographic phenomena and the interrelationships between them. This work finally became objective so that one could specify with considerable skill the sea-surface temperature field from atmospheric variables (particularly the 700 mb flow field), or conversely, obtain the flow field from the contemporary ocean temperature field. This work utilized monthly means over the North Pacific for about 30 years of data and techniques of stepwise multiple regression. This procedure had been exploited by Bill Klein, formerly of the Extended Forecast Division of the Weather Bureau, to derive temperature

anomalies over the United States from contemporary upper-level height patterns. The work we did involved a tremendous amount of data processing and computation, but Bob Born was up to it. Later on, a new data set of this type was worked up by an equally proficient programmer, Tony Tubbs. Even to this day, I am amazed at the tremendous amount of information at our fingertips, made possible by the high-speed computers. But first there have to be IDEAS to feed into the computers! There is entirely too much research done under the false assumption that digital manipulations alone will produce answers and forecasts. At any rate, the specification procedure has illuminated many physical phenomena and suggested new avenues of research. Practically, it underpins our current methods of long-range forecasting. Of course, one must know how either medium—atmosphere or surface layer—will behave in the period ahead. Because the time constant of the sea is at least an order of magnitude slower than that of the atmosphere, sea-surface temperature patterns are most heavily weighted. Studies have also been made of the advective component in altering SST patterns. This is now done objectively by computer, but with over-simplified methods.

Other research problems I worked on involved temporal coherence in SST, lag effects induced by advection and by the resurrection of subsurface anomalous water masses, genesis and steering of a devastating hurricane (Agnes of 1972), and monthly and seasonal studies of tropical systems. The last-named topic involved a number of studies of El Niño.

In the SST studies, it was reasoned that pools of anomalous water might be hidden at depths below the surface thermocline during the warm season, but with the onset of increased storminess in fall, might be vertically mixed, thus providing for the generation of surface anomalies unaccounted for by other factors. The data over parts of the North Pacific indeed suggested this to be the case, so that some surprising recurrence patterns of SST were found that related to wind variations.

The research on hurricane Agnes showed that it probably developed south of the Equator and was carried into the Northern Hemisphere before making landfall in eastern United States. A later study showed that there were a number of premonitory signs of the development and movement of this hurricane into an area around Florida. These signs showed up in anomalous midtropospheric wind patterns at least a few months preceding the storm—thus holding out the hope for long-range probabilistic prediction. This work strengthened my earlier-stated conviction.

The research on tropical short-period climate fluctuations centered primarily on three topics: El Niño, Sahel droughts, and big variations in precipitation over northeastern Brazil. Several papers were written about El Niño, particularly the statistical signs of its probability and the associated wind systems, both with and without lag. It was indicated that eight months of lead time, on the average, might be present. Some recent, more thorough studies give results not too far from this estimate. The association of El Niño with temperate latitude wind systems over the North Pacific, particularly the strength of the Aleutian Low, was also documented. However, it was continually stressed that this association did not necessarily mean cause and effect.

In much of this work, I had the encouragement of J. Bjerknes, the great pioneer in El Niño and Southern Oscillation studies. In fact, Bjerknes and his wife, and Edith and I became very good friends during the last decade of his life, when he was associated with our ONR-NSF–sponsored Norpax Project. He had been an idol of mine for decades since the 1930s, when I was weaned on his many scholarly papers published in Norway and elsewhere. Our closer association started with the described

Rancho Santa Fe Conference.

The studies of the Sahel Drought indicated that the seat of wind variations over that area might often be indicated by the far away wind systems over western Europe and England—a most interesting teleconnection. The research on the northeastern Brazil drought indicated that it was related to abnormally high pressure aloft off New Foundland—still another fascinating teleconnection.

NORPAX, the Climate Research Group, and a New Expert in the West

While some of my work dealt with atmospheric teleconnections during the 1970s, most of it was concerned with the interaction of the atmosphere and the ocean. After all, this problem was the central reason why I had been invited to Scripps, and indeed it became one of my consuming interests. Most of these studies involved the North Pacific and downstream effects on U. S. weather and climate patterns. Several papers were written describing mechanisms associated with the stabilization of abnormal wind patterns over periods of months, seasons and even years, often dealing with case studies and statistical inferences. Case studies were especially popular in the 1930s and 1940s, when the Norwegian School discovered many novel concepts in this manner. With the advent of numerical simulation, the number of case studies have perhaps declined in favor of machine-produced studies and statistics therefrom. However, cases involving the real atmosphere are increasingly being studied, and I predict that this type of work will accelerate with time. Nothing is so exciting or convincing as predicting real events.

One of the most striking cases of weather and climate aberrations of the 1970s occurred during the winter of 1976-77, when the far West suffered a severe drought and the eastern two-thirds of the nation was very cold with frequent snows. These abnormalities were associated with some big anomalies in upper air wind patterns and in North Pacific sea-surface temperatures. The wind patterns were composed of a strong ridge in the far West and a strong trough over the East. These patterns were remarkably stable from month to month over a six-month interval from fall to winter, so that a persistence forecast would have been quite successful. Of course, one would have had to know in advance that the period was to be so persistent. Fortunately, several premonitory signs showed up in the fall of 1976, including the forcing Pacific SST patterns, atmospheric flow patterns with strong teleconnections, an El Niño in the tropics, and some early snows, providing enhanced baroclinicity along the eastern seaboard. All these factors and the suggested enhancement by the normal general circulation led to an excellent forecast for the 1976-77 winter—a forecast which catapulted me into the national limelight. While this circumstance helped the image of Scripps as a center for climate study and prediction, and helped my popular image, it carried with it some less desirable features. Obviously, conditions of this type, involving multiple synergistic factors are not common, and therefore, the success obtained in this case is not easily duplicated. Unfortunately, the adage, "one swallow does not make a summer," did not apply to weather forecasts in the mind of the lay public.

Since 1977, many studies of this abnormal winter have been conducted. Some of these have furnished grist for numerical modelers, who found encouragement in being able to simulate some of the main features of that abnormal winter. At a large NATO-sponsored workshop held in Erice, Italy, both Joe Smagorinsky and I were invited to speak. I chose to discuss the 1977 case, and Joe asked to be placed just after me in the program. My paper dealt with the synoptics and statistics of the meteorological situation. Joe gave an excellent paper describing the results obtained

by Miyakoda of his staff at GFDL, Princeton, by employing a sophisticated model to predict the weather for the entire month of January, 1977, using the data of January 1, 1977 as initial data, and iterating. The results were remarkably good. Many of us felt that the long-awaited breakthrough in numerical long-range forecasting was at hand. I was so impressed that I gave a speech stating that I felt privileged to be present at this event, much as I has been on the occasion of the first numerical forecast made by the Eniac at the Princeton project a few decades earlier. While the Princeton group may have thought that it would be smooth sailing thereafter with other monthly forecasts, no such thing was to happen. Tests on other situations did not succeed as did the 1977 case, so computer-generated forecasts seemed to have the same fallibility as human forecasters. Nevertheless, in my opinion, this 1977 forecast was a breakthrough. Further studies suggest that one of these days (or more likely years), numerical long-range forecasting will provide a more objective physical basis for predictions. However, it is inconceivable to me that the human forecaster will ever be completely removed from the scene.

In the following winter, 1977-78, one of the main topics in the minds of westerners was whether another year of drought was in store. This became especially important because of the state of water reservoirs in California, the forest fire potential, ski resort problems, and even water for boating activities on rivers. Although I was pressured by the press and many industries for an early forecast for the winter, I held off until late November—a time which former studies had indicated was often crucial. Thus, around the first of December, after carefully studying series of maps and indications from sophisticated statistical methods, I held a press conference at Scripps.

The computations pointed to a different condition than 1977 in terms of winds and air masses affecting the West Coast, and the various indications seemed to be mutually reinforcing. All this information was pointed out to the reporters present, along with appropriate caveats. The media quickly made my report a "page one" issue, leading to headlines of "Break in the Drought" stories. It made the lead story on page 1 of the Los Angeles Times, as well as top news on several radio and television broadcasts. A long period of "sweat-out" now began, and each day's map took on special significance for me. To make events more exciting, a contemporary long-range forecaster in the Southern California area disagreed, saying that another year of drought was in store. The result is now history—heavy rains and snows dominated the far West, and once again, I became the guru of long-range forecasting. No clarification to the press of fallibility in this field seemed to carry weight. These cautionary statements were taken more seriously a few years later, when a couple of forecasts for the West did not work out. I am not too unhappy when these failures occur, because it gives the public a more accurate idea about the complexities of the long-range forecasting field and of our ignorance of the physics involved.

After I had been making these seasonal forecasts for some years at Scripps, and they had been well publicized by the media and used by a host of American industries, Dr. Nierenberg, Director of Scripps, decided that the problem of climate fluctuations should be one of the principal topics at Scripps. By this, he meant not only the short-period fluctuations, but all time and space-scale variations. Under the general heading of climate, this project would serve as an umbrella for many projects in which Scripps had internationally recognized expertise. In the mid-seventies, a general meeting of the SIO staff was held regarding this idea. Although nothing was formally done in this regard, I believe that developments have resulted in the implementation of this

concept. The CO_2 problem, deep sea cores and sea floor spreading, variations in biota, the ocean gyres as manifestations of atmospheric wind stress, and other subjects under the general climate heading, as well as the more mundane problem of climate prediction, fit under this umbrella. I bring this up to show that we in the climate group have had the whole-hearted support of the Scripps administration, without which little could have happened.

In 1978, I was selected to be a visiting scholar at the Rockefeller Study Center in Bellagio, Italy. This is considered to be one of the most idyllic centers of its kind, for it provides a wonderful place to write and complete research. It is on a penninsula jutting into Lake Como and contains about ten miles of walking trails, as well as historic buildings and part of a castle dating from 1500. My wife and I were invited to spend about six weeks along with about a dozen other visiting scholars and their wives from various countries. Thirty-two servants, including bellmen, took excellent care of us. The bellmen carried breakfast to the rooms and responded to calls, since phones were purposely omitted from guest quarters. The meals were exquisite, the service incomparable, and the entire atmosphere most heady. While there, we had the pleasure of being with top experts in medicine, law, history, literature and philosophy. Two Nobel Laureates, Peter Medawar and Arthur Kornberg, and occasional distinguished guests joined us for lunch or dinner. Included among them were the American Ambassador to Italy and young scientists from the Sloan-Kettering Cancer Center, who were holding a conference. The guests were scrambled at each dinner setting, so we were able to converse with some most interesting people. After-dinner activities were also most pleasant. During the day, we spent considerable time writing. I finished three papers, the research for which I had completed at Scripps. Edith was enlisted as the typist for these. At the conclusion of this stay, I felt especially indebted to the Rockefellers for providing such an outstanding accommodation, as well as the earlier cited year in Europe on a Rockefeller Public Serice Award.

Several requests for forecast information from high government sources had come to our Climate Research Group, and we did our best to supply predictions on the basis of data at hand and state of the art. Among these requests were estimates of the character of the forthcoming winter over the East during the oil embargo of 1974. After several cold winters, it was predicted that this winter would be mild. On this basis, authorities decided not to issue gas rationing cards. Luckily, this prediction succeeded. Other predictions involved wheat growing weather in the Ukraine, incidence of drought, and possible flood conditions in various areas. California agricultural interests, gas and electric power groups, ski resort owners and many others have always asked for and frequently received estimates. Thus in 1980, when the National Climate Program was established, Scripps was the first Experimental Climate Forecast Center to be set up.

The concept of the Experimental Forecast Center was an outgrowth of the National Climate Act, an act which was generated by activities in the 1970s and, in part, stimulated by several years of highly abnormal winters and summers. Several meteorologists testified before congressional committees relative to the desirability of passage of this act. I took an active role in stressing its importance to the nation. In 1974, before the main activity took place, I was asked by Senator Hubert Humphrey and Representative Charles Mosher to give a lecture on short-period climatic fluctuations before interested members of Congress and did so before a large audience in Washington. Senator Humphrey was especially interested, asked several relevant questions, and characteristically brought up a number of points regarding climate and its variability.

Perhaps this meeting had much to do with getting motion started on the National Climate Act a few years later. In another congressional committee meeting on the Act in San Francisco, which consisted of Senators Alan Cranston, Adlai Stevenson, Jr., and others, I made a strong plea for the Experimental Climate Forecast Center concept—something that seemed attractive to both senators. Others participated in hearings there and elsewhere, and it wasn't long before a bill was drafted and the Act passed—including the proposal for one or more experimental forecast centers. After the bill was passed, the first such center was set up at Scripps, which pleased Bill Nierenberg, as well as many of us in the Climate Research Group. I have recently stepped down as Head of the Group and the position went to Richard Somerville. In many ways I was relieved, for over the years, I had more than my share of administrative responsibilities. At the present time, other centers are being developed, but with accents on somewhat different modes of attack. This is healthy because it also provides for competition between groups in the quest for solution to one of the most complex of world problems.

On numerous occasions, we receive letters from the general public expressing bizarre ideas about what was really causing abnormal weather of a given season or even period of years. One wealthy lady inquired whether it would be wise to sell thousands of acres of soy bean-growing land that she owned in a southern state because of the imminence of an ice age! That one was not hard to answer. Another attributed warm pools of surface water in the oceans to heat rising from bottom hot spots. Still others felt that man's technological tinkering was producing strange weather and climate changes, though few letters blamed the CO_2 rise.

During the late 1970s, I continued to work on the same climate variation problems, with emphasis on interactions between the surface conditions and the overlying atmosphere, and particularly on the remote influences of these on North American climate. These problems resulted in studies of the persistence and persistent recurrence of weather patterns between adjacent seasons and between seasons of successive years. This problem has always fascinated me–for if solved, even partially, it could lead to improved predictions. My present work encourages me to believe that there is some order to these problems of persistence or non-persistence of wind and weather patterns.

My one attempt to enter the arena of the CO_2 problem involved a piece of research indicating that the combined ocean-atmosphere system operates in such a way so that the warming effects produced by increased CO_2, with maximum heating at the poles, would generate atmospheric wind systems that would cool the ocean surface waters and thus reduce the CO_2-induced effects delaying the warming. This study, empirical in nature, was based on Northern Hemispheric wind and SST patterns of the past 35 years. Naturally, such a conclusion ran into opposition by most of the scholars of the CO_2 problem. I have not returned to it.

Aside from several general articles for the entire scientific community, some written with my new colleague, Daniel Cayan, I worked on long-period coastal phenomena as manifestations of air-sea interactions, on U.S. heat waves, on effects of snow-covered areas on the atmosphere and vice-versa, and on practical methods of forecasting. This work kept me busy participating at national and international meetings and interacting with the ever-present media representatives.

Meanwhile, the El Niño problem attracted the attention of many oceanographers and meteorologists. Among these were Bjerknes and Wyrtki. I tried to point out some statistical properties of the phenomena, bearing on possible prediction. I also

used some of Wyrtki's sea level data taken off Central America to propose wind-caused effects (coming from the West) with about an eight-month time lag. While this work has since been eclipsed by other data and research, the central ideas and facts are still valid. Among these are the variations in position and strength of the Aleutian Low associated with El Niño, and the downstream responses over North America.

In all this work, I had the excellent support of a small dedicated group of assistants, including map plotters and analysts, statistical clerks, programmers and clerical workers. Among these were Dan Cayan (now making a name for himself in climatology), Charles Stidd, Madge Sullivan, Marguerette Schultz, Leslie Martinich, Carolyn Heintskill, and lately, Mary Ray. The amount of work turned out by this staff continues to amaze me.

Besides the permanent staff, several visiting scientists have spent periods varying from a few weeks to a year in the Climate Research Group, helping me with their constructive criticism and discussion. The interactions also have led to new research on their part. One of our most creative visitors was Huug van den Dool, originally from Holland but now with the University of Maryland and NOAA. He and I usually see eye to eye on the philosophy and science related to our field, and he has already made fundamental contributions. Another longer-term visiter was Arthur Douglas, now at Creighton University, who is a most enthusiastic young man in long-range forecasting, often making good predictions by intelligently scrutinizing a wealth of meteorological information. Rudy Preisendorfer spent a couple of years with us, working largely with Tim Barnett. He contributed to developing a system of analogue selection. Abraham Oort of GFDL, Princeton, spent a summer helping us with climate-related problems and working on his own research. Joe Egger of Munich spent a few months concentrating on blocking. Many others have visited for shorter periods, including Dr. Julio Buchman, a Brazilian, who seems to have been stimulated by some of my work on drought in the Southern Hemisphere, and is now zeroing in on Brazilian drought in collaboration with other members of our Climate Group, particularly our tropical expert, John Horel. Also, at present, a young lady from Beijing, China, Xiaojun Yuan, is working with me on improving SST forecasts.

In the early 1970s during the Norpax Project, Tim Barnett joined our group, first as academic administrator and later as a research scientist. He proceeded, along with Rudy Preisendorfer, to try out many novel ideas for use in forecasting, including a sophisticated analogue system wherein one finds a similar situation in the past and assumes that the present case will behave in the same way. I helped Tim and Rudy select some of the primary parameters to use in this system, but cautioned them about the long and dismal history of such attempts in the past and of my lack of confidence in this method. Although it is objective and can be employed by relatively unskilled people, it has many flaws, not the least of which is that no two months or seasons are alike over large areas in wind SSTs and weather patterns. For this reason, it is difficult, if not impossible, to find a "good analogue" to the present situation. The track record of analogue forecasts over a period of more than ten years shows wild fluctuations—some predictions have been the opposite of what occurred, while others have had some skill. However, the average performance of temperature forecasts over the United States has shown little success; perhaps with further study and development, the methods may become helpful. At present, studies indicate that the same order of success can be obtained by letting the abnormalities of a month persist into the next season.

I have devoted this amount of time to the analogue method because during my

50-year career, I have seen false hopes generated and large amounts of money spent on this system with little to show for the effort. Perhaps this is a reactionary point of view, but the very philosophy of analogues, which involves short-circuiting understanding, runs counter to mine.

In addition to allowing me to profit from discussions with the immediate members of our Climate Research Group, Scripps gave me the opportunity to confer with many excellent scientists in other parts of UCSD, including physicists, chemists, statisticians and, of course, oceanographers. In the latter group, I must single out Russ Davis, Bob Arthur, Joe Reid, Roger Revelle, Charles Keeling, Rick Salmon, Myrl Hendershott, Walter Munk, Wolfgang Berger, and of course, the late John Isaacs.

The Climate Research Group recently expanded further by merging with the Satellite Group to form a new Climate and Remote Sensing (CARS) unit. CARS is cochaired by Catherine Gautier and Richard Somerville, and brings the total number of scientists in the Climate Research Group to about 20, including Geoffrey Vallis, John Bates, Mark Anderson, Robert Frouin and James Coakley (visiting scientist). This merger represents the conviction that the world observational platforms furnished by the satellite is the wave of the future in climate studies.

Returning to research—as I indicated, the El Niño problem began to become popular in the 1970s and continues with accelerated pace today. This circumstance is in part due to the great El Niño and associated worldwide responses in 1982-83. Thus, El Niño is now almost a household word. In addition to the statistical work on El Niño cited earlier, Dan Cayan and I investigated the uniqueness (or lack of uniqueness) among different El Niño years. This was done because we felt that the literature often gave the erroneous idea that one, and only one, wind and weather pattern over the United States attended this phenomenon. The study of ten such events dating back to 1925 showed little similarity in climate anomalies of temperature, precipitation or wind pattern over the United States. This did not come as a surprise to me, because of hundreds of studies of earlier anomalies attended and not attended by El Niño conditions. Nevertheless, some evidence exists that the extreme Southeast and the Northern Plains did tend to have some common abnormality with El Niño; although even then, this relationship would not be of great help in forecasting—even if one knew there would be an El Niño. This complexity indicates that considerably more research must be performed before true understanding can come about.

The above considerations were detailed in a paper presented at the Paris meeting of TOGA (Tropical Ocean Global Atmosphere). My own feeling is that this vast ten-year undertaking, while well worthwhile, should be renamed GOGA (Global Ocean Global Atmosphere) in view of the multitude of teleconnections that I have studied in my career. It is my conviction that El Niño is generated by abnormalities in the global wind systems, and then influences the weather elsewhere and increases the longevity of the newly developed patterns. Presumably the demise of El Niño is also provided by abnormal wind systems. This sequence seems to take place in many, if not all, long-period air-sea interactions. When J. Bjerknes was alive, he and I spent many hours discussing such problems, and we agreed on the causes and demise of El Niño.

One major event in my career that occurred during the 1970s must be elaborated on. This was in 1978, when I was asked to be the banquet speaker on the occasion of the fiftieth anniversary of the founding of the Department of Meteorology at M.I.T. Preparing this talk took considerable effort and time because I had to dig up pictures and resurrect nostalgic memories of the early days at M.I.T., some of which were

described in this autobiography. The three-day celebration at M.I.T. was attended by hundreds of graduates, staff, faculty members and friends, so it was a great pleasure to address such an audience. After my introduction by my good friend and former professor, Hurd Willett, I was off and running with many anecdotes about Rossby and the early years of the department. My euphoria should be understandable in view of my earlier feelings of insecurity as a "special case" at M.I.T., which I described earlier.

In the following several years, I was to gather a number of honors for research (listed in Biographical Sketch). The most surprising and gratifying of all these honors was election to the National Academy—something I thought would never happen because of the fuzzy nature of my field of research and my poor formal background. When Bill Nierenberg wakened me early in the morning (9 a.m., Washington time) to tell me of my election to the Academy in May 1983, I thought it might be a case of mistaken identity. It is an honor that strengthens my belief in our system, whereby a person is judged solely on the basis of his contributions.

As I conclude the first three quarters of a century of my life, I reiterate that I have been very fortunate in many respects—lucky in selecting a vocation that has never ceased to stimulate me, lucky to often have been in the right places and jobs at the right time, lucky to have had family encouragement, and lucky to have surmounted occasional serious physical obstacles. What more could one ask? A Freudian slip by a typist in a telegram from Kirk Bryan on the occasion of my birthday celebration said, "Write on!" instead of "Right on!" I'll try to follow both directives.

This excellent volume could never have appeared were it not for the diligent and dedicated work of many people. My heartfelt thanks go to John Roads, who organized the symposium, assigned tasks, was not diverted by many well-meaning but impractical suggestions, corraled and persuaded the participants to submit manuscripts, edited this volume, and finally kept morale high among a heterogeneous group.

Others closely involved were Ginny Roberts, who made arrangements for social events, word processed and edited manuscripts, and made excellent general suggestions for the event; Mary Ray, who assisted with diverse tasks, including word processing and dissemination of information; and Marguerette Schultz, whose versatility was helpful in accomplishing many necessary jobs. Deborah Day did a wonderful job in assembling and presenting the pictures and illustrations at the symposium, some of which are to be put on display at the Climate Research Group and Experimental Climate Forecast Center conference room. Dagmaar Grimm did a wonderful job in presenting the pictures and cover in this volume. Most pictures of the event were furnished by Sylvia Somerville. My daughter, Judy, helped both in editing and in organizing my autobiography into coherent sections. Others, too numerous to mention, lent support in a multitude of ways.

To all the above and to the gracious participants, I wish to express my sincere gratitude for making me "King for a Day" and thus providing me with indelible memories.

My mother, Saydie (Jacobs) Namias around 1900.

My father, Joseph Namias in 1925.

I (left) and my brother, Foster, in 1922. At this time, I became a proud Tenderfoot in the Boy Scouts, while my older brother is a Second Class Scout (no derogatory connotation).

My brother, Foster, with the piccolo; and I as accompanist on the snare drum in 1922. We played many of Sousa's marches, probably to the consternation of the neighbors.

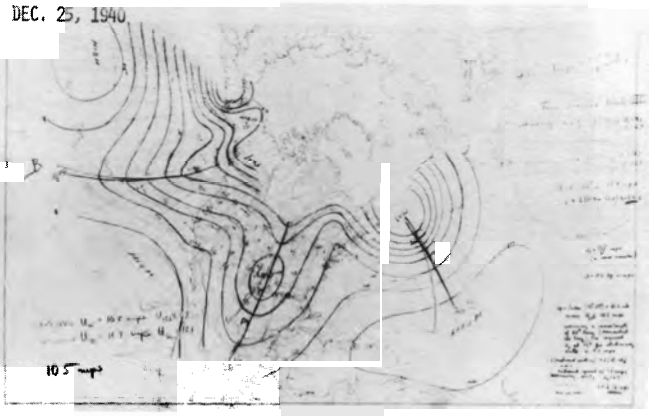
As a member of Troup 3, outside the Bear Tent at Camp Noquochoke in Westport, Massachusetts.

As a Durfee high school student in Fall River, Massachusetts in 1927. Note that I was up with the latest styles, which helped to attract dates. Most clothes were bought with cash earned by house-to-house selling and by playing percussion instruments in the jazz band.

The year (1937) before my marriage, when I was a research associate at M.I.T.

Professor J. Bjerknes, one of my early idols in meteorology. Picture was taken in Washington in 1938.

Upper-air map, 1940. This map represents the first effort at M.I.T. to construct a midtropospheric (10,000 foot isobars) flow pattern over oceans as well as North America. It was constructed by J. Namias, using surface ship reports and was one of the first differential analyses. C. G. Rossby suggested the idea in order to see the larger picture of Rossby waves and more importantly, to test his ideas of the future displacement of these waves. His computations appear at the right of the map.



Another busted forecast! Harry Wexler (right) and I, together with our wives, went on one of our annual bike trips to Cape Cod for a few days in the summer of 1939, only to be caught in a three-day Northeaster and forced to devise makeshift rainwear.

At our summer retreat by my mother-in-law's place in Sharon, Massachusetts, where I fished and swam in Lake Massapoag. This picture was taken in 1941.

Carl-Gustav Rossby and I at meetings of the American Meteorological Society in Washington, 1941.

Edith and I at Grand Rapids, Michigan, where I lectured to 900 Air Force cadets at a time. (Lieutenants with long poles were stationed around the lecture hall to keep the students awake!)

At Bethany Beach, Delaware, where we went to escape the Washington summer heat (1941).

Members of the Extended Forecast Section of the Weather Bureau at a picnic in Washington in 1945. Among those pictured: Donald Martin (later, Colonel, USAF); Tom Gray (became a two-year veteran of the IGY in Antarctica); Hirsh Slater (later to become Colonel Slater, USAF); Professor Tor Bergeron of Uppsala (a visitor) and his wife, Vera; Phil Clapp, my principal research collaborator; Bob Dickson, a forecaster; I and my daughter, Judith; my wife, Edith; Harry Wexler (head of the Weather Bureau Research); Jim Andrews, one of our top forecasters; Joe Smagorinsky and his fiancee, Margaret (guests); Jay Winston and his wife, Ruth (forecaster and researcher); Walter Leight (researcher) and his wife, Fran; Bill Klein, forecaster and researcher, and his wife.

At work on a five-day forecast in 1946. In the picture are Jay Winston, Tom Gray, Jim Andrews, Ralph Bortman, Myrt Wagner, Phil Clapp, Jerome Namias, Doyne Sartor, and Hawy Hawkins. Phil Clapp holds a slide rule for computing constant absolute vorticity trajectories—our substitute for the barotropic model that was to come about ten years later. Slide rule was developed by John Bellamy.

I (with fishing rod), Harry Wexler, and my wife's uncle, preparing for a fishing trip on the Potomac in my arguably "trustworty" rubber boat in 1947.

My daughter, Judy, and Professor von Ficker of Austria, photographed at his retreat in the Tyrol in 1950.

Colonel Merryweather (president of the American Meteorological Society), I, and Ernest Cristie, Head of the New York Weather Office, at the annual meetings of the Meteorological Society in New York in 1949.

In front of the Eniac, Aberdeen Proving Ground, April 4, 1950, on the occasion of the first numerical weather computations carried out with the aid of a high-speed computer. Left to right: H. Wexler, J. von Neumann, M. H. Frankel, J. Namias, J. C. Freeman, R. Fjortoft, F. W. Reichelderfer, and J. G. Charney.

Above (left to right): Sid Teweles, Louis Kaplan. Below: Morris Tepper, Vera Bergeron (wife of Tor), and I—on the occasion of a visit by the Bergerons from Sweden in 1950.

Fishing at Rehoboth Beach, Delaware, while Judy looks on (1952).

Japanese colleagues take me out to dinner in Toyko in 1954. (Geishas not shown.)

With daughter, Judy, in Washington, 1952.

Participants at the Typhoon Conference in Tokyo, 1954.

Members of the Air Force Tokyo Weather Central take us out to a real Japanese dinner, 1954. Col. Don Martin is to the right of Edith at the end of the table.

Behind a glass map in Chicago for an article on developments in weather forecasting in COLLIERS magazine.

With Sir Graham Sutton (then Director-General of the British Meteorological Office) and Harry Wexler, Washington, 1954.

With Dr. F. W. Reichelderfer (middle) and C. G. Rossby (right) in Washington, 1955.

With Harriet Rossby (now Woodcock), Prof. Rossby's wife at a Christmas party at the International Institute of Meteorology in Stockholm, 1955.

Teaching a class of Air Force officers in 1956. The other civilian is Harry Hawkins of our forecasting staff.

1954 on S. S. President Wilson at Capt.'s dinner. Returning from Japan.

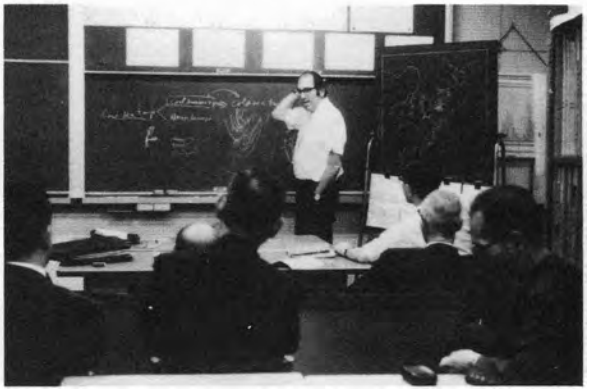
On a chair lift over the Rhine, 1956.

Rossby just before leaving with the Rexes and the Namiases for a three-day trip to the island of Gotland, where Rossby was born and knew every nook and cranny.

I (third from left), with Dr. Del Trono, General Fea, and Dr. Bilanchini, outstanding Italian meteorologists, Rome, 1956.

With daughter, Judy, Arlington, Virginia, 1958.

After a question and answer period at the Japanese Meteorological Office, Tokyo. On my right Dr. Wadachi, the Director, 1960.



Lecturing to Japanese colleagues at the JMA, Tokyo, 1960.

A general lecture to Japanese meteorologists in Tokyo in 1960.

With the new electronic map analyzer at the National Meteorological Center in 1961. This had been part of the dream of von Neumann and Charney now come true.

During an extended period as consultant to the International Anarctic Analysis Center in Melbourne. Colleagues (left to right) are Tom Gray, I, Phillpot, and Gibbs (Director of the Australian Weather Service).

Edith and I at lily-pad pond outside of Washington in 1964.

With a large-mouth bass caught on a vacation trip to Florida in 1964. Fresh water fishing was always my hobby, but not usually with such success.

At the conference on forecasting held in Vienna, 1965. Front row (right to left), Namias, Bleeker, Defant, Reuter. Flohn sits behind Reuter.

At a picnic with the Isaacs and the Ureys near the Salton Sea, 1968. Edith took the picture, so was very much present.

At a dinner during the "Ocean World" Symposium in Tokyo in 1970. Dr. Bjerknes is in the middle, flanked by geishas on either side.

Formal retirement from the National Weather Service in 1970. Joe Smagorinsky was master of ceremonies. Phil Clapp also retired at this time.

With Warren Wooster on the beach outside our home in La Jolla.

Harold Urey (facing camera) with a few of us on the beach at La Jolla, 1971.

Namias, Veronis and Bob Stewart at the Nobel Symposium at Åspenasgarten, near Goteborg, Sweden, 1972.

With Jack and Hedwig Bjerknes at the Mission, not far from San Diego, 1975.

On the estate of Peter Sheppard, outside London, 1976. From left to right: Fleagle, Mrs. Sheppard, Robinson, Namias, Mrs. Fleagle, Tatu Sheppard. Edith, who dislikes being photgraphed, took the picture.

Expounding for the press on air-sea interactions responsible for abnormal winters in the coterminous United States. The "PNA" boys would later ascribe it all to El Niño.

Another photo opportunity, 1977. This picture was published in TIME magazine.

Uncharacteristically, here I am with a bathythermograph on Scripps Pier. My early colleague at M.I.T., Athelstan Spilhaus, its inventor, would be pleased or perhaps amazed.

A favorite photo of mine, 1978, La Jolla.

Signing the "Great Book" on the occasion of my induction into the National Academy of Science, 1984, a year after election.

After receiving an honorary D.Sc. at Clark University, 1984.

With Richard Somerville on the occasion of the celebration of my 75th birthday at Scripps, 1985.

Curriculum Vitae

Date of Birth:	March 19, 1910
Birthplace:	Bridgeport, Connecticut
Father:	Joseph Namias
Mother:	Sadie Jacobs
Wife:	Edith Paipert
Date of Marriage:	September 15, 1938
Daughter:	Judith Ellen Klassen

Education:

B.M.C. Durfee High School
Fall River, Massachusetts

Massachusetts Institute of Technology
M.S. Degree
1932-34; 1940-41

University of Michigan
1934-35

University of Rhode Island
D.Sc. (Hon.) 1972

Clark University, Worcester, Massachusetts
D.Sc. (Hon.) 1984

Experience:

1934	Meteorologist, TWA
1935-1936	Blue Hill Meteorological Observatory Blue Hill Massachusetts
1936-1941	Research Associate, Meteorological Department Massachusetts Institute of Technology
1941-1971	Chief, Extended Forecast Division U. S. Weather Bureau
1968-present	Research Meteorologist, Scripps Institution of Oceanography University of California, San Diego
Summer 1972	Rossby Fellow, Woods Hole Oceanographic Institution Woods Hole, Massachusetts

Awards:

Meisinger Award for Aerological Research, American Meteorological Society, 1938

Citation from Navy Secretary, Frank Knox, for Weather Forecasts in connection with Invasion of North Africa, 1943

Meritorious Service Award from U. S. Department of Commerce, 1950

Award for Extraordinary Scientific Accomplishment from American Meteorological Society, 1955

Rockefeller Public Service Award, 1955 (Year of Research at International Institute of Meteorology in Stockholm)

Department of Commerce Gold Medal Award, 1965

Visiting Scholar, Rockefeller Study and Conference Center, Bellagio, Italy, September 1977

San Diego Press Club, Headliner Award (Science) 1978

Sverdrup Gold Medal, American Meteorological Society, 1981

Marine Technology Society's Compass Distinguished Achievement Award, 1984

UCSD Chancellor's Associates Award for Research, 1984

Department of Commerce Certificate of Appreciation, presented by Malcolm Baldrige, Secretary of Commerce, 1985

Sabbaticals:

Visiting Lecturer	University of Stockholm and Uppsala, Sweden Winter and Spring 1949 and 1955
Visiting Professor	National University of Mexico Mexico City, Summer 1961
Distinguished Visiting Lecturer	The Pennsylvania State University November 1962
Distinguished Visiting Scholar	New York University September 1966 through December 1966

Listed in:

Who's Who in America
Modern Men of Science (McGraw Hill)
World Who's Who in Science
American Men and Women in Science
Who's Who in Technology Today
Dictionary of International Biography (London)
The Blue Book of Leaders in the English Speaking World (Great Britain)

Societies:

Sigma Xi (elected at MIT, 1939)

American Meteorological Society (Fellow)
Councilor during 1940-42, 1950-53, 1960-63, 1970-73

American Geophysical Union (Fellow)

Royal Meteorological Society of Great Britain (Member)

Washington Academy of Sciences (Fellow)

National Weather Association (Member)

Mexican Geophysical Union

American Association for the Advancement of Science (Fellow)

Board of Editors, Geofisica Internacional, Mexico

The Explorers Club (Fellow)

National Academy of Sciences (Member)

American Academy of Arts and Sciences (Fellow)

Publications:

Author of about 200 papers and monographs in scientific literature and numerous encyclopedia articles. These are listed on subsequent pages. In addition, many of these papers have been published as "Short Period Climatic Variations, Collected Works of J. Namias 1934 through 1974, Vols. I and II," 905 pp., and "Vol. III, 1975 through 1983," 393 pp., published by the University of California, San Diego (available from the UCSD Campus Bookstore).

Publications

1.Structure of a wedge of continental polar air determined from aerological observations. Massachusetts of Technology Meteorological Course, Professional Notes No. 6., 41 pp.

2.Specific humidity as a conservative element. *Bull. Am. Meteor. Soc.,* **15**, 24-26.

3.Subsidence within the atmosphere. *Harvard University Press*, Harvard Meteorological Studies, the Blue Hill Meteorological Observatory of Harvard University, No. 2., 61 pp.

4.Some aspects of the surface of subsidence. *Transactions of the Am. Geophys. Union, Fifteenth Annual Meeting*, 105-114.

5.Structure and maintenance of dry-type moisture discontinuities not developed by subsidence. *Mon. Wea. Rev.,* **64**, 351-358.

6.An introduction to the study of air mass analysis. *Bull. Am. Meteor. Soc.,* **17**, 84 pp.

7.Thunderstorm forecasting with the aid of isentropic charts. *Bull. Am. Meteor. Soc.,* **19**, 1-14.

8.with R. G. Simmers. Examples of isentropic analysis. Presented at the Meteorological Conference at M.I.T., September 6-9, 34 pp.

9.with H. Wexler. Mean monthly isentropic charts and their relation to departures of summer rainfall. *Transactions, Am. Geophys. Union*, Joint Meeting, Meteorology and Oceanography, 164-170.

10.The forecasting significance of anticyclonic eddies on the isentropic chart. *Transactions, Am. Geophys. Union*, Nineteenth Annual Meeting, 174-176.

11.with C. G. Rossby, et al. Application of fluid mechanics to the problem of the general circulation of the atmosphere. *Transactions, Am. Geophys. Union*, Nineteenth Annual Meeting.

12.Isentropic analysis as a practical tool of weather forecasting. Massachusetts Institute of Technology, 13 pp.

13.with H. C. Willett and B. Haurwitz. An introduction to the study of air mass analysis, Fourth Edition. *Amer. Meteor. Soc.*, 122 pp.

14.Technique and examples of isentropic analysis. (Papers in physical oceanography and meteorology, included in Fluid Mechanics Applied to the Study of Atmospheric Circulations.) *Massachusetts Institute of Technology and Woods Hole Oceanographic Institution,* **VII** (1), 18-71.

15. The use of isentropic analysis in short term forecasting. *J. of Aeronautical Sci.,* **6**, 295-298. 16.On the dissipation of tall cumulus clouds. *Mon. Wea. Rev.,* **67**, 294-296.

17.Two important factors controlling winter-time precipitation in the southeastern United States. *Transactions, Am. Geophys. Union*, 341-348.

18.with R. A. Allen, et al. Report on an experiment in five-day weather forecasting. (Papers in physical oceanography and meteorology.) *Massachusetts Institute of Technology and Woods Hole Oceanographic Institution,* **VIII** (3), 94 pp.

19.with H. C. Willett, R. A. Allen. Report of the five-day forecasting procedure, verification and research as conducted between July 1940 and August 1941. (Papers in physical oceanography and meteorology) *Massachusetts Institute of Technology and Woods Hole Oceanographic Institution,* **IX** (1), 88 pp.

20.The relation between 10,000 foot zonal index and speed of cyclones. Five-Day Forecast Section, U. S. Weather Bureau, 5 pp.

21.with P. F. Clapp. Use of trend methods in forecasting five-day mean pressure charts. Extended Forecast Section, U. S. Weather Bureau, 60 pp. 22.Methods of extended forecasting practiced by the five-day forecasting section. Extended Forecast Section, U. S. Weather Bureau, 64 pp.

23.with K. Smith. Normal distribution of pressure at the 10,000 foot level over the Northern Hemisphere. Extended Forecast Section, U. S. Weather Bureau, 16 pp.

24.with P. F. Clapp. Studies of the motion and development of long waves in the westerlies. *J. of Meteor.*, **1**, 57-77.

25.Construction of 10,000 foot pressure charts over ocean areas. *Bull. Am. Meteor. Soc.*, **25**, 175-182.

26.Investigations of polar anticyclogenesis and associated variations of the zonal index. U. S. Weather Bureau, Research Paper No. 24, 22 pp.

27.with P. F. Clapp. Normal fields of convergence and divergence at the 10,000 foot level. *J. of Meteor.*, **3**, 14-22.

28.Extended forecasting by mean circulation methods. Extended Forecast Section, U. S. Weather Bureau, 89 pp.

29.Characteristics of the general circulation over the Northern Hemisphere during the abnormal winter 1946-47. *Mon. Wea. Rev.*, **75**, 145-152.

30.Physical nature of some fluctuations in the speed of the zonal circulation. *J. of Meteo.*, **4**, 125-133.

31.Remarks on long-range forecasting. *Weatherwise*, April, 5 pp.

32.Evolution of monthly mean circulation and weather patterns. *Transactions, Am. Geophys. Union*, **29**, 777-788.

33.with P. F. Clapp. Confluence theory of the high tropospheric jet stream. *J. of Meteor.*, **6**, 330-336.

34.Basis for extended forecasting as practiced in the United States Weather Bureau. *The Meteorological Magazine*, **78**, 360-361.

35.The index cycle and its role in the general circulation. *J. of Meteor.*, **7**, 130-139.

36.Meteorology in navigation: general circulation of the upper troposphere and lower stratosphere. *The Science in Navigation*, Institute of Navigation, New York, pp. 62-74.

37.The great Pacific anticyclone of winter 1949-50: a case study in the evolution of climatic anomalies. *J. of Meteor.*, **8**, 251-261.

38.with P. F. Clapp. Observational studies of general circulation patterns. *Am. Meteor. Soc.*, Compendium of Meteorology, 551-567.

39.General aspects of extended range forecasting. *Am. Meteor. Soc.*, Compendium of Meteorology, 802-813.

40.with W. Leight. The current long-range forecasting program of the U. S. Weather Bureau. *The Scientific Monthly*, **74**, 21-28.

41.Problems associated with extending the time range of weather prediction. *Transactions of the New York Academy of Sciences, SER. II*, **14**, 177-179.

42.with W. A. Mordy. The February minimum in Hawaiian rainfall as a manifestation of the primary index-cycle of the general circulation. *J. of Meteor.*, **9**, 180-186.

43.The annual course of month-to-month persistence in climatic anomalies. *Bull. Am. Meteor. Soc.*, **33**, 279-285.

44.The jet stream. *The Scientific American*, **187**, 26-31.

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