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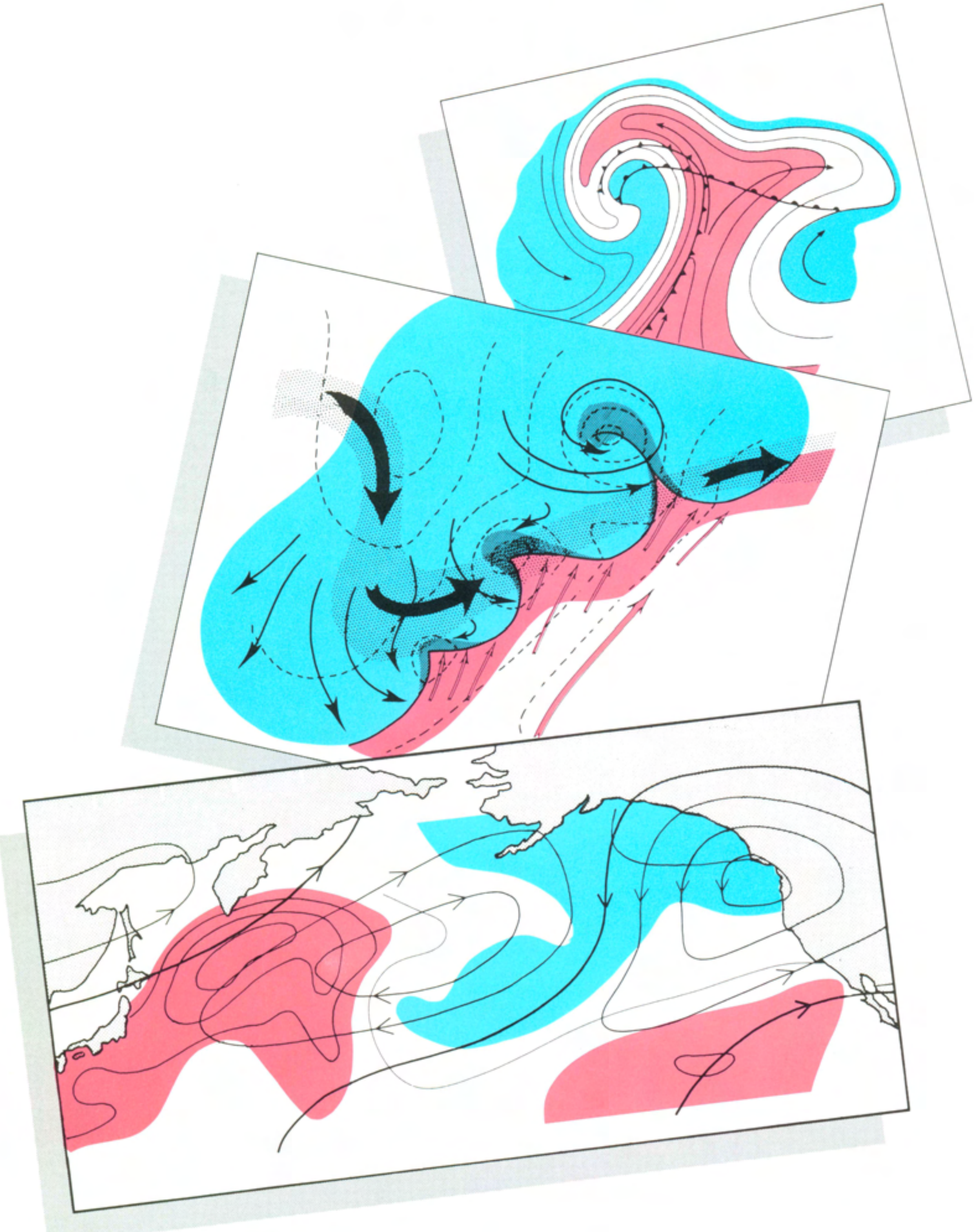

Scripps Institution of Oceanography
University of California, San Diego

NAMIAS SYMPOSIUM

Edited by

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La Jolla, California

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This is the limited first edition of the Namias Symposium published by the Experimental Climate Forecast Center of Scripps Institution of Oceanography, University of California, San Diego.

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The cover shows three favorite meteorological drawings of Namias. From top to bottom they represent, work on isentropic analysis, synoptic analysis and atmosphere-ocean interactions.

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Preface

On October 22, 1985, the Experimental Climate Forecast Center of the Climate Research Group at Scripps Institution of Oceanography, University of California at San Diego, held a special symposium in honor of the 75th birthday of Dr. Jerome Namias. The all-day symposium consisted of nine scientific talks on topics relevant to his life's work, presented by some of the world's leading meteorologists, including Dr. Richard A. Anthes, NCAR; Prof. Arnt Eliassen, University of Oslo, Norway; Prof. Dave Fultz, University of Chicago; Dr. Donald Gilman, National Weather Service; Prof. Robert L. Haney, Naval Postgraduate School; Dr. Chester W. Newton, NCAR; Dr. Joseph Smagorinsky, President of the American Meteorological Society; Harry van Loon, NCAR; and Prof. John E. Walsh, University of Illinois. Presentations were also made by Daniel R. Cayan, Dr. William Nierenberg and Prof. R. Somerville of Scripps. At the end of the symposium, Dr. Alan Hecht, Head of the National Climate Program Office, presented Dr. Namias with a plaque signed by Secretary of Commerce, Malcolm Baldridge, and Administrator of NOAA, Anthony Calio.

The original idea for this gathering was suggested by Prof. Larry Gates, Oregon State University, and then initiated by Prof. Richard Somerville who asked if I would take charge of the affair. This volume is the final result of the party. It contains Namias's autobiography, the hardcopy of the talks that were given at the symposium and a talk that would have been given by Prof. Edward N. Lorenz, Massachusetts Institute of Technology, had he been able to attend in October. The symposium was supported in large part by NOAA through the helpful efforts of Dr. William Sprigg. I am also grateful for the support from Scripps Institution of Oceanography through Dr. William Nierenberg and from the Climate and Remote Sensing Group at Scripps through Dr. Catherine Gautier.

Several people were especially helpful in putting out this work. Virginia Roberts helped with the editing and word-processing; Mary Ray helped with the word processing; Marguerette Schultz helped to set the figures into the text; Dagmar Grimm did the final setup of the photos and cover as well as provide useful advice; Sylvia Somerville provided photos of the day's events; the SIO photo lab provided the group photo; Don Betts shared his experience on setting up this type of book; and at the end Dick Crumrine sent the proofs to the printer.

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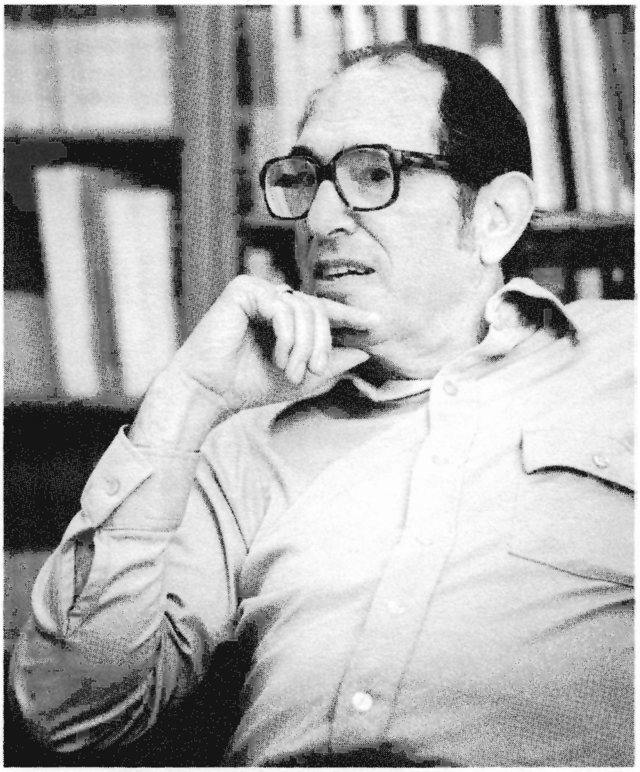
AUTOBIOGRAPHY



Jerome Namias
Scripps Institution of Oceanography

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 graphical 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.

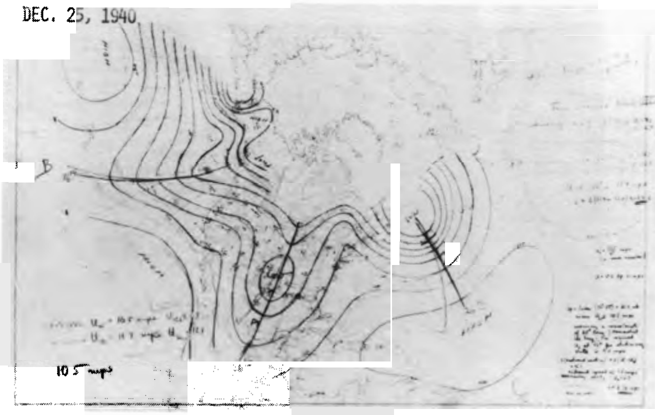
As a member of Troup 3, outside the Bear Tent at Camp Noquochoke in Westport, Massachusetts.

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.

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

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.

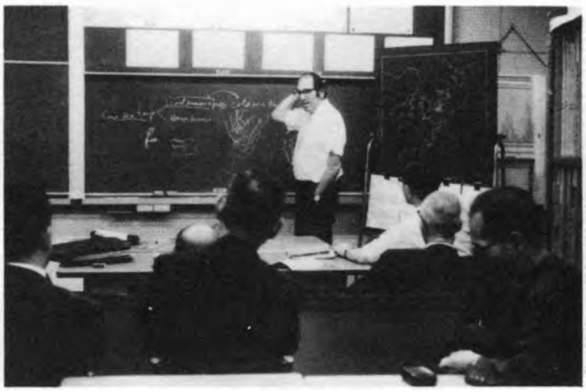
On a chair lift over the Rhine, 1956.

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).

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

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Experimental Climate Forecast Center,
Climate Research Group and Climate and Remote Sensing Group

left to right (standing): L. Volfson, S. Iacobellis, T. Tubbs, J. Horel, T. Barnett, M. Ray, M. Schultz, J. Roads, G. Vallis, C. Gautier.
left to right (seated): D. Cayan, B. Chertock, R. Somerville, C. Baxter, H. Panofsky, J. Namias. Absent: V. Roberts, M. Sullivan.

Dan Cayan giving a roast of Namias.

The Long Range Eye of Jerry Namias

Joseph Smagorinsky
President-Elect, American Meteorogical Society

Namias' very early perceptions of the interactive role of the atmosphere with its lower boundary in shaping future events deserve special attention. Anomalous sea-surface temperature, soil moisture and snow cover "feed-backs" provided a long term "memory" to the atmosphere, not only locally, but also remotely through "teleconnections". So what's new?

Personal Antecedents

As I recall it, I first became aware of Namias in 1943, when I was an Air Corps meteorology cadet at MIT. It was references in the AMS monograph on air-mass and isentropic analysis, published in 1940 [1], that attracted my attention. At this tender point in my own beginning career, Jerry was already an established figure in the literature.

But it wasn't until 1947 (in August) that I first actually met him. It was at a picnic of his Extended Forecast Section of the Weather Bureau (Figure 1). I was accompanying my bride-to-be. At the time, I was a kid of 23 and he was a middle-aged man of 37. Well, Jerry is still a middle-aged man, but the rest of us have gotten a lot older.

In June 1948, I joined the Weather Bureau as a very junior employee and thereupon began my long professional relationship with Jerome Namias.

My Goal Today

When I was invited to help celebrate Jerry's diamond birthday, many things ran through my mind. Where does one start in paying tribute to his extraordinary talents? There is so much that I have admired and there is so much that our science and profession owe him.

Of all that one can say, I have decided to try to exemplify his contributions by a brief analysis of his early track record on the interactive influence of the atmosphere's lower boundary on its longer-term behavior.

Namias was so far ahead of his time, empirically, conceptually and semantically, that most of his ideas were not immediately picked up, but had to be rediscovered some years later.

To accomplish my task, I have gone to Namias' 3 volumes of Collected Works. (I guess it's still only three volumes.) As it turns out, to make my point, I need only refer to little more than a dozen of his papers published between 1948 and 1963. My problem was to keep my eye on my single-minded objective, and not wander off into so many of the other things that he has written between 1934 and this morning. Of course, I will be making quotations from Namias' white book.

Figure 1. *August, 1947 picnic. P. Clapp, H. Wexler, J. Namias and J. Smagorinsky, left to right.*

Precedents

Before 1948, Namias concerned himself with a variety of atmospheric properties and phenomena newly revealed by the aerological network. This ranged from the dissipation of tall cumulus clouds, to subsidence and to the jet stream. He recognized the power of isentropic analysis as a diagnostic tool for the study of the larger scale motions of the atmosphere, and already showed a curiosity about fluctuations in characteristics of the general circulation, no doubt influenced by Rossby. In particular, Namias wrote several papers having to do with changes in the intensity of the westerlies and the development and propagation of long waves. In the latter 40s, he began to consider longer period phenomenology: such as normal fields of convergence and divergence, and the abnormal winter of 1946-47. This is where we pick him up.

Starting with 1948

In 1948, Namias published a paper [2] in the Transactions of the American Geophysical Union entitled "Evolution of monthly mean circulation and weather patterns". It was an attempt to summarize some main conclusions drawn from 6 years of experience in the preparation of monthly mean forecasts:

"(1) that regardless of how they are made up, monthly mean charts, particularly those constructed for mid-tropospheric levels, to a large extent determine monthly mean temperature and precipitation anomalies; and

(2) monthly mean mid-troposphere flow patterns of the general circulation, when

treated on a hemisphere-wide scale, undergo an orderly evolution and development which can be rationalized at least qualitatively by use of physical and kinematical principles."

The interpretation and value of monthly mean maps was very controversial at the time, and I think there still are some skeptics today. But the second conclusion is the one I want to dwell on. Namias was not only an extraordinary and consummate observer of nature, perceiving order where most others could see only chaos, but also he hardly ever hesitated to interpret what he saw. He was, and still is, the quickest gun in town to offer a hypothesis. In this case he offered three, which he attributed to predecessors in the foregoing century. He suggested that the original large-scale disturbances may be due to:

"(1) Extraterrestrial influences upon the Earth's upper atmosphere, particularly the effect of variations in the energy of selected bands of the Sun's spectral radiation on the atmosphere's ozone layer.

(2) Terrestrial influences, particularly variations in snow cover and temperature of ocean currents.

(3) Variations in the character and amount of nuclei of condensation and/or other atmospheric suspensoids."

As far as I know, this was Namias' first mention of possible effects (though not yet interactions) of snow cover and sea surface temperature. He dropped the idea of condensation nuclei or suspensoids in later papers, but he hung on to the possible influence of solar variations for surprisingly long, as we shall see.

In a subsequent paper in 1950 [3] on the role of the index cycle in the general circulation, Namias marveled at the coherence of this atmospheric phenomenon for intervals ranging from four to six weeks, but confessed "that its usefulness in the practice of extended forecasting has been somewhat disappointing". Nevertheless, in recognition of it's symptomatic significance, he set out to comment on three characteristics of the index cycle (and I paraphrase):

(1) The main poleward heat transfer occurs sporadically, at least once a year, during the low index, blocking phase of the cycle.

(2) Interannual variations in the intensity of the index cycle are connected with varying locations of the "quasi-permanent anchor troughs and ridges of [the] mid-troposphere" in different years; "it is quite possible that lag effects of ocean currents, snow cover, etc., are dominant factors".

(3) There appears to be one particular period (in late February) when the most pronounced cycle appears. The two essential ingredients, an extensive cold [polar] reservoir [providing maximum available potential energy for baroclinic instability] and Atlantic blocking are both favored at this time of the year.

Although these were offered as explanations, only the second one, on interannual variability, qualifies as such. And this is a reassertion of a rationale that was to develop over the years in Namias' line of thinking and personal research.

In the following year, Namias undertook a study of the winter of 1949-50 [4]; and, as usual, his main tools were the time mean 5, 15 and 30-day surface and 700 mb geopotential maps. He was ready to more firmly offer a hypothesis for the long life of a "vast warm anticyclone [which] moves in a great arc from the southeast Pacific into the Bering Sea and Canadian Yukon" and its influence on "Pacific storm tracks and anomalies over the United States". Shades of the PNA!

In this paper he referred to Rossby in declaring that "once one large-scale feature of the general circulation is established, it attempts to mold other features of the circulation in far distant areas, largely through the flux of vorticity". But then Namias went on with what must be his own intuitive idea. It is typical of his ability to reason non-linearly, as if he were an intimate part of the system itself, but willing to reveal its secrets. And I quote:

"But this attempt may conflict with or be reinforced by the effects of differential heating. The final state of the mean circulation for a period of, let us say, a month is the result of the interplay between differential heating and mutual dynamic interactions of the components of the great mid-tropospheric wave patterns. Thus, differential heating may operate on a given initial state to encourage one flow-pattern, but that pattern is rearranged through mutual interactions of troughs and ridges into a more harmonious and stable assemblage of component parts." And so colorfully said!

In 1952, he wrote on the annual course of month-to-month persistence [5], and it is sufficient to directly quote his concluding paragraph:

"An analysis of the year-to-year behavior of persistence suggests possible secular variations most pronounced in winter and spring with possible oscillations of the order of a decade. While the data are insufficient to test a possible solar cause of these secular variations, at least they are not easily related to solar variations. The long-period variations in persistence of contour patterns during cold seasons appear to be related to the mid-tropospheric zonal index in the sense that greater persistence accompanies low index. This relationship may suggest self-perpetuating controls operating preferentially during [the] sluggish air flow period through earth-bound conservatizing factors like snow cover, soil moisture, and ocean temperature."

There, he said it! A little obscured by his characteristic picturesque language but, nevertheless, he said it.

In 1954, in a paper published in the Bulletin of the AMS [6], Namias still puzzled over "what determines the positions and intensities of the centers of actions both in their normal and abnormal states." He restated his inclination "to join the 'old fashioned' school of meteorologists which believes that abnormal circulations may bring about abnormal conditions at the surface (e.g., snow cover, ocean temperatures, etc.) which may substantially play a regulatory role in determining atmospheric circulations."

In the same year, he wrote the first in a series of papers [7] tying genesis and paths of tropical cyclones to larger-scale longer-term fluctuations of the general circulation and climate.

In a 1955 MWR paper on drought [8], Namias repeated that persistent anomalies could be influenced by "the differing effect of various surfaces (snow cover, open water, bare land, etc.)", and attributed the possibility of extraterrestrial causes to "another school of thought". So he finally got it out of his system.

He also mentioned in passing "the impact upon various air flows of mountain chains". I doubt whether Namias foresaw the orographic implications on multiple-equilibria. However, I do feel that he understood early-on that certain, more persistent states, were inherently more predictable than were the more volatile transitions. The main question unanswered at this point was the reason why. Was it primarily the result of non-linearities of the atmosphere-lower boundary system or could it be explained entirely in terms of non-linearities within the atmosphere itself?

But even today, the last county has still to be heard from. It may very well be that both types of non-linearity are important, probably depending on the time scale of variability.

We now skip to 1958 [9], when Namias pointed out that interrelations between the subpolar cyclonic centers of action and the subtropical anticyclones were already known by Walker in 1930. Namias suggested that such sea-saw "interconnections [he hadn't yet used 'teleconnections'] and many others of a regional nature often are associated with 'index cycles' which last from 3 to 6 weeks".

But "while the index cycles ..are primarily cold season phenomena, there are apparently great and persistent abnormalitites of the hemispheric circulation which also occur in summer. One of these aberrations which has strong influence on the seasonal characteristics of much of the Northern Hemisphere occurs when a girdle of persistent positive anomalies appears in mid-latitudes, and is generally associated with a northward displacement of the subtropical anticyclones." He then referred to a year-earlier paper where he suggested that there may be a connection between summers characterized by circulations of this anomalous type and the adjacent winters.

We now come to Namias' first published attempt to document relationships between North Pacific waters and the overlying (not remote) atmospheric circulation. It was in a 1959 paper in the JGR [10]. That study, confined to the Pacific basin north of 20 deg latitude, described conditions from the summer of 1957 through the spring of 1958. Namias found that anomalous warming of the surface waters of the eastern North Pacific was related to prevailing abnormalities in the overlying circulation. He ascribed the long-period continuity to a "feedback" (his terminology) "between ocean and atmosphere against the slowly changing climatological background." Changes in surface wind were considered to be a decisive interactive factor on ocean surface temperature.

Of course, all of these papers had a great deal of detail in them, both in the empirical evidence offered and in the development of his arguments and explanations. Many of his hypotheses and conclusions were reasserted in his prolific productivity. But one can see how a highly intuitive early hunch came back more than once to be restated and reenforced with more data, though not always convincing to his contemporaries. For example, in still another paper on persistence in 1959 [11], Namias concludes "evidence seems to be accumulating that in seeking the reason for long-period persistence, one must not only examine further anomalous surface conditions, perhaps brought about by preceding circulations and weather, but also the inherent hydrodynamic stability of different mean flow patterns at certain times of the year." Would he have felt at home with the notion of multiple-equilibria if it had been known then?

For a conference on numerical weather prediction in Tokyo in 1960, Namias prepared a paper [12] entitled "Influences of abnormal surface heat sources and sinks on atmospheric behavior". And it was abnormal behavior that Namias was concerned with. He tried to give specific examples of evidence of the influences of variable sea surface temperature, soil conditions and snow cover back on the overlying atmosphere as well as remotely. The latter spatial interdependence he termed "teleconnection". It is the first reference to the term in Namias' papers, but I am not certain whether it was original with him.

By 1963, Namias was beginning to openly admit his full appreciation for the utter complexity of the nature of large-scale air-sea interactions. In a JGR paper [13] he notes: "The feedbacks envisioned are not simple cause and effect relationships but are complexly coupled mechanisms established by the eternal abnormailty of the large-scale states of both atmosphere and sea. This type of interaction renders futile any attempt to discover an 'ultimate cause' of climatic anomalies in air or sea, because one abnormal state in either medium leads to abnormalities in the other, and the longevity of the disturbed condition differs between atmosphere and ocean." And that's Namias' way of expressing the notion of fast and slow manifolds.

These extraordinary insights suffered one essential flaw: Namias' maps stopped at 20 deg north latitude. However, in another paper later that year in the MWR [14], Namias set out to see if he could relate events in the two hemispheres. With a knowledge of Walker's earlier work on the Southern Oscillation, he reasoned that regional (that is, longitudinal) variations in the position of the Hadley cell (that is, the Walker circulations) would be the agent for coupling the displacements of planetary waves in temperate latitudes.

At this point we can say with 40/40 hindsight that Namias failed to appreciate the possible role of the equatorial oceans. Namely that the coupling of the atmosphere and oceans in the equatorial tropics was singular, in that the reaction time of the ocean was very short because it was highly stratified, and the reaction of the atmosphere was very deep because of the dominance of convection. The net result was a much shorter time scale and a much more direct interaction between atmosphere and ocean than elsewhere.

This, as we now know, is still not the whole story, but it no doubt is an essential chapter. However, it is also becoming clear that the extra-equatorial chain of events in the atmosphere and oceans over intervals of seasons to years cannot be understood with the tropics excised. It took another remarkable observer of nature to fill that gap in the latter 1960s, Jacob Bjerknes. I have never looked upon Namias and Bjerknes as competitors. Rather the extraordinary clarity with which they each viewed rather limited observational data in both media, together have contributed to stimulating a whole new and exciting era of scientific inquiry on a subject of enormous intellectual and practical importance. It is indisputable that Namias and Bjerknes are two of the few intuitive giants in our field this century.

I guess the question that is left begging is just what is the role of the extra-equatorial oceans? And to that Jerry would probably say: WHAT??!! We know that normally the oceans greatly influence the atmosphere off east coasts in winter during cold outbreaks, when convection over the warm coastal currents carries the heat deeply into the atmosphere. Also, rather pronounced low level air-mass modification normally occurs as a result of upwelling along west-coasts. But what about the influence of atmosphere on the ocean? Are Namias' empirical large-scale findings a reflection of secondary interactions in the extra-tropical latitudes? They certainly cannot be ignored.

At this point it is appropriate to quote the last paragraph of one of the last papers [15] of the first volume of Namias' Collected Works, also dated 1963. It sums things up quite well at that point in Namias' history:

"...some influences external to the atmosphere must be called upon to provide a 'memory' in order to cause the persistence and persistent recurrence. These influences might well be provided by abnormalities in the surface both at sea and on land...; abnormalities created in the first place by circulations which remain anomalous in the same sense over intervals at least a season in length. The author hopes to throw further light on this special case by more exhaustive study." And, as we know, that he did!

Postscript

I just note the following in passing. Upon examining the current menu of key subjects of the World Climate Research Program, see how quickly you recognize some of Jerry's adolescent playthings: seasonal to interannual prediction; the role of the oceans in climate variability; land-surface processes including the ground hydrology; the cryosphere and its interactions with the climate system.

It's tempting to continue to track Jerome Namias' ideas and findings to more recent times. I'm sure that others today will uncover many other facets. My object was to show how early on it was that Jerry appreciated the importance and the nuances of many of the basic ideas that have made interannual variability one of today's hottest research topics in meteorology and oceanography. And, in the process, he also greatly enriched our vernacular.

I have always considered it a privilege to have been Jerry's friend and colleague. I am grateful to our hosts for inviting me to participate in this party.

[1] 1940: *An introduction to the study of air-mass and isentropic analysis*, 5th edition (revised and enlarged), Ed. Robert G. Stone, October 1940, American Meteorological Society, Boston, MA, 232 pp.

[2] 1948: Evolution of monthly mean circulation and weather patterns. *Transactions American Geophysical Union*, **29**, 777-788.

[3] 1950: The index cycle and its role in the general circulation. *Journal of Meteorology*, **7**, 130-139.

[4] 1951: The great Pacific anticyclone of winter 1949-1950: a case study in the evolution of climatic anomalies. *Journal of Meteorology*, **8**, 251-261.

[5] 1952: The annual course of month-to-month persistence in climatic anomalies. *Bulletin of the American Meteorological Society*, **33**, 279-285.

[6] 1954: Further aspects of month-to-month persistence in the mid-troposphere. *Bulletin of the American Meteorological Society*, **35**, 112-117.

[7] 1954: Long-range factors affecting the genesis and paths of tropical cyclones. *Proceedings of the Unesco Symposium on Typhoons*, 213-219.

[8] 1955: Some meteorological aspects of drought with special reference to the summers of 1952-54 over the United States. *Mon. Wea. Rev.*, **83**, 199-205.

[9] 1958: The general circulation of the lower troposphere over Arctic regions and its relation to the circulation elsewhere. *Polar Atmosphere Symposium, Part 1, Meteorology Section*, Pergamon Press, London, 45-61.

[10] 1959: Recent seasonal interactions between North Pacific waters and the overlying atmospheric circulation. *J. Geophys. Res.*, **64**, 631-646.

[11] 1959: Persistence in mid-tropospheric circulations between adjacent months and seasons. *Rossby Memorial Volume*, Oxford University Press, NY, 240-248.

[12] 1962: Influences of abnormal surface heat sources and sinks on atmospheric behavior. *The Proceedings of the International Symposium on Numerical Weather Prediction*, Tokyo, Nov. 7-13, 1960, Meteorological Society of Japan, 615-627.

[13] 1963: Large-scale air-sea interactions over the North Pacific from summer 1962 through the subsequent winter. *J. Geophys. Res.*, **68**, 6171-6186.

[14] 1963: Interactions of circulation and weather between hemispheres. *Mon. Wea. Rev.*, **91**, 482-486.

[15] 1963: Surface-atmosphere interactions as fundamental causes of drought and other climate fluctuations. *Proceedings of Rome Symposium on Changes of Climate*, Arid Zone Research XX, Unesco and WMO, 345-359.

A Method Pioneered by Jerome Namias: Isentropic Analysis and its Aftergrowth

Arnt Eliassen

Although somewhat impractical, isentropic analysis is theoretically attractive, being directly connected with basic dynamical quantities such as potential vorticity and available potential energy. In recent years this method and the corresponding use of isentropic coordinates have led to a number of interesting results.

Back in 1939 when I was a graduate student in Oslo with Einar Höiland, Sverre Petterssen and Halvor Solberg as teachers, aerological observations had just recently begun to reveal a three-dimensional picture of air flow in the troposphere and lower stratosphere. I was lucky to belong to a little group of enthusiastic students. We studied the aerological papers by Jabob Bjerknes, Erik Palmén, Jacques Van Mieghem, and Jerome Namias. We had not met these authors, but I envisioned them, including Jerry, as dignified old scientists. It was quite a surprise, when I met Jerry after the war, to find a young-looking person not much older than I. The Namias papers which we had studied as students were written by a youth no more than 23 years old.

In the 1930s many American atmospheric scientists, and some European, were gathering at MIT under the leadership of Carl-Gustaf Rossby. Rossby (1932) proposed to use potential temperature and specific humidity for the purpose of identification of air masses, since these quantities are conserved in dry-adiabatic processes. Jerry Namias was quick to make use of Rossby's suggestion, and his subsidence paper from 1934, which even today is very much worth reading, contains cross-sections with isentropes rather than isotherms, perhaps for the first time (Fig. 1).

Beginning in 1937, charts of potential temperature surfaces, called isentropic charts, were drawn regularly at MIT. Jerry Namias was mostly responsible for the construction of these maps, although Hurd Willet, Harry Wexler, and others participated in the work. The maps contained contour lines and lines of constant specific humidity. Dry and moist tongues of air could be identified and followed from day to day. They represented cyclones, anticyclones and long waves in a new perspective. Fig. 2 shows an isentropic chart from one of Jerry's early publications (1939). From consecutive maps, it was possible to trace approximate three-dimensional air trajectories. Another objective was to study mixing processes, which were assumed to take place mainly in the isentropic surfaces. Thus the isentropic charts represented research tools with many possibilities.

In 1937 Raymond Montgomery, a young member of Rossby's research group, showed that the horizontal pressure gradient force could be measured on the isentropic map as the negative gradient of the sum of geopotential and dry enthalpy. This quantity is called the Montgomery stream function or, as I prefer, the Montgomery potential. Thus, geostrophic winds can be obtained from isolines of Montgomery potential on isentropic maps in just the same way as from contour lines on isobaric maps.

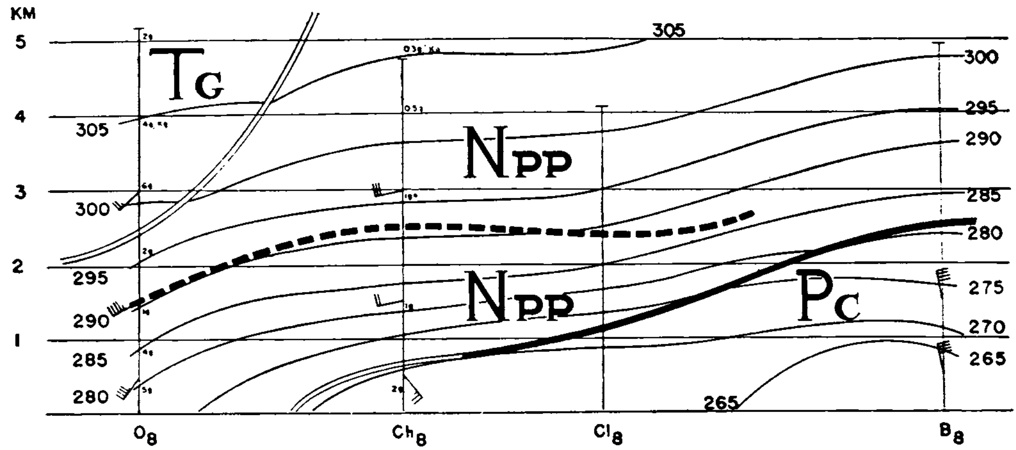

Figure 1. *Cross-section Omaha-Chicago-Cleveland-Boston; December 8, 1931, with potential isotherms and air mass boundaries. Air mass designations: TG - tropical Gulf, PC - polar Canadian, NPP - transitional polar pacific. From Namias (1934).*

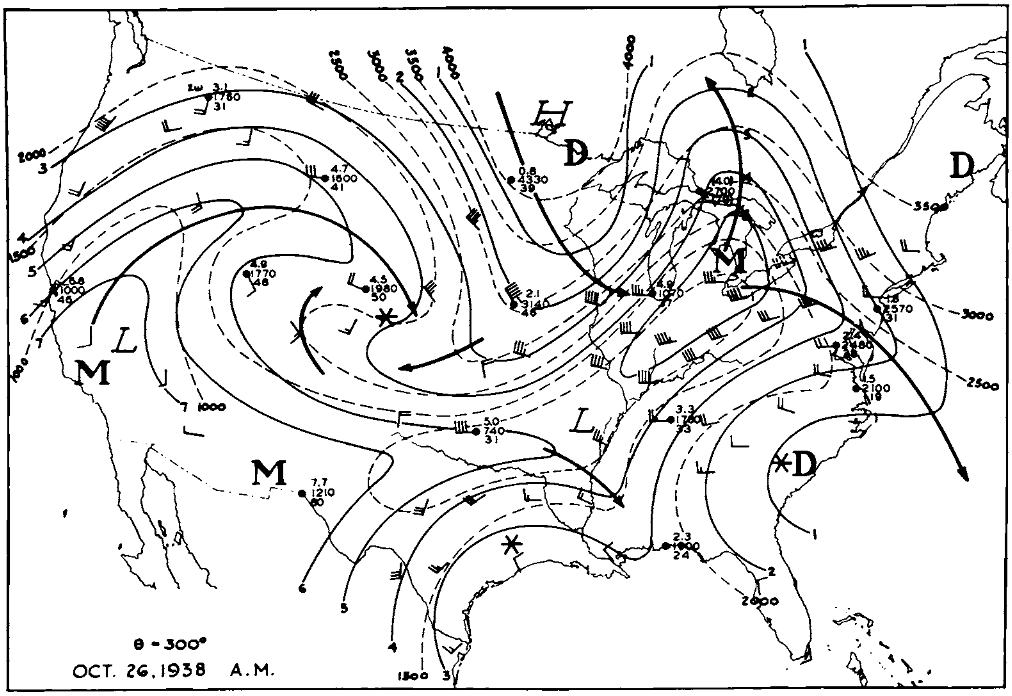

Figure 2. *Isentropic chart for October 26, 1938. Solid lines are lines of constant specific humidity, dotted lines are contour lines, in meters. M - moist, d - dry. From Namias (1939).*

In spite of these favorable properties, isentropic maps were never much used in practical weather service. The construction of such maps were time-consuming, and besides, they are not well suited for displaying weather conditions, neither at the ground nor for aviation, because of their pronounced inclination. After all, air planes follow isobaric, not isentropic surfaces.

From a theoretical viewpoint, however, isentropic analysis has certain attractive properties, and the corresponding mathematical representation, i.e. the use of potential temperature as vertical coordinate, has in recent years received increased attention. Many theoretical results can be expressed simpler and more precisely in such isentropic coordinates. Of course, this is true primarily for processes of short time scale which may be considered as dry-adiabatic; but the advantage of isentropic coordinates is not limited to dry-adiabatic processes.

During World War II, Rossby (1940) and H. Ertel (1942) independently derived the conservation theorem for potential corticity. In Ertel's version, the potential vorticity is $Q = \alpha(2\boldsymbol{\Omega} + \nabla \times \mathbf{v}) \cdot \nabla\theta$, (symbols explained in Appendix) and he showed that this quantity is an individual constant in dry-adiabatic, frictionless flow.

If we take account of the hydrostatic approximation, which is appropriate for large-scale motions, Ertel's expression is modified into: $Q = \alpha(f\mathbf{k} + \nabla \times \mathbf{v}_h) \cdot \nabla\theta$. In pressure coordinates, this becomes

$$Q = g[(f + \zeta_p)(-\frac{\partial\theta}{\partial p}) + \frac{\partial v}{\partial p}(\frac{\partial\theta}{\partial x})_p - \frac{\partial u}{\partial p}(\frac{\partial\theta}{\partial y})_p]$$

Here the first term in the brackets is a barotropic contribution, whereas the two remaining terms represent the effect of baroclinicity.

In isentropic coordinates, however, the barotropic and baroclinic contributions combine into just one term: $Q = g(f + \zeta_\theta)/(-\frac{\partial p}{\partial\theta})$.

This is nearly the expression originally given by Rossby (1940), viz.: $Q = (f+\zeta_\theta/\Delta$, where Δ is the weight per unit horizontal area of as isentropic layer. Rossby also coined the name "potential vorticity".

One of the first to exploit the potential vorticity theorem was Ernst Kleinschmidt from West Germany. In two remarkable papers from 1950, he studied the distribution and the properties of potential vorticity both synoptically and theoretically.

Kleinschmidt intuitively realized the central role played by potential vorticity in atmospheric dynamics. For instance, he knew that the criterion of symmetric stability of zonal current is $fQ > 0$ (not $Ri > 0$ as is sometimes stated).

He conjectured that the distribution of Q in $xy\theta$-space, together with certain boundary conditions, suffices to determine a balanced state of motion and temperature in the atmosphere. He proved this in the special case of a symmetric vortex (Fig. 3).

The assumption of balance is necessary in this important theorem because gravity waves do not possess potential vorticity since their vorticity vector is tangential to the isentropic surfaces. Thus any set of gravity waves, or more generally, Poincare waves, may be added without changing the field of potential vorticity. Therefore, to exclude the possibility of an arbitrary field of gravity waves, some kind of balance condition must be imposed.

Suppose we choose a simple geostrophic balance. Then, in pressure coordinates, the geostrophic potential vorticity Q_g can be expressed in terms of the geopotential ϕ:

$$\frac{1}{g}Q_g = [f + \nabla_p \cdot (\frac{1}{f}\nabla_p\phi)]\frac{\partial}{\partial p}(\Gamma\frac{\partial\phi}{\partial p}) - \frac{\Gamma}{f}(\nabla_p\frac{\partial\phi}{\partial p})^2$$

where $\theta = \Gamma(p)\alpha$,

$$\Gamma(p) = \frac{p_r}{R}(\frac{p}{p_r})^{c_v/c_p}$$

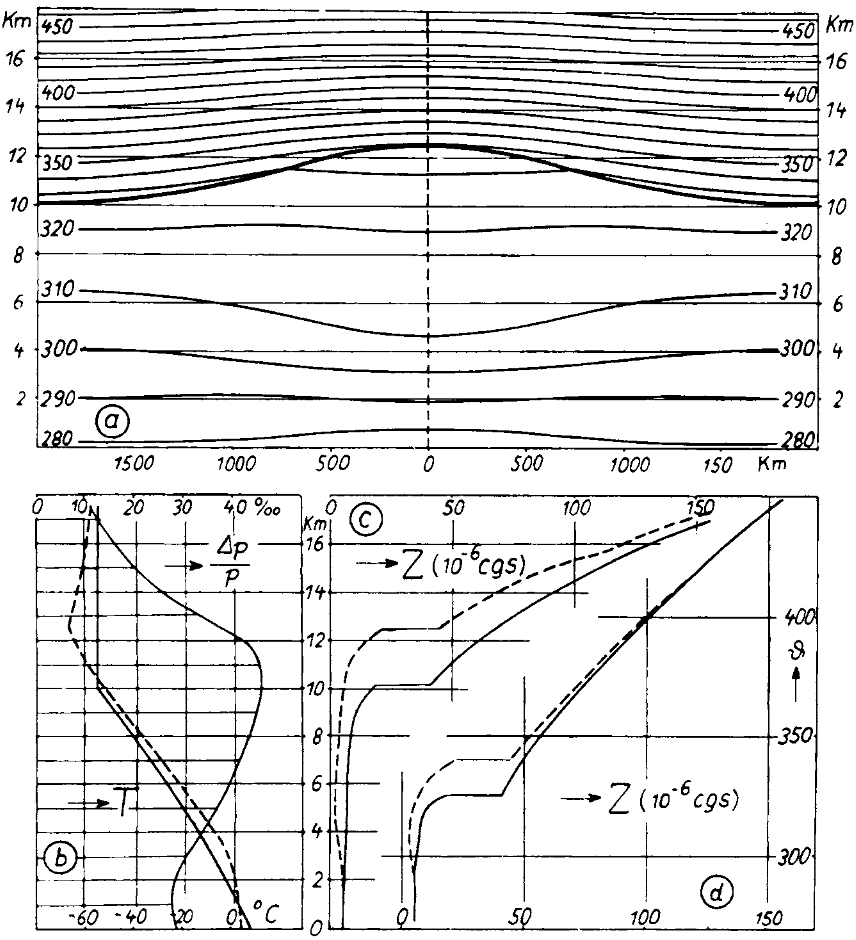

Figure 3. *a: Schematic vertical section through a warm anticyclonic vortex, with potential isotherms. b: Central pressure increment, central temperature and ambient temperature, as functions of height. c: Central (dotted) and ambient (solid) potential vorticity (Z) as functions of potential temperature. From Kleinschmidt (1950a).*

The baroclinic contribution to Q, represented by the last term, is seen always to be negative. If the distribution of Q_g is known in x,y,p-space, then the above expression is a differential equation of second order with ϕ as the sole unknown. However, being of second degree, it is a very unpleasant equation.

This difficulty was noticed by Charney and Phillips (1953), who recommended instead to introduce the geostrophic balance in the expression for Q in isentropic coordinates:

$$\frac{1}{g}Q_g = [f + \nabla_\theta \cdot (\frac{1}{f}\nabla_\theta M)](-\Gamma\frac{\partial^2 M}{\partial\theta^2})$$

or, after rearranging,

$$\nabla_\theta \cdot (\frac{1}{f}\nabla_\theta M) + \frac{\Gamma}{g}Q_g\frac{\partial^2 M}{\partial\theta^2} = -f$$

If Q is specified as a function of x, y, θ, this is almost a linear Poisson-type equation in the Montgomery potential $M(x, y, \theta)$, and it is elliptic when $fQ_g > 0$. It is a complication that Γ is a function of $\partial M/\partial p$, but the solution can be obtained by numerical methods, provided the distribution of θ at the earth's surface is also known. Charney and Phillips proposed in 1953 a NWP method based on the solution of this equation at every time step and a step-wise geostrophic advection of Q_g in isentropic surfaces. The method was not tested until 20 years later when R. Bleck (1973, 1974) made several integrations using this method. The results were not too good, presumably because of the geostrophic errors.

Charney and Stern (1962) proposed another way of avoiding the difficulty presented by the geostrophic potential vorticity equation in pressure coordinates. Under certain simplifying assumptions, they found that the quantity

$$Q_p = f + \frac{1}{f_0}[\nabla^2\psi + \frac{1}{\varrho_0}(\frac{f_0^2}{N_0^2}\varrho_0\frac{\partial\psi}{\partial z})]$$

which Charney called "pseudopotential vorticity" is conserved in the horizontal nondivergent motion defined by the streamfunction ψ.

From this remarkably simple theory of large-scale motions, Charney and Stern (1962) derived a fundamental theorem concerning the stability of baroclinic currents with constant potential temperature along the ground. They found that in order for such a current to be unstable to wave disturbances, it is necessary that the meridional gradient of potential vorticity along isentropic surfaces does not have the same sign everywhere in the meridional plane.

Charney-Stern's theory depended on a number of simplifying approximations: neglect of the baroclinic part of the potential vorticity, setting f constant except in the β-term, and replacement of density and static stability by standard functions of altitude. However, as shown by the author (1983), their theorem can be proved from the "exact" potential vorticity theorem and the geostrophic approximation, without invoking all the other simplifying approximations inherent in Charney-Stern's quasi-geostrophic theory.

The restriction to isentropic ground in the Charney-Stern theorem was removed by Pedlosky (1964), but Bretherton (1966) pointed out that all currents can formally be made isentropic along the ground by adding an infinitely thin, fictitious layer of infinite static stability along the ground. With this trick, Charney and Stern's formulation of the theorem is quite general.

As examples, we consider the Eady model, i.e. a baroclinic Boussinesq fluid of constant potential vorticity between two parallel horizontal planes; and the Charney model, a baroclinic atmosphere of infinite height over a level ground surface (Fig. 4). Adding Bretherton's spurious surface layer of infinite potential vorticity (in Eady's case also at the top surface), the Charney-Stern necessary criterion of instability is seen to be fulfilled in both these cases.

As a consequence of the Charney-Stern theorem, the distribution of potential vorticity in relation to the isentropic surfaces is of great importance. Fig. 5 shows the

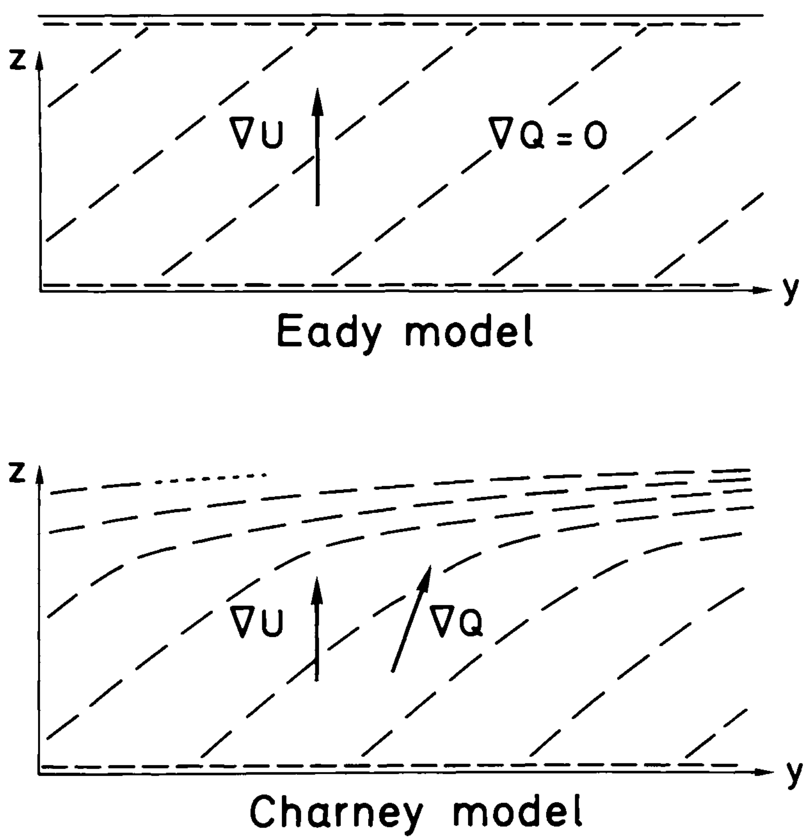

Figure 4. *Meridional cross-sections through Eady's and Charney's zonal flow models. Dashed lines are lines of constant potential temperature.*

distribution of these quantities in a cross-section normal to a jet-stream frontal system, as analysed by M.A. Shapiro (1976).

Very few integrations of the primitive equations using isentropic coordinates are reported in the literature. Perhaps the first experiment of this kind was made by E. Raustein and myself in 1970 (1968). We used a model with just two isentropic surfaces, one of which intersected the level ground in a cyclic beta-plane channel. Starting from a baroclinic state with straight zonal surface isotherms and a smooth low and high surface pressure system, we obtained after 48 hours the pattern shown in Fig. 6. Here sharp frontal discontinuities had developed in pressure gradient and surface wind, whereas the surface isotherms showed a moderate crowding north of the front - a rather realistic feature.

R. Bleck (1974) has made experimental calculations of prognostic maps from real initial data, using a primitive equation model in isentropic coordinates. It would be

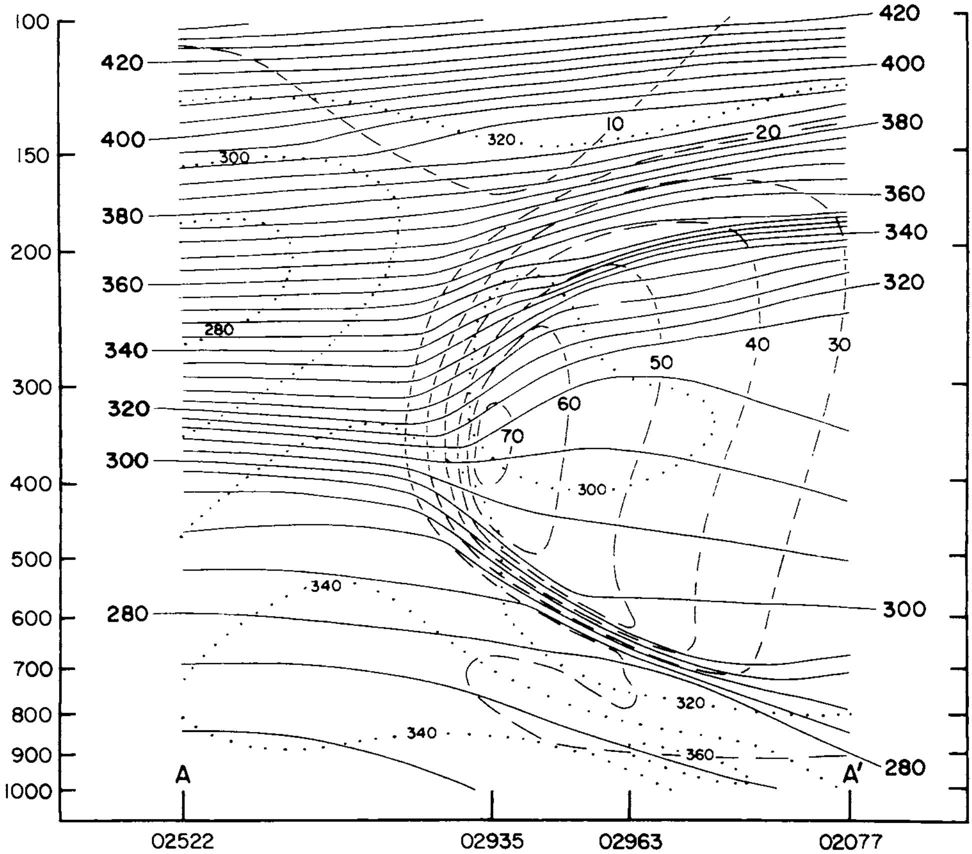

Figure 5a. *Cross-section from the White Sea to Stockholm through a NW-erly jet stream over Scandinavia at 00GMT 9 April 1974. Potential temperature, solid lines; wind speed, dashed lines; wind direction, dotted lines. From Shapiro (1976).*

possible to use models also with diabatic heating included. The main advantage of isentropic surface models would be a reduction of truncation errors, because the vertical advection terms would be very small. Moreover, folding of the isentropic surfaces does not seem to occur. However, there are certain difficulties connected with the use of numerical isentropic surface models: the number of isentropic data surfaces decreases equatorward, and moreover, it is difficult to model satisfactorily the intersection of the isentropic surfaces with the ground. It is therefore not surprising that σ-coordinates are preferred in numerical weather prediction as well as in general circulation models.

On the other hand, isentropic models are very convenient for theoretical studies of certain aspects of atmospheric behavior. It is noteworthy that Edward Lorenz's extremely important concept, available potential energy, (1955) requires isentropic coordinates for its rigorous expression; the formula commonly used in pressure coordinates is just an approximation.

Fig. 7, from a paper by S. Thorsteinsson and the author (1984), shows calculated streamlines in a meso-scale mountain wave over an infinitely long mountain ridge. In the case shown, the incident current velocity decreases with height in a middle layer. The calculation of these flows was much simplified by the use of isentropic coordinates.

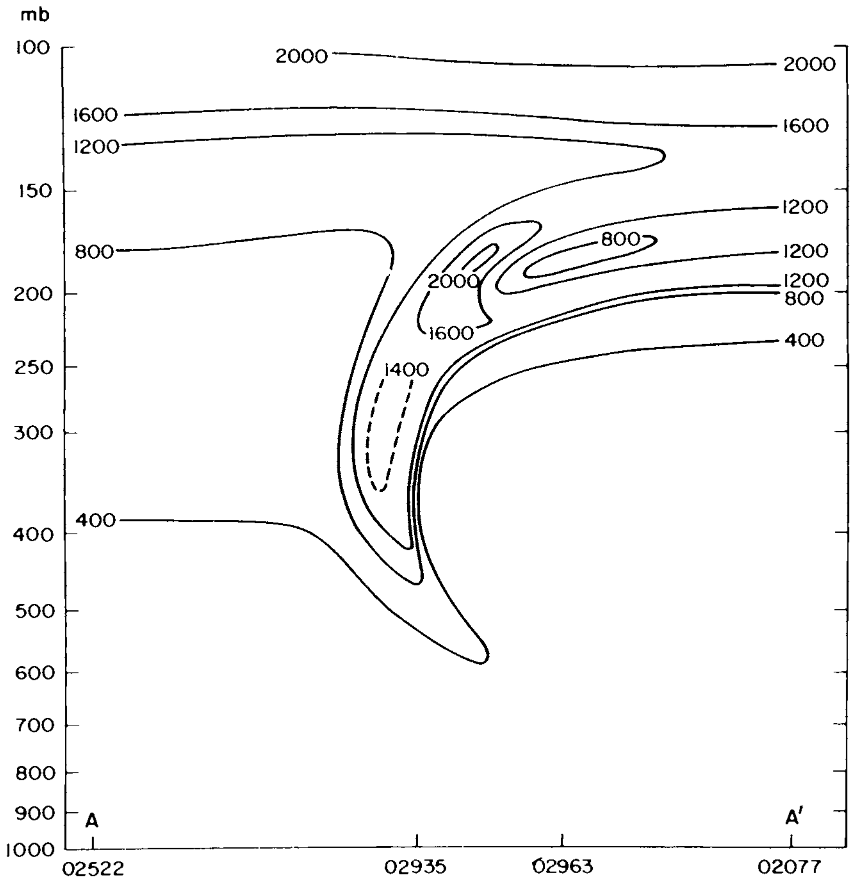

Figure 5b. *Cross-section from the White Sea to Stockholm through a NW-erly jet stream over Scandinavia at 00GMT 9 April 1974. Potential vorticity. From Shapiro (1976).*

Waves in the westerlies will affect the mean zonal flow in two ways: directly by the meridional eddy fluxes of heat and angular momentum, but also indirectly by the meridional circulations caused by the same eddy fluxes. In 1961 Charney and Drazin (1961) showed that under certain conditions these two effects would cancel so that the mean zonal flow would remain unaffected; this is Charney-Drazin's non-acceleration theorem. This effect was thoroughly studied by D. Andrews and M. McIntyre in a series of papers in 1976-78 (1978). They showed that the compensation is complete if the waves are steady and conservative. They also showed that if a Langrangean definition is used for the zonal mean, rather than the usual Eulerian definition, then the eddy fluxes and the compensating meridional circulation disappear from the analysis, and there remains only a weak residual circulation due to diabatic effects.

The Langrangean mean is hard to define synoptically, but the zonal mean taken

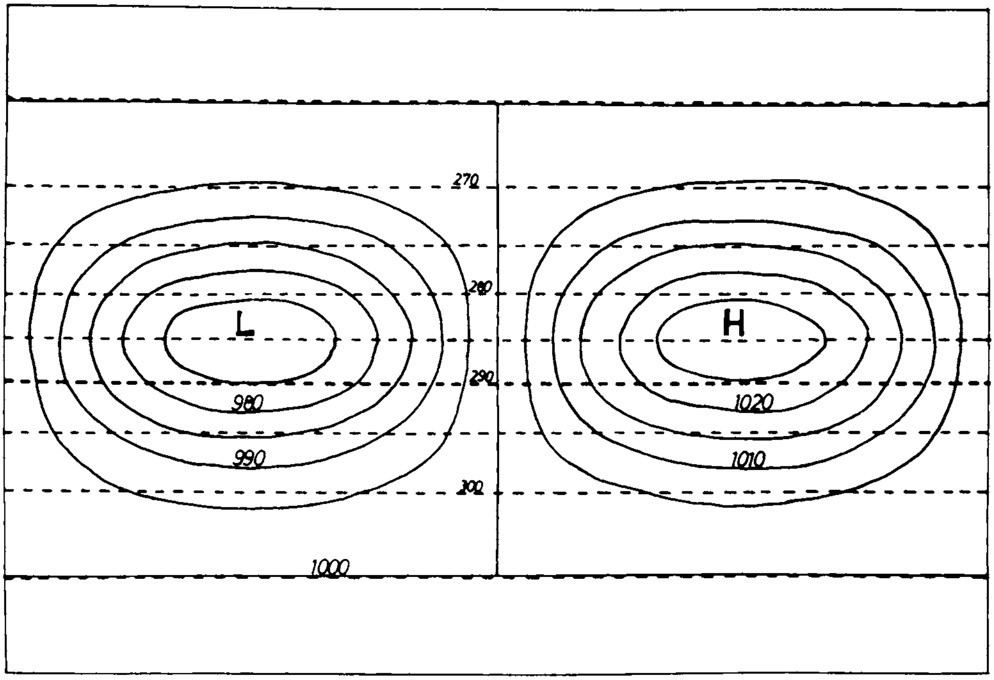

Figure 6a. *Surface pressure (solid) and surface potential temperature. Initial situation. From Eliassen and Raustein (1968).*

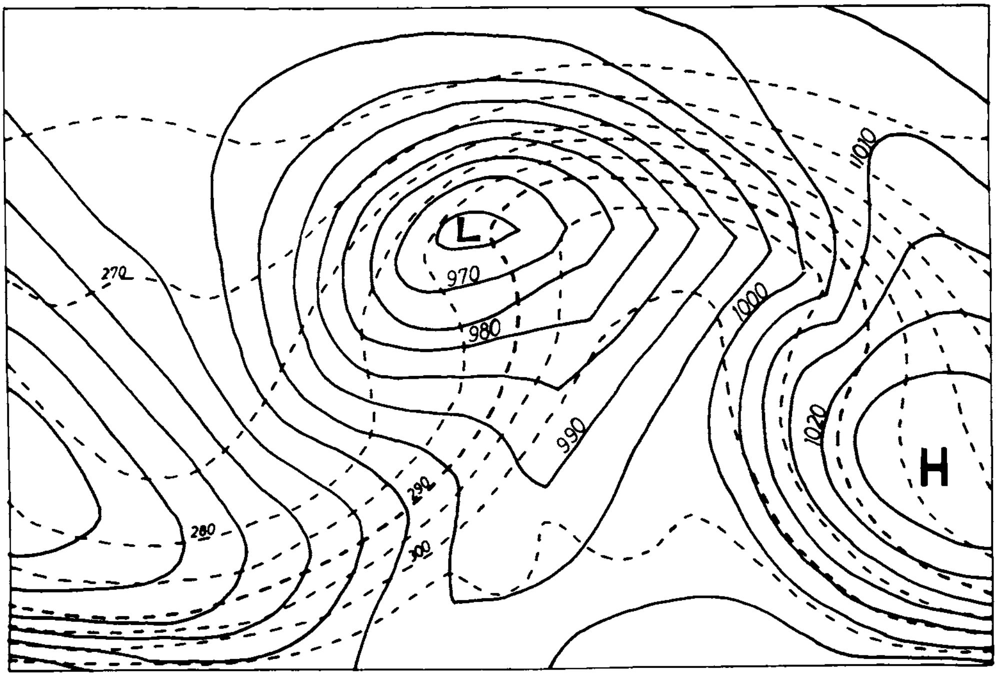

Figure 6b. *Surface pressure (solid) and surface potential temperature. After 48 hours. From Eliassen and Raustein (1968).*

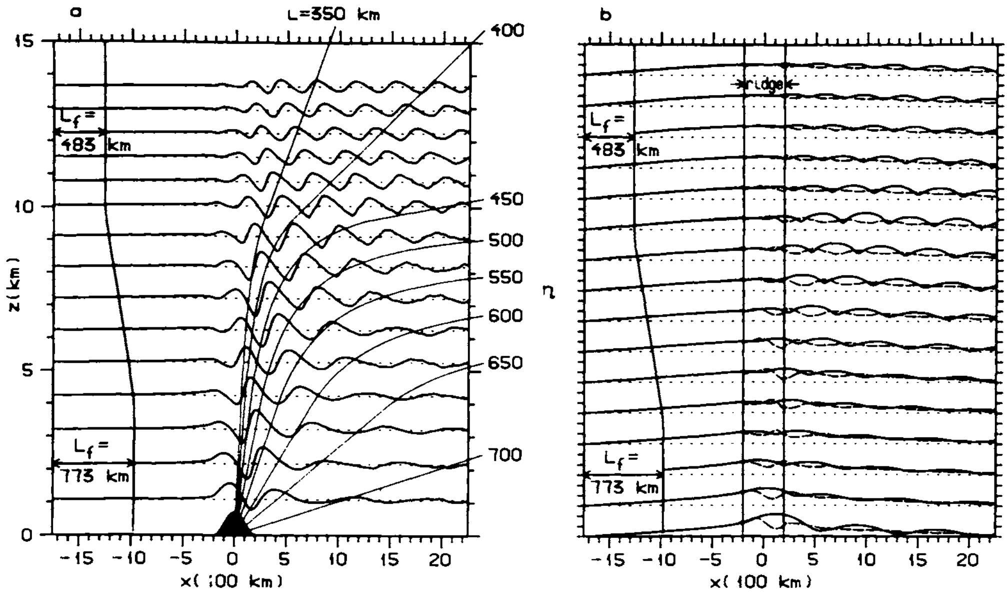

Figure 7. *Left: Streamlines in vertical projection. Thin lines: wave rays from linear theory. Right: Streamlines at various levels in horizontal projection.* $L = 2\pi U/f$ *is the inertia wave length. From Eliassen and Thorsteinsson (1984).*

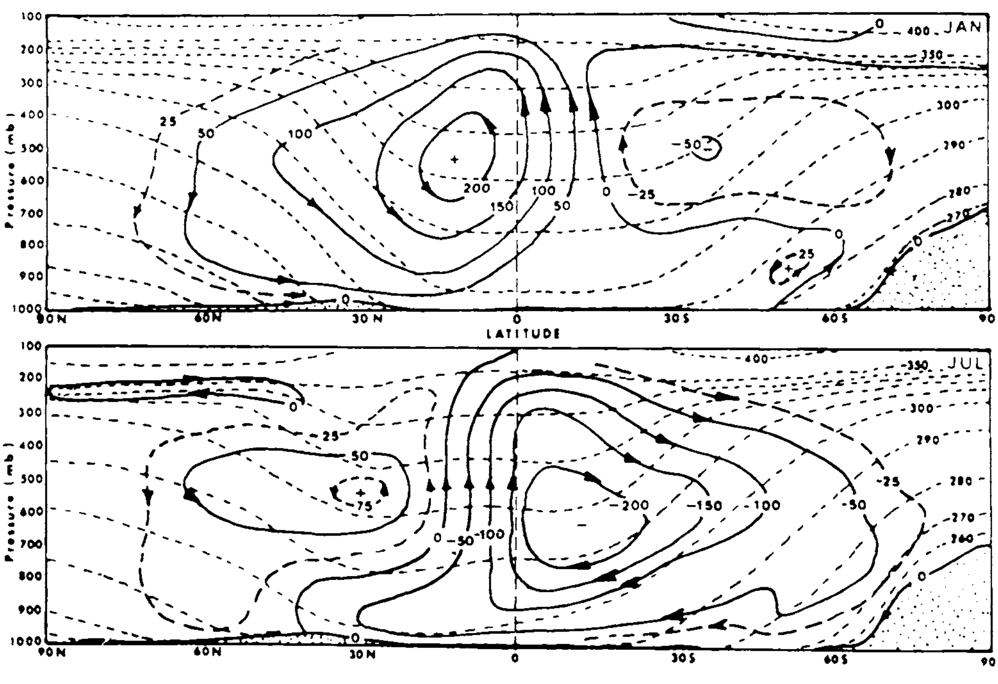

Figure 8. *Streamfunction of the mean meridional mass circulation in isentropic coordinates. Dashed lines: isentropic surfaces. From Gallimore and Johnson (1981).*

along isentropic surfaces has similar properties. If this isentropic zonal mean is used, one obtains a definition of the meridional circulation relative to the isentropic surfaces which is due only to diabatic effects (including condensation heat), and the corresponding eddy fluxes are reduced mainly to those caused by irreversible transient waves.

Gallimore and Johnson (1981) have applied the zonal mean taken in isentropic charts in their studies of the general circulation. Fig. 8 shows their calculated meridional circulation pattern, which consists of just one large direct circulation cell in the winter hemisphere, compared with the conventional Hadley-Ferrel double cell structure obtained from isobaric zonal mean analysis.

These theories have become very important also for the study of meridional transport of chemical tracers in the atmosphere. In recent years, several authors have taken advantage of these developments and based their calculations on the residual circulation and the reduced eddy fluxes, thus avoiding the calculation of the strong Eulerian eddy fluxes and their compensating circulations.

Many other studies ought to have been mentioned, but had to be left out in this short lecture. However, I hope these examples suffice to show that isentropic analysis is as important as ever, and that it has resulted in many interesting new developments. Thus the pioneering work of Jerry Namias 50 years ago continues today to bear valuable fruits.

x, y - horizontal coordinates

z - height

$\mathbf{k}$ - vertical unit vector

g - acceleration of gravity

$\mathbf{\Omega}$ - angular velocity vector of the earth's rotation

f - Coriolis parameter

R - gas constant

c_p, c_v - specific heats

p - pressure, $p_r = 1$ bar

α - specific volume

T - temperature

θ - potential temperature

$\phi = gz$ - geopotential

$M = \phi + c_pT$ - Montgomery potential

$\mathbf{v}$ - velocity

$\mathbf{v}_h$ (u,v) - horizontal velocity

$\frac{\partial}{\partial x}, (\frac{\partial v}{\partial x})_p, (\frac{\partial}{\partial x})_\theta$ - derivative at constant z,p,θ, respectively

$\zeta = \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y}$, - vertical vorticity

$\zeta_p = (\frac{\partial v}{\partial x})_p, (\frac{\partial u}{\partial y})_p$ -"isobaric" vorticity

$\zeta_\theta = (\frac{\partial v}{\partial x})_\theta - (\frac{\partial u}{\partial y})_\theta$-"isentropic" vorticity

∇_p, ∇_θ - horizontal gradient in isobaric, isentropic map, respectively

Q - potential vorticity, Q_g - geostrophic value

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Residence Times and Other Time-Scales Associated with Norwegian Air Mass Ideas

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Introduction

Norwegian ideas about air masses have had a more checkered career than those concerned with the polar front and fronts in general. Compared to great emphasis on air masses in the 1920s and 1930s, when J. Namias was instrumental in introducing knowledge of them to the United States (Namias, 1940), they are now comparatively seldom mentioned or written about. One excellent recent text (Wallace and Hobbs, 1977) almost avoids mentioning the words 'air mass' at all. Nevertheless, the underlying ideas remain valid though with some modification of earlier expectations.

In the basic source papers (Bjerknes 1919, Bjerknes and Selberg 1922) and especially Bergeron (1928) there is clear attention to the time scales involved in the formation and transformation of air masses. Thus, Bergeron carefully distinguishes formation times in source regions, exchange times across middle latitudes, and transformation times but without stating any specific ranges for these times. All these ideas are basically Lagrangian, and concern particular material regions, respecting which direct observations are very sparse and difficult. Air mass lifetimes must be limited by the ultimate eddy mixing that produces constancy of the main dry air composition to about 1 per mille (Glueckauf, 1951) all the way to 80 km altitude, but the corresponding upper limit to lifetimes could be rather long. I am not aware of any attempt to estimate such a bound.

In later standard meteorological sources only qualitative estimates of a few days to a week or so for air mass lifetimes in middle latitudes and similar estimates for formation times are given (Willett, 1933). Petterssen (1940) gives formation times of polar continental air as several days to as short as two days. Godske *et al.* (1957) state "we wish our air-masses to be so defined that their "lifetime" is of the order of a week."

The transformation processes involved in converting polar air ultimately to tropical air have been the subject of extensive recent studies devoted to air-sea interactions and the effect of various small-scale processes on the numerical modelling of the larger scale flows. Thus, major field experiments have been laid on such as the Air Mass Transformation Experiment (AMTEX) to study processes transforming Siberian air over the ocean around Japan, the Barbados Oceanographic and Meteorological Experiment (BOMEX) to study processes in near-equilibrium trade wind air from the Atlantic, and the GARP Atlantic Tropical Experiment (GATE) to study convective processes over the ocean off West Africa.

By comparison, the reverse processes of transformation of tropical and maritime polar to continental polar or arctic air have received less attention. The structure of air over the polar basin was investigated by Sverdrup (1925) during the Maud expedition with a conclusion that the cold layers were only a few hundred meters deep with substantial inversions above in which temperatures, however, were well below those of typical maritime air. The first attempts to estimate the transformation process from maritime polar to continental polar or arctic were those of Wexler (1936, 1937) who applied a simple clear air infrared radiative cooling model without explicit turbulent transfer. He concluded that reasonable soundings with inversions and cooling to about 3 km could be produced from a typical maritime polar sounding in about 26 days or so. The problem then lay dormant until taken up by a student J. Curry of Prof. H.L. Kuo who applied a much more elaborate radiative model including turbulent and large-scale subsidence effects and concluded that condensation, particle precipitation, and ice crystal radiative effects were crucial in producing sufficient net cooling. The net results were to produce reasonable soundings to 2 or 3 km within about 2 weeks, slightly less than Wexler's time (Curry, 1982; Curry, 1983). Since both these estimates seemed on the long side, for example against Petterssen's statement, it occurred to the present author that one might at least see whether, on the average, sufficient time was available for air mass transformation by determining the average time spent by the air itself in suitable Eulerian regions. Thus, in a long time average, the mass flux in or out across the 70th North parallel divided into the mass of air in the cap north of 70°will give a residence time for the Arctic basin which must at least put some constraint on any theory of formation rates for Arctic air.

With the amount of free air data now available, reasonable estimates can be made of such residence times for a variety of regions and pressure levels and further are relevant to a wide variety of exchange and geochemical budget problems which we will touch on below. I have found only one comparable specific time estimate in the literature and that is the estimate by Palmén and Newton (1951, Newton, 1970) of a time of 15 to 20 days, for the cap north of 45°. This was obtained by extrapolating through multiplication by 5 of the mass flux measured in the storm of 5 April 1950 over North America, and, though a fairly intense storm, we will see the time is substantially too long. The other area where a substantial amount of work has been done is that of interhemispheric exchange times across the equator. The results vary quite widely and will be discussed later in detail.

Mass flux calculations

The simplest case to think about is that of the total amount of air in the cap poleward of a given latitude ϕ. Even for relatively short time intervals, the mean surface pressure, P_s, over such a cap of any size changes so little that the total mass (determined exactly by mean P_s) is invariant and consequently the net mass flux across a wall at latitude ϕ is zero:

$$\int_0^{2\pi}\int_0^{\infty} \rho v dz(a cos\phi d\lambda) = \int_0^{2\pi}\int_0^{P_s} v\frac{dp}{g}(a cos\phi d\lambda) = 0 \tag{1}$$

where v is the meridional velocity component, ρ the density, g the acceleration of gravity, a the radius of the earth, and λ the longitude. If we separate the contributions V_+ and V_- to northward and southward fluxes, and let each be considered positive, then flux in or northward equals flux out or southward. If we let <> denote an average over the entire lateral surface at latitude ϕ and — a suitably long time average, then the mean fluxes can be written:

$$\overline{< P_s g^{-1}|v|_+ >} L_+ = \overline{< P_s g^{-1}|v|_- >} L_- \qquad (2)$$

where L_+ and L_- are the mean perimeter lengths over which v is respectively northward or southward. The mean absolute deviation of v over the domain is then in this notation

$$< \overline{|V - < \bar{|V|} >|} >= \frac{< \overline{|V|_+} > L_+ + < \overline{|V|_-} > L_-}{L_+ + L_-} \qquad (3)$$

This neglects in (2) the possibility of fluctuation covariances in either time or longitude between surface pressure P_s and vertical mean V's as discussed by Rosen (1976). We will ignore this possibility.

(1) shows that it is the mean absolute deviation of V in the statistical sense over the total space and time sample that determines the flux which is the physically significant property for our purposes (a somewhat unusual result). If the region considered is limited in the vertical by two pressure surfaces (which is the way most presently observed data are organized), then the argument is slightly different. The total mass in the region is now constant by construction (except where there is intersection with ground topography) and (2), with Δp the pressure difference between the two surfaces, becomes:

$$\Delta p/g < \overline{|V|_+} > L_+ - \Delta p/g < \overline{|V|_- >} L_- + verticalfluxin - verticalfluxout = 0 \quad (4)$$

Depending on circumstances, the vertical fluxes may largely cancel or be small relatively and then the lateral fluxes are again equal in absolute value. Or for a limited region, the lateral fluxes may not cancel. The difference is the net vertical flux and whichever is the larger of the lateral fluxes will be appropriate for calculating an upper bound residence time that only neglects the vertical flux parts that cancel. We will, for most of the later discussion consider only lateral fluxes and treat them as balancing each other.

If we denote the mean absolute deviation of V for the sample by $\hat{V}$ then either the flux in or flux out can be calculated from

$$Flux = \hat{V}(\pi a cos\phi)\frac{\Delta p}{g} \qquad (5)$$

using the half-circumference of the latitude circle. One can drop $\Delta p/g = (10.2kg/m^2mb)\Delta p$ and simply measure the fluxes in units of area occupied per unit time. Cap residence times for whatever standard layer is considered are then simply cap area divided by flux in area per unit time. If several layers are involved, the fluxes in area units can simply be weighted by appropriate Δp fractions. It is convenient to measure areas in units of $10^{12}m^2 = 10^6 km^2$ and fluxes in units of $10^6 km^2$/day. Cap areas are then

$$A_c(\phi) = 2\pi a^2(1 - sin\phi) = 255.05(1 - sin\phi)(10^6 km^2) \qquad (6)$$

and fluxes, if $\hat{V}$ is in m/s, are

$$Q_+(\phi) = \hat{V}(\pi a cos\phi) = 1.729_3\hat{V}(m/s)cos\phi(10^6 km^2/day) \tag{7}$$

The cap residence time then is

$$t_{res} = \frac{A_c}{Q_+} = \frac{147.5}{\hat{V}(m/s)}(\frac{1-sin\phi}{cos\phi})(days) \tag{8}$$

All these formulae are appropriate when the basic data are available as meridional velocities V if one can obtain an appropriate estimate of the mean absolute deviation $\hat{V}$ for whatever sample is involved. We will see below that such an estimate can be made from the circulation data sets available.

The other principal alternative occurs if the velocities are evaluated geostrophically for then, on a latitude circle, the net flux is identically zero. The flux integral is:

$$Q = \int_{P_1}^{P_2} \oint gs/f\hat{K}x\nabla z_g \cdot \hat{n}ds \cdot \frac{dp}{g} = \int_{P_1}^{P_2} \oint g\hat{s}/f\frac{\partial z_g}{\partial s}ds\frac{dp}{g} = \int \oint g\hat{s}/f dz_g \cdot \frac{dp}{g} = 0 \tag{9}$$

where g_s is standard gravity, f is the coriolis parameter, z_g is the geopotential height of the pressure surfaces, $\hat{k}$ and $\hat{n}$ are appropriate unit vectors, and s is a curvilinear running coordinate around the boundary curve (latitude circle in our cap case). The geostrophic fluxes north or south are simply proportional respectively to the cumulative sum of positive or negative z_g differences along the latitude circle measured in geopotential meters.

$$Q_+ = g_s/f\sum(\Delta z_{g+}) = \frac{.005806}{sin\phi}\sum(\Delta z_{g+})\Big(\frac{10^6 km^2}{day}\Big) \tag{10}$$

in area units again. The calculation from cumulative sums of differences is precisely the same as the procedure used by Willett *et al.* (1940, p. 33) to calculate geostrophically some of the meridional circulation indices that they obtained for surface and 10,000 foot Northern Hemisphere data. An extract from some of Willett's later results will be given below.

The most extensive published circulation data sets are those of Oort and Rasmusson (1971) based on 1958 to 1963 Northern Hemisphere data and of Oort (1982) based on 1963 to 1973 data for both hemispheres. Extensive statistics are given for the variance of V separated in the standard way into contributions from the transient and standing eddies relative to zonal means. The annual mean total variances of V are given in Fig. 1 both for the vertical mean and the 300 mb level at which the maximum values usually occur. Typical values range from 20 or 30 m^2/s^2 around the equator to around $100m^2/s^2$ in the midlatitude maxima (vertical mean) and to over $260m^2/s^2$ in both hemispheres at the 300 mb maxima. If the statistical distribution of the meridional velocities V for any time and space sample is Gaussian, then according to the Cornu criterion, Brooks and Carruthers (1953):

$$\frac{\hat{V}}{\sigma_v} = (\frac{2}{\pi})^{1/2} = .7979 \tag{11}$$

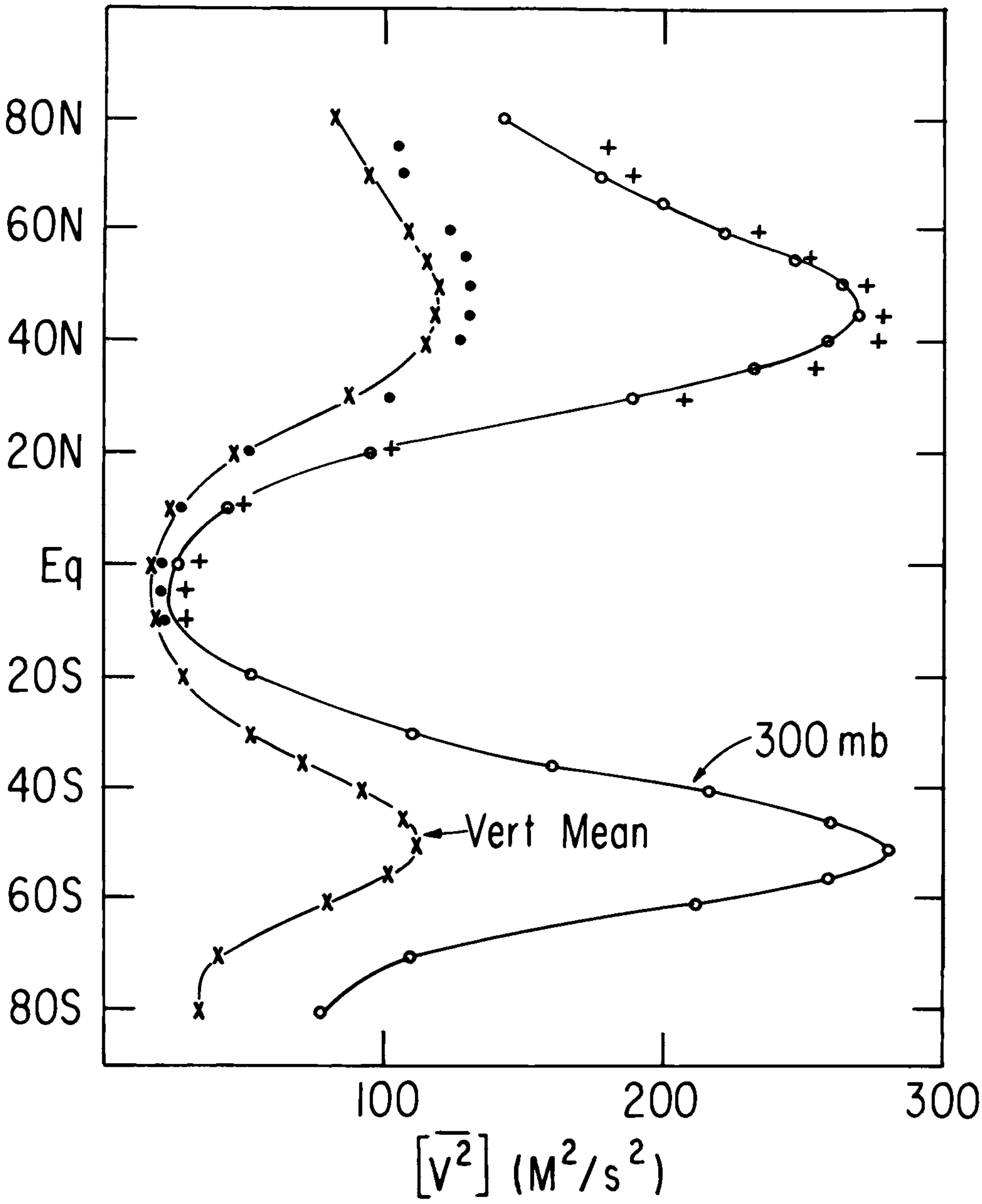

Figure 1. *Annual mean total variances of meridional velocity against latitudes for x, vertical mean in m^2/s^2, 25-1012.5 mb, and ∘, at 300 mb from Oort (1982). Data from 1963 to 1973. Additional points are •, vertical mean, and +, 300 mb data for 1958 to 1963 from Oort and Rasmusson (1971).*

L. Donner and R. Jenne of NCAR have very kindly run a test for me on observed data from 3 years of 3 winter months (DJF) and 3 years of 3 summer months (JJA) at 3 pressure levels and 5 north latitudes. The winter results are shown in Figure 2 (summer results differ only slightly). It is seen that the 9 month sample ratios of $\hat{V}/\sigma_v$ differ by only a few percent from the Cornu value and I conclude we can safely estimate $\hat{V}$ from the eddy variances. The variances in Fig. 1 then lead to mean absolute deviations of anywhere from 4 m/s around the equator to 8 m/s in the mid-latitude maxima and as high as 13 m/s at 300 mb.

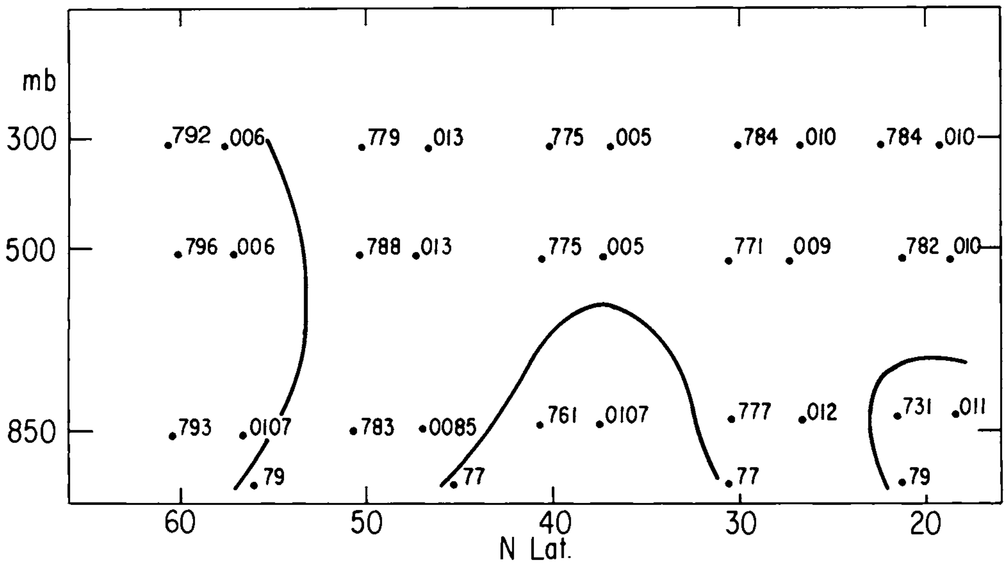

Figure 2. *Ratios of mean absolute deviation of V to the total eddy standard deviation of V for nine monthly samples from December, January and February of 1978-9, 1979-80, and 1980-81 at levels of 850, 500 and 300 mb and 20, 30, 40, 50, and 60 N latitude. Left figure is the mean ratio and the right figure the standard deviation of the ratio for each sample of nine months.*

It is obviously of interest to consider other regions appropriate for such calculations such as the zones between two latitude circles or figures bounded by portions of two latitude circles and two meridians since Oort (1982) gives maps containing most of the required data. For zones, one merely adds the respective unsigned fluxes for the two latitudes since each will have an inward and outward contribution. The case of a general quadrilateral is a little more complicated but need not be gone into in detail as a few comments should suffice. The appropriate modification of (1) to a sum of integrals of normal velocity components on each section of the boundary is required but more contributions are left after time and space averages are taken. Thus, on meridional boundaries in westerly regions, the flux due to the normal component u will be, after time averaging, usually inward on the left boundary and outward on the right boundary and also not necessarily equal. In addition, on the other 2 boundary sections, fluxes from time mean v's will not vanish the way they do on a complete

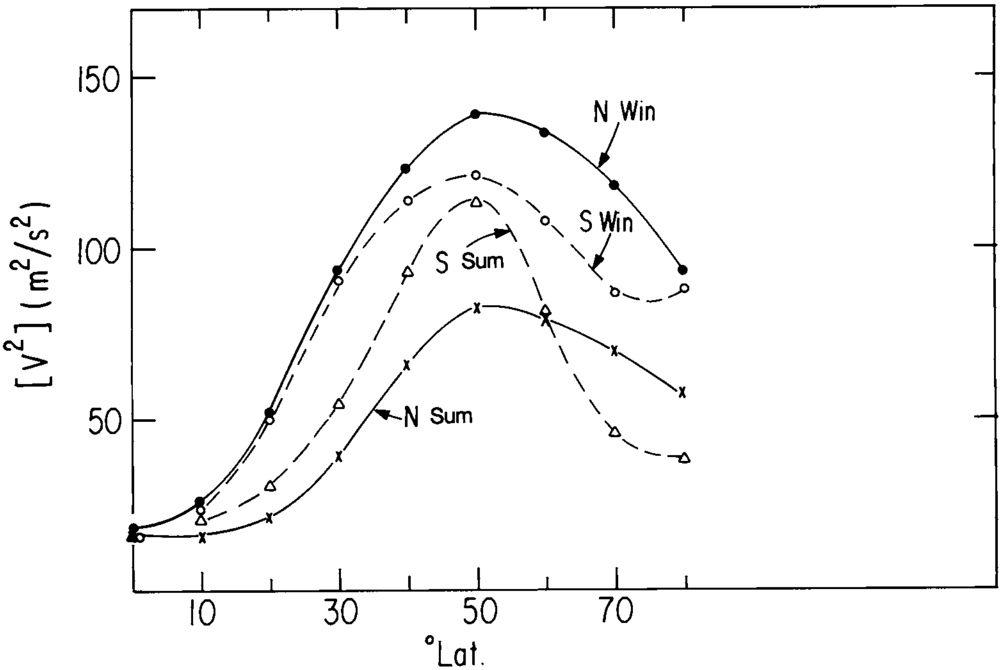

Figure 3. *Vertical mean (25-1012.5 mb) seasonal total eddy variances of V in m^2/s^2 against latitude for Northern and Southern hemisphere from Oort (1982). Data from 1963-1973. a) • Northern Hemisphere DJF. b) x Northern Hemisphere JJA. c) ○ Southern Hemisphere JJA. d) Δ Southern Hemisphere DJF.*

latitude circle lateral boundary. For reasonable sized regions, the constraints discussed above apply to the sum of fluxes over all four boundary sections.

We will give some data in the next sections on limited regions such as Siberia and North America, but this cannot be done as exactly from Oort's statistics as for caps and zones bounded by full latitude circles. The reason is that the time-averaged normal flows are not sufficient on the lateral boundaries but, depending on the actual eddy scales compared to the lengths of the boundaries, the eddy normal velocities will contribute in different proportions to the total inward or outward fluxes. If there were no changes of sign of the normal velocities the time averages would contribute the total fluxes. An illustrative example is, for an east-west boundary if V varies sinusoidally (say in neutral travelling waves) with a wavelength equal to the length of the boundary, then the time and face-averaged V will be identically zero while there is a steady flux in or out associated with each half-wave-length of the sinusoid.

For geostrophic calculation of the fluxes for limited regions, (9) and (10) apply directly as long as the variation of f can be neglected. If the region is too large, one can put f^{-1} to right of the summation and apply some mean value for f in $\Delta z_{g+}/f$ but presumably an over elaborate procedure would not be worthwhile.

Cap and zone fluxes and residence times

The total eddy variances of V averaged zonally, in time, and vertically are shown by season for the two hemispheres in Figure 3. There is surprisingly little difference in the two winter curves with the southern mid-latitude maximum variance only 20

m^2/s^2 below the northern value of 140 m^2/s^2. The summer curves differ in about the expected ways except for the unusually small southern variances at latitudes 70 and 80 S. Curves of the corresponding N or S fluxes are given in Fig. 4. These strongly resemble one another because of the square root operation and the strong weighting in higher latitudes by the diminishing half-circumferences. In all of these vertically averaged variances, the standing eddy contributions are 10% or less of those from the transient eddies (except for reaching above 20% at 60 and 70 N in winter DJF) so that the flux and residence time values would not change much even if the standing eddies were completely ignored.

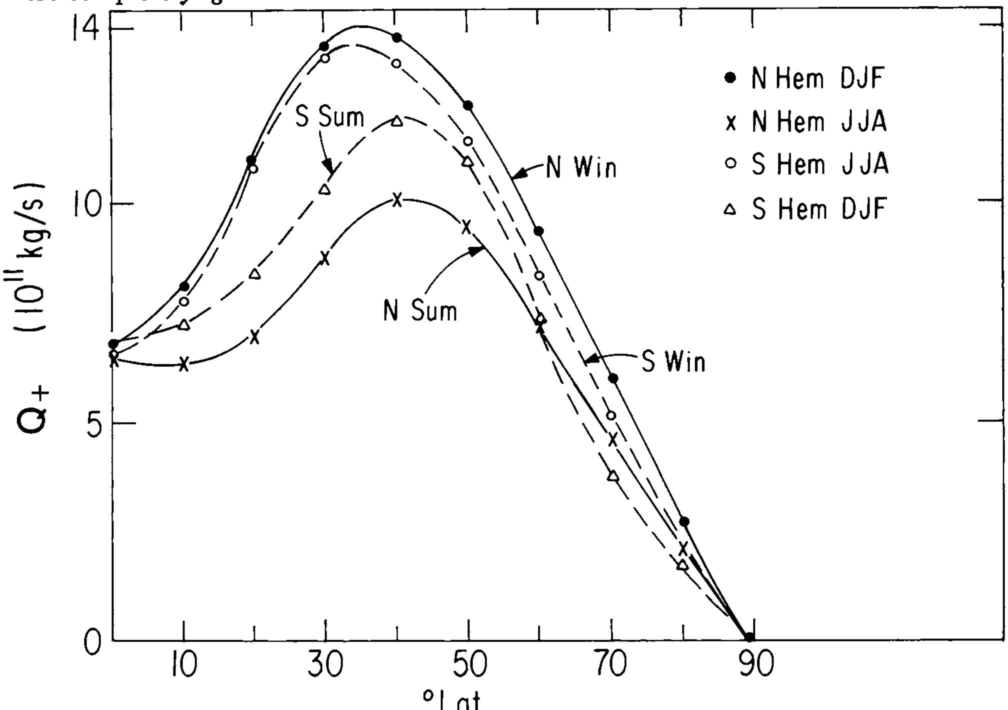

Figure 4. *Mean average positive (or negative) fluxes in Q+ units of* $10^{11} kg/s$ *across latitude circles as functions of latitude for Northern and Southern hemisphere seasons. Calculated from total eddy standard deviations multiplied by the Cornu ratio 0.7979. Other data as in Fig. 3.*

Figures 5 to 8 give cap and zone residence time curves calculated from fluxes similar to Fig. 4 for 3 different intervals: 1012.5 to 600 mb in the lower troposphere, the vertical mean values (Fig. 4) for 1012.5 to 25 mb, and an upper troposphere, lower stratosphere layer from 600 to 150 mb. Fig. 5 gives the results for Northern hemisphere winter (DJF) for these three intervals together with entries giving the time difference (Southern winter - Northern winter) for each latitude and layer. It is striking that these differences are so small nearly everywhere that if shown graphically the curves would barely differ visually. There are a number of striking features that we will have to discuss further in later sections. First, on the polar air question that was the original motivation, the cap times north of 70° are only 3 days in the vertical mean and only 4 1/2 days for the low troposphere layer. The Wexler and Curry formation time estimates of about 26 and 15 days would clearly face a severe logistical problem with average winter arctic air. There are at least several possible solutions to the

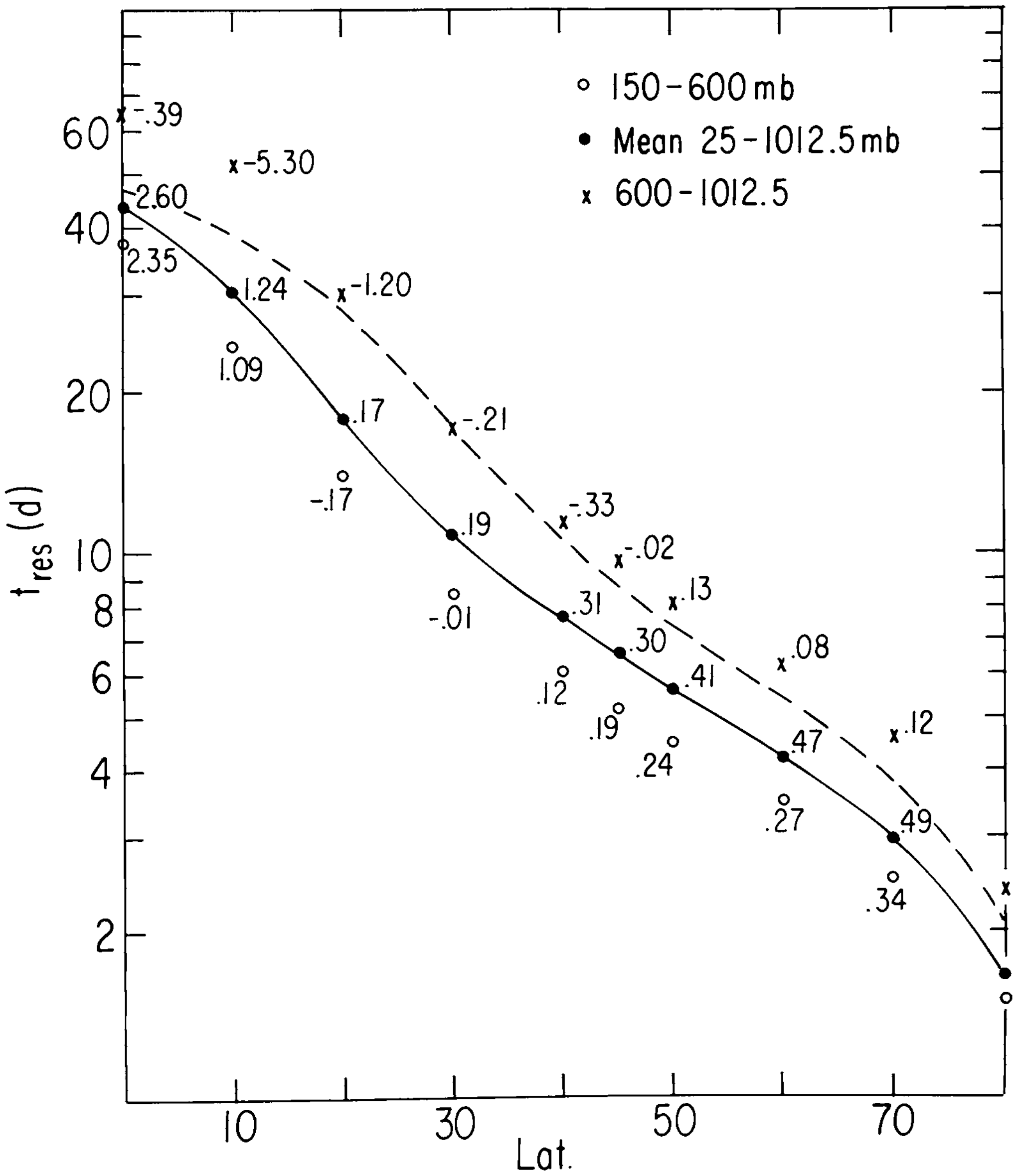

Figure 5. *Cap residence times in days plotted against latitude for Northern Hemisphere winter DJF.* •, *vertical mean 25-1012.5 mb. x, low troposphere 600-1012.5 mb.* ○, *upper troposphere 150-600 mb. Entries are the winter differences in times between south and north, i.e.* $t_S(JJA)$-$t_N(DJF)$. *The dashed curve is the summer curve from Fig. 6.*

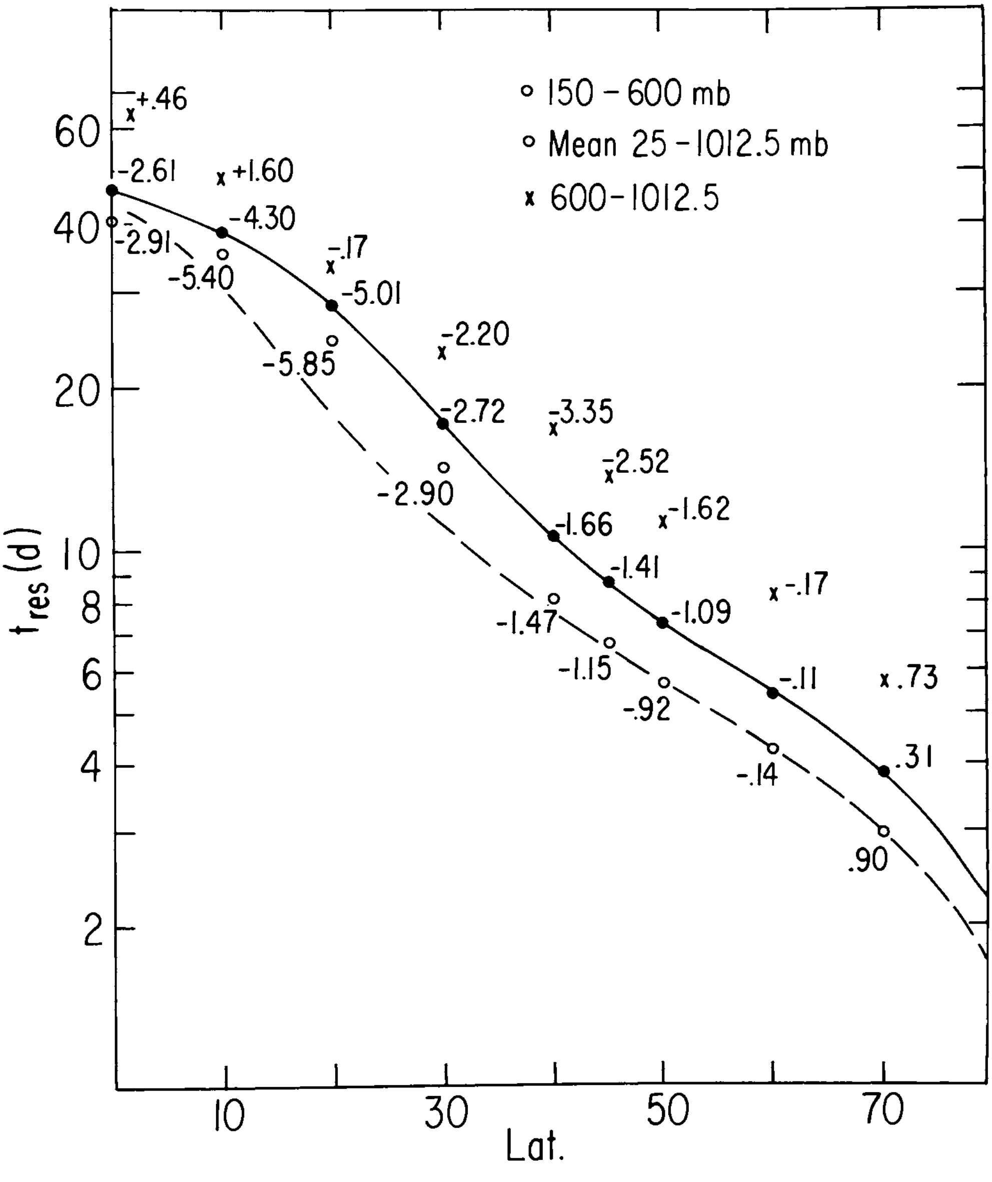

Figure 6. *Same as Figure 5 except for Northern Hemisphere summer JJA and the dashed curve is now the winter curve from Fig. 5.*

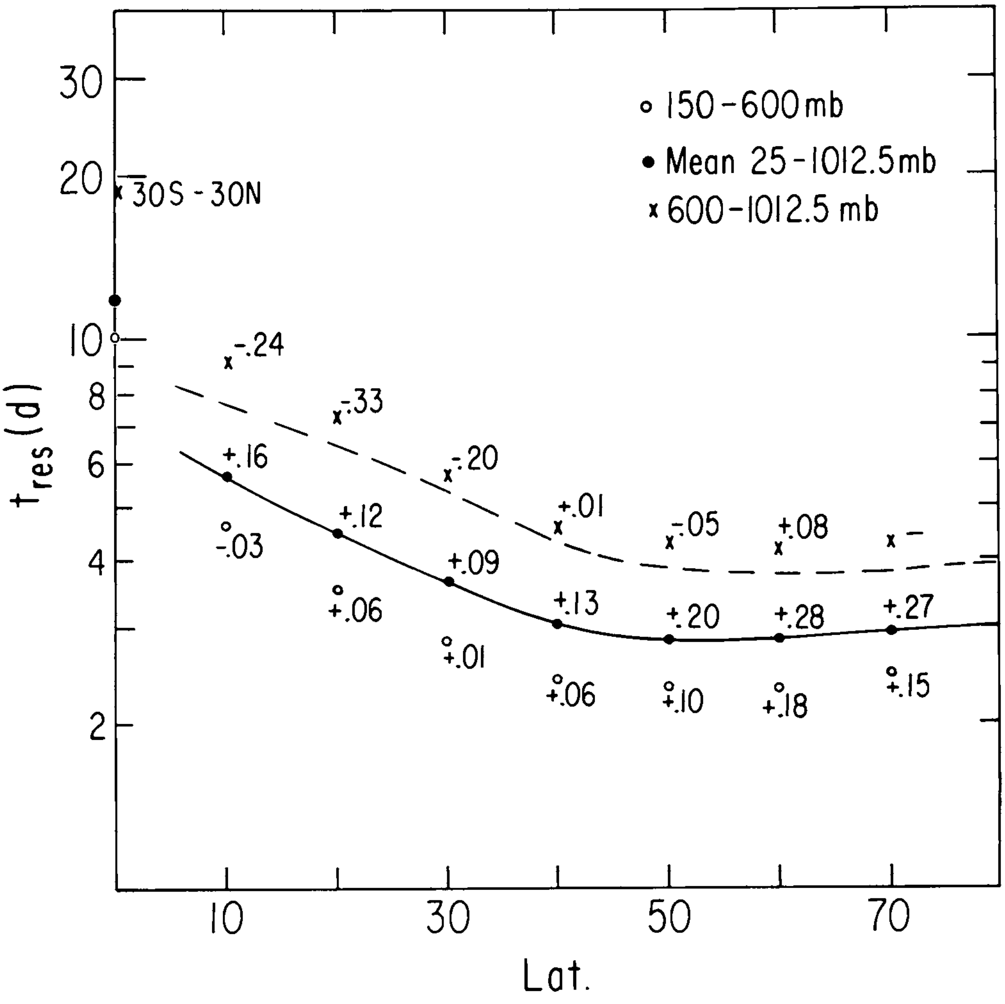

Figure 7. *Zone residence times in days plotted against latitude for Northern Hemisphere winter DJF. Zones are 20° wide about the central plotted latitude except at the equator where the zone from 30° S to 30° N is plotted. Entries are the winter differences in times between south and north i.e.* $t_S(JJA) - t_N(DJF)$. *Solid curve is drawn for the vertical mean data and the dashed curve is the summer curve from Fig. 8. Other data as in Fig. 5.*

dilemma that might be offered and that we will discuss later but it will certainly take further research to decide among them. In middle and lower latitudes the times are unexpectedly short. At 30°, the vertical mean cap time is only 11 days and even the low troposphere value is only 17 days. Similarly, the 45°times of 5 to 10 days are well below Palmén and Newton's (1951) extrapolated estimate of about 20 days. At the equator, the results are similar with a vertical mean exchange or residence time

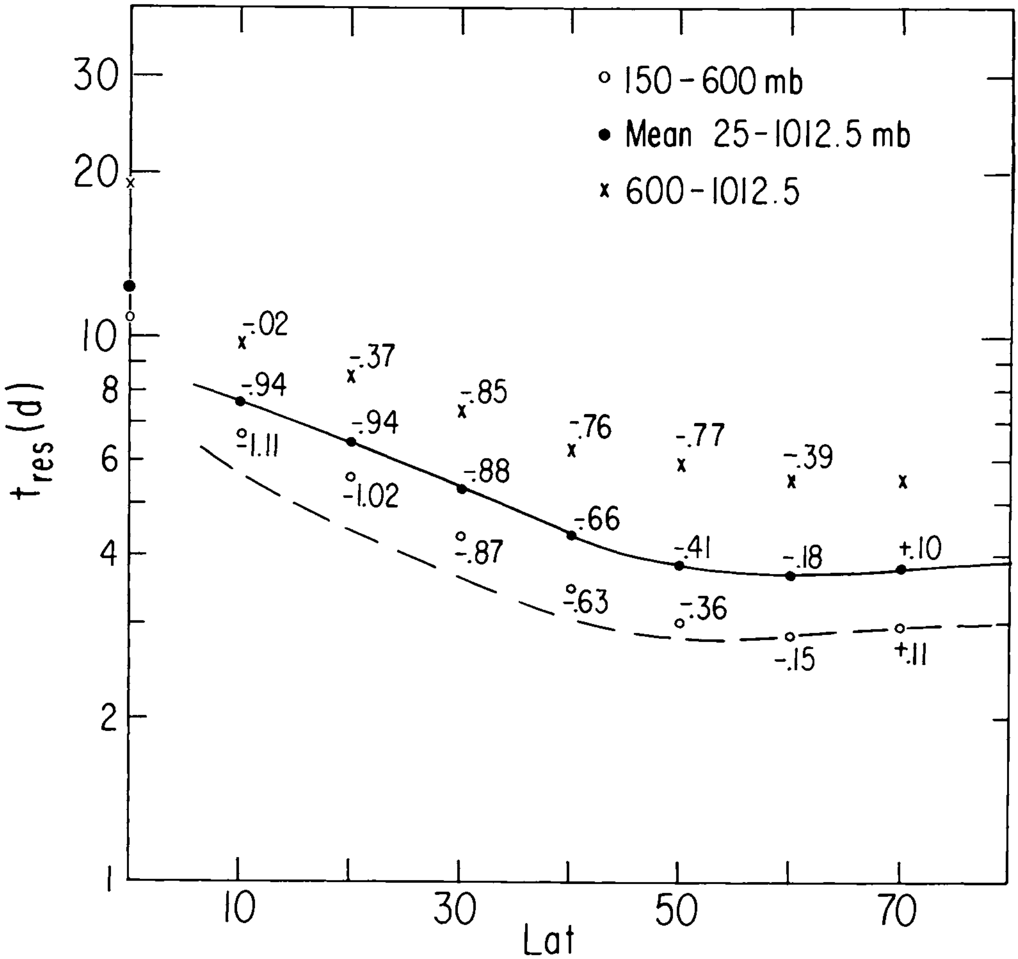

Figure 8. *Same as Figure 7 except for Northern Hemisphere Summer JJA and the dashed curve is the winter curve from Fig. 7.*

of only 44 days and a low troposphere value of 66 days. These values are in strong disagreement with an extensive body of estimates made from tracer data on $^{14}CO_2$ and other chemical species using box model inferences to produce exchange times between the hemispheres. Czeplak and Junge (1974) summarize many of these which give interhemispheric exchange times anywhere from about a year to four years. The only meteorologically based estimates previously available appear to be Newell *et al.* (1969) and Litvinenko (1965). We will return to this topic below.

Fig. 6 gives the results for Northern Hemisphere summer (JJA). As expected, the cap times are higher but by not a great deal in middle and high latitudes, where the differences from winter values are only one or two days for the vertical mean.

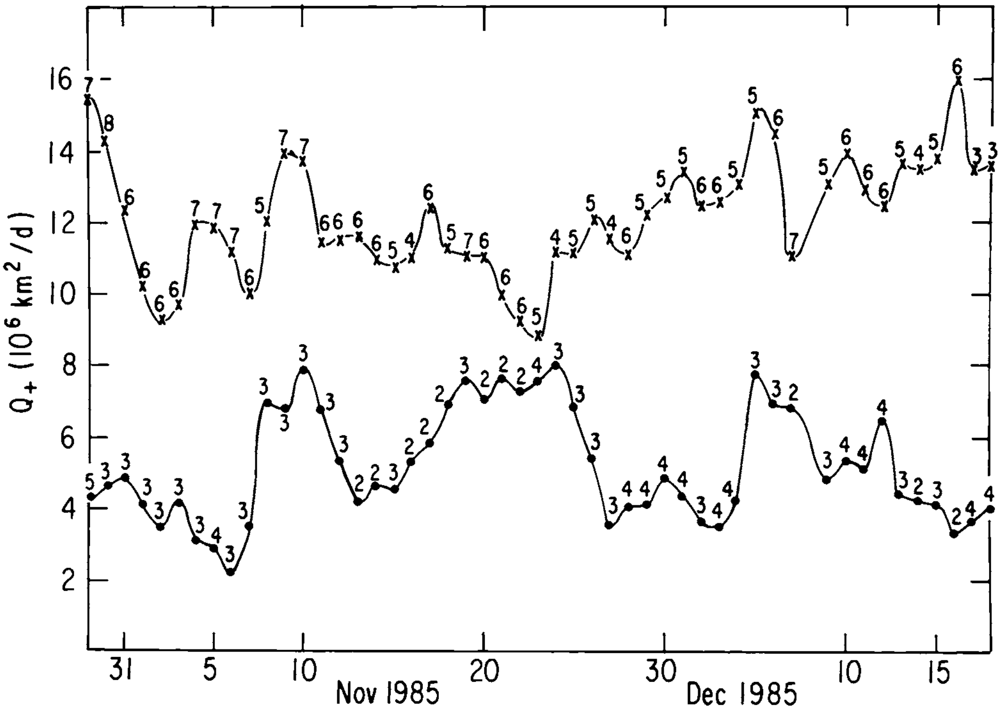

Figure 9. *Geostrophic flux values in units of* $10^6 km^2/day$ *at latitudes 50° and 70° from daily NMC Northern Hemisphere Analyses at 500 mb for 00 GMT. November 1985 through December 1985. Cap areas are 59.7 and 15.4 respectively and the 50-70° zone is 44.3 in units of* $10^6 km^2$. *Entered figures are the number of maxima (minima) around each circle.*

However, in the strip 10°to 30°, the summer times are around 10 days longer. The overall impression is of shorter times than one might have expected from the weaker summer circulations. As indicated by Fig. 3, the southern hemisphere cap residence times are appreciably different being shorter at many lower and middle latitudes.

The meridional indices calculated for a number of years by the MIT Extended Forecasting Project (Allen *et al.*, 1940; Willett, 1941) directly determine geostrophic flow and cap residence times. The first such data for the International Polar Year 1932-33 and 1935-36 (Stone and Willett, 1944) are shown in part in Table 1. In spite of being determined only from surface pressure analyses, the values are seen to be very comparable to those of Fig. 5.

Zone residence times for 20°strips centered on the plotted latitude are given in Figs. 7 and 8. These values in middle latitudes, e.g. 30-50°, essentially measure exchange times northward of tropical air and southward of polar air across the strip. Striking features are the flat distribution across latitude, with changes of only a few days from 10°to 60°; the small values for all layers, within a few days of five; and the changes of only a couple of days from winter to summer. The last particularly would have been difficult to foresee given the impressions of sluggish circulations on many summer maps. Both in winter and summer, these exchange times agree rather well with Godske *et al.*'s (1957) choice of "about a week" for an air mass lifetime.

The equatorial points on Figs. 7 and 8 are for the 30S to 30N zone which covers essentially all the source regions for true tropical maritime air. Here, the residence time values of 10 to 20 days seem to give easily adequate formation times in contrast

Latitude	30°	45°	70°
Area A_c $(10^6 km^2)$	127.5	74.7	15.4
Q_+/V_+ $(\frac{10^6 km^2 d^{-1}}{ms^{-1}})$	1.498	1.223	.592
Meridional Index (ms^{-1})			
Dec-Feb 1932-3	6.04	7.66	6.71
Dec-Feb 1935-6	4.68	7.16	5.09
Q_+ $(10^6 km^2 d^{-1})$			
Dec-Feb 1932-3	9.05	9.37	3.97
Dec-Feb 1935-6	7.01	8.76	3.01
Residence Time (d)			
Dec-Feb 1932-3	14.0	8.0	4.0
Dec-Feb 1935-6	18.0	8.5	5.0

Table 1 *Surface meridional indexes for the winters of the polar years 1932-33 and 1935-36 and corresponding fluxes and cap residence times for 30°, 45°, and 70° north latitude (Stone and Willett, 1944).*

to the 70°polar cap problem.

Geostrophic fluxes are particularly easy to determine on latitude circles. One example is given in Fig. 9 and Table 2 where daily flux values at 500 mb from NMC Northern Hemisphere analyses at 00 GMT are given for latitudes 50°and 70°over the period Oct., 1985 to Feb., 1986. The instantaneous residence times are 4 to 7 days for 50°and 2 to 4 days for 70°while the zone time between the two circles hardly varies outside 2 to 3 days. 500 mb should be roughly representative of an overall average and these values jibe with Figs. 5 and 7. The attempts to find meridional indices that are significant markers of circulation changes and significantly correlated with other measures of the general circulation were not particularly successful (Forsdyke 1951, Stone and Willett 1944, Willett 1949), but the question is probably worth being systematically reopened. Thus, the large changes in the fifteen day correlations of 50°flux with 70°flux in Table 2 suggest some sort of significant change in circulation types.

Limited region fluxes and residence times

In many of the areas of current interest that will be mentioned in the last section, the important applications of these ideas are to fluxes and transit and turnover times for particular regional areas. We give three examples before returning to discuss some general topics.

With respect to the original Curry and Wexler question of formation times for polar continental air, Table 3 gives data extracted from Oort (1982) for two continental areas in Siberia and North America that, together with the 70°cap, comprise essentially the total areas available as source regions for Winter polar and Arctic air. The layers chosen are low troposphere to 1000-700 and 1000-850 mb and the data are separated into time-mean components on each of the four boundary curves. On the north and south boundaries, Oort's maps of time-mean eddy V^2 by season allow one to determine the mean absolute deviation $\hat{V}$ on the assumption that eddy scales of V are sufficiently less than the length of these boundaries. On the east and west boundaries there might be some contribution from variances of U but they are short enough that it is probably safe to neglect them.

1985, 1986	$Q_+(50)$ ($10^6 km^2/day$)	σ	$Q_+(70)$ ($10^6 km^2/day$)	σ	r 50-70
01-1510	11.64	1.76	3.57	.68	−.128
16-3110	12.42	1.42	4.62	.77	−.292
01-1511	11.37	1.32	4.73	1.69	+.605
16-3011	11.11	1.09	6.15	1.49	−.591
01-1512	13.11	.95	5.21	1.31	+.050
16-3112	13.14	1.48	4.40	1.05	−.303
01-1501	12.64	2.19	3.83	.71	+.206
16-3101	12.91	1.85	4.96	1.11	+.543
01-1402	11.76	2.54	5.84	1.64	−.618
15-2802	9.89	1.49	6.68	1.49	−.815

Table 2 *Fifteen-day averages and standard deviations of geostrophic fluxes at 500 mb for latitudes 50° and 70° in units of* $10^6 km^2/day$. *Data from NMC Northern Hemisphere analyses for 00 GMT from October 1985 to February 1986.*

Even though these are low troposphere layers, it is somewhat surprising that the $\overline{u}$ fluxes are as small a fraction as they are of the totals. Both for Siberia and North America (Canada), the results are rather low values of turnover times: 4 to 5 days for Siberia and around 3 days for Canada which begins to look marginal as a source region in this sense. We will take up some of the implications in the last section.

Table 4 gives both winter and summer data for a subtropical North Atlantic region that covers the main Atlantic maritime tropical air source areas. The data is organized as in Table 3 and the same low contribution of $\overline{u}$ fluxes holds except for the winter vertical mean flux which is dominated even in these low latitudes by upper troposphere and lower stratosphere westerlies. The regional residence times in the low layers are essentially 7 or 8 days in both seasons, which for this region where much of the area is dominated by relatively steady trades, have to be rather conclusive evidence of the mean time available for tropical air formation. On general grounds these times appear adequate if on the short side again.

The last example is a set of geostrophic fluxes from the United States for the maps of 12 Oct. 1980. Table 5 gives the results which were chosen to be indicative of what can be expected for any area in middle latitudes large enough to be the same order of size as a typical synoptic system. Perhaps half as large an area would be closer to an average system and would have still shorter times (similar to half). The lower

Siberia

(60°E-140°E, 40°N-70°N; 16.8 $10^6 km^2$)
Flux out in $10^{10} kg/s$

		1000-700mb	sum	1000-850mb	sum
N boundary	$\overline{V}$	.56		.51	
	$\hat{V}$	2.30	2.86	1.94	2.45
S boundary	$\overline{V}$	3.13		1.36	
	$\hat{V}$	3.54	6.67	3.26	4.62
W boundary	$\overline{U}$	-3.26		-.76	
E boundary	$\overline{U}$	2.34	2.34	.92	.92
Sum out		11.87		7.99	
Mass		$5.15 \times 10^{16} kg$		$2.58 \times 10^{16} kg$	
T_{res}		5.0 day		3.7 day	

North America

(120°W-60°W, 50°N-70°N; 7.4 $10^6 km^2$)

		1000-700mb	sum	1000-850mb	sum
N boundary	$\overline{V}$	-1.26		-.70	
	$\hat{V}$	1.70	1.70	1.45	1.45
S boundary	$\overline{V}$	2.10		.66	
	$\hat{V}$	3.47	5.57	2.80	3.46
W boundary	$\overline{U}$	-2.17		-.53	
E boundary	$\overline{U}$	1.68	1.68	.66	.66
Sum out		8.95		5.5r	
Mass		$2.26 \times 10^{16} kg$		$1.13 \times 10^{16} kg$	
T_{res}		2.9 day		2.4 day	

Table 3. *Winter (DJF) low level fluxes and residence times for Siberia and North America continental source regions. Data from Oort (1982). Fluxes in units of* $10^{10} kg/s$.

troposphere turnover times are 3 or 4 days but in the upper troposphere they are closer to 1 day than to 2. We will make use of this in the discussions of the next section.

Discussion and interpretations

Taking up first the Wexler and Curry model formation times of 26, 15 days for

Subtropical North Atlantic

(70°W-20°W, 10°N-40°N, $16.6 \times 10^6 Km^2$)

Winter (DJF)		1000-850mb	sum	1000-700mb	sum	Vertical Mean	sum
N boundary	$\overline{V}$	.85		2.02		6.5	
	$\hat{V}$	2.07	2.92	4.32	6.34	21.2	27.7
S boundary	$\overline{V}$	1.0		.75		.56	
	$\hat{V}$	.84	1.84	1.89	2.64	12.6	13.2
W boundary	$\overline{U}$	.72	.72	-1.43		-37.4	
E boundary	$\overline{U}$	-.71		-.20		+30.6	30.6
Sum out ($10^{10}kg/s$)		4.48		8.98		71.5	
Mass ($10^{16}kg$)		2.54		5.09		16.95	
T_{res} (days)		6.6		6.6		2.7d	

Summer (JJA)		1000-850mb	sum	1000-700mb	sum	Vertical Mean	sum
N boundary	$\overline{V}$	.65		.78		1.74	
	$\hat{V}$	1.21	1.86	2.67	3.45	14.0	15.7
S boundary	$\overline{V}$	-.25		-1.26		-3.91	
	$\hat{V}$	.83	.83	1.91	1.91	7.7	7.7
W boundary	$\overline{U}$	1.35		2.14		-1.70	
E boundary	$\overline{U}$	-.89	1.35	-1.86	2.14	-1.70	
Sum out ($10^{10}kg/s$)		4.04		7.50		23.4	
Mass ($10^{16}kg$)		2.54		5.09		16.95	
T_{res} (days)		7.3		7.9		8.4	

Table 4. *Winter and summer low level and vertical average fluxes for the subtropical North Atlantic maritime tropical source region. Fluxes in units of* $10^{10}kg/s$.

polar continental air, it is clear from the low level results for Siberia and Canada in Table 3 and the 70°cap times in Fig. 5 that all the average residence times are 4 or 5 days or even shorter. There could be a number of possible alternatives that might reconcile these data.

1. The model times might be essentially correct and also the mean residence times. Then the formation of deep continental polar air would have to take place over a fraction of the source areas with the longer times compensated by enhanced exchange in other parts. If so, this would be an especially important property to devise regional synoptic measures for in both short and extended range forecasting. Certainly, every experienced forecaster in the U.S. pays careful attention to measures of the amount and intensity of polar air in Canada in the winter.

2. Even in the Curry (1982, 1983) model, the times might be too long. If so, some

United States

(120° W - 70° W, 30°N - 50°N, 9.42 x $10^6 km^2$)

Layer (mb)	Geostrophic Flux Out ($10^5 m^2/s$)	Q_+ ($10^{10} kg/s$)	Residence, Transit Time (day)
1000-850	310	4.7	3.6
850-700	370	5.7	3.0
700-500	470	9.6	2.4
500-300	690	14.0	1.6
300-200	900	9.1	1.2
1000-200 overall	530	43.1	2.1

Table 5. *Geostrophic fluxes from the United States on 12 October 1980, 00 GMT.*

	Root-Mean-Square Total Eddy Velocity (m/s)	$\hat{V}$ (m/s)	Q_+ ($10^6 km^2/d$)	T_{res} (day)
Low Stratosphere 50-150mb				
DFJ	5.33	4.25	7.35	35
MAM	4.43	3.54	6.12	42
JJA	4.43	3.54	6.12	42
SON	4.24	3.38	5.85	44
600-1012mb				
DJF	2.81	2.24	3.87	66
MAM	2.63	2.10	3.63	70
JJA	2.83	2.26	3.91	65
SON	2.71	2.16	3.74	68

Table 6. *Data from Oort (1982) by season for equatorial exchanges.*

process or processes capable of producing a factor of 2 or 3 speedup in rates would have to be identified and incorporated in the model. This seems rather unlikely.

3. Bolin and Rhode (1972) show that, depending on the statistical distribution of (Lagrangian) times since entering a reservoir, the turnover or residence time calculated from fluxes can be either less, the same, or greater than the mean age of the material inside. The mean age is the time that is relevant to the formation problem but so large a discrepancy again seems very unlikely. To determine directly whether mean ages are greater than turnover times would require very extensive Lagrangian statistics that might be feasible, though expensive, in computer models but would be out of the question from any feasible field data.

4. There could be systematic biases in the residence times due to poor representation of the total time and space spectra of the true motion by the observed data. But since truer values would almost certainly add to total variances the effects on fluxes and times would be in the wrong direction.

Whether one or some portion of all these possibilities may be correct can only be answered by additional research. For example, careful regional semi-Lagrangian case studies for part of these source regions might be illuminating.

More generally, there are implications for modern versions of the original air mass ideas. Flohn (1969, p. 125) gives a judicious discussion of some of the revisions, for example, that bodies fitting those ideas well are mostly low level, somewhat shallow structures. Palmén and Newton (1969) introduce "middle-latitude air" as a third principal type occupying major regions in the troposphere. The implications of Table 5 for synoptic-scale regions and any frontal zones or air masses extending to the upper troposphere are that the exchange times are on average so low (below 2 days or even fractional days) that the corresponding structures must primarily be formed by dynamics and not by the sort of regional processes originally envisaged to extend through persistent material bodies. Even in physical model experiments, as in the steady wave experiment analyzed by H. Riehl (Fultz, *et al.* 1959, Fig 85.) near-discontinuity surfaces were confined to the lowest levels near the surface cold front and were shallow compared to the deep baroclinic zones extending to the jet at the top.

Some of the most important research areas of recent decades for which these flux results have important implications are those concerned with air pollution and long-range transport of pollutants (Carlson 1981, Rahn 1981), with global budgets and various geochemically important substances including CO_2, with the transport and fate of various radioactive species including nuclear bomb debris, with the nuclear winter controversies (NRC 1985), and with the transport of water substance and the hydrologic cycle. Comprehensive surveys of many of these areas were made by Reiter (1971, 1972, 1978) and many of the estimated residence times for various species are collected there. We mentioned in section II that some interesting and large differences exist in the interhemispheric exchange times across the equator as obtained from different substances (Czeplak and Junge 1974, Junge 1962) with some values over 4 years. Most of these estimates are made from simple reservoir or box models and really do not involve knowledge of the actual fluxes but only, from concentration measurements, of changes in reservoir content with time which in various ways allow fluxes to be determined in the model indirectly. The values for the equatorial (hemispheric) residence time in Figs. 5 and 6 varied from 40 to 65 days for the three layers. Table 6 gives the results by season for the lower troposphere (600-1012 mb) and for the lower stratosphere where Oort (1980) has data only at levels of 50 and 100 mb. The lower troposphere varies only from 65 to 70 days and the stratosphere only from 35 to 45 days. This seasonal constancy was a surprise as one would have expected to see more imprint of the Indian monsoons especially in the low levels. The other meteorologically based estimate gave a result of about 0.9 y. (Newell *et al.* 1969). It was obtained, however, using only the zonally averaged Hadley cell fluxes and they clearly are overwhelmed by the eddy contribution. Or put another way, from (8) for the combined residence time to be near a year, the mean absolute deviation of V on the equator would have to be well below a half meter per second. The 60 day level is a value also obtained by Litvinenko (1965, Monin 1972).

I cannot see how to reconcile the other box model results of one to 4 years with these 70 to 65 day values but do not see how they can stand. Perhaps the absence of direct flux estimates for checking is the weak point. If a comparable procedure were followed with the net seasonal flux of dry air from southern hemisphere winter to Northern hemisphere winter (amounting to an average 2.7 mb, Trenberth 1981) the hemispheric residence times would be well over 300 years.

I am very much indebted to Dr. A.H. Oort of GFDL for supplying some of his data prior to publication. I have gained much from discussions with Dr.'s J. Curry and L. Donner and Prof.'s J. Frederick, H.L. Kuo, and G.W. Platzman. It is a pleasure to dedicate this paper to J. Namias' over half-century of service to our science.

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Global Circulation to Frontal Scale Implications of the "Confluence Theory of the High Tropospheric Jet Stream"

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The concept of transverse solenoidal circulation and acceleration of the upper westerlies in response to the confluence of warm and cold air masses, put forward by Namias and Clapp in 1949, has been widely invoked to explain atmospheric phenomena. These range from the hemispheric-scale wind distribution to the more local circulations associated with frontal zones and jet streaks. Salient ideas in the original paper are reviewed, along with selected aspects of later related investigations. These include confirmation of the ageostrophic circulations and energy transformations in the confluent and diffluent regions of wind maxima for both individual cases and the climatological mean pattern, in the latter case dominating the effects of transient eddies. Some basic aspects of the three-dimensional circulations associated with frontogenesis are briefly summarized. The distinctive characters of confluence zones in waves dominated by gradient winds, or by accelerations associated with cross-contour flow, are discussed in relation to middle- and upper-tropospheric frontogenesis processes.

Introduction

One of the seminal contributions to our understanding of the atmospheric circulation is Jerome Namias' and Philip F. Clapp's "Confluence Theory of the High Tropospheric Jet Stream", published in the *Journal of Meteorology* in 1949. This paper, now 37 years old and thus about the same age as many present-day researchers (and of Namias himself when it was written), has been increasingly acknowledged in recent years. For example, the number of journal references to this article listed in *Citation Index*, during the past two decades, quadrupled from 6 to 25. Although to some extent a tally of this kind reflects recognition of the concept to renascent areas of investigation, it is of course an incomplete measure of the offspring of the "Confluence Theory" since papers beget other papers.

The significance of the "Confluence Theory" resides in its statement of the nature of organized ageostrophic circulations forced by concentration of the solenoid field, thereby linking a ubiquitous form of deformation to the fields of divergence and vertical motion as well as to kinetic energy generation. The large-scale flow is itself to the first approximation geostrophic or, in flow with appreciable curvature, characterized by gradient-wind balance. It is, however, departures from these conditions that account for wind-speed variations along the current, whose prominence on the global and long-wave scales was brought out by Namias and Clapp. The transverse circulations associated with synoptic-scale confluence have also been shown to concentrate its effect on a much smaller scale, related to the process of frontogenesis. This essay is intended as a concise outline of some essential aspects of these subjects, rather than a complete review. To this end references have been kept to a minimum and, where authors are mentioned and references not given, these may be found in the papers that are cited.

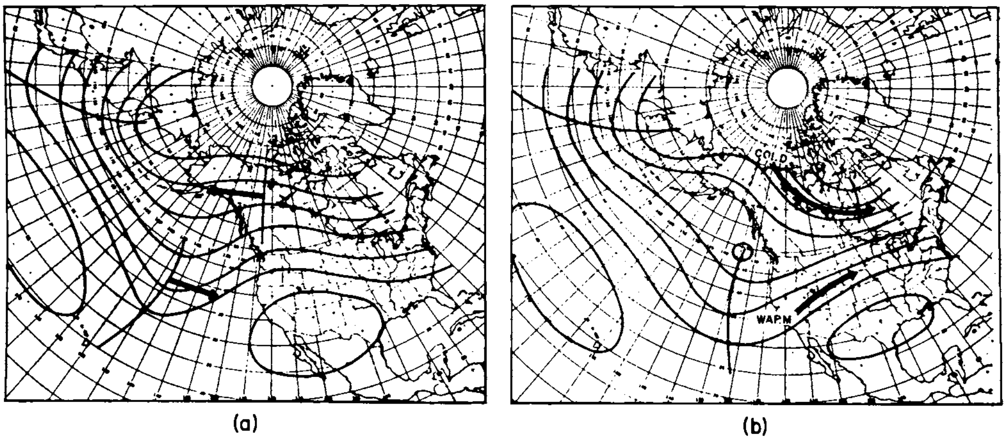

Figure 1. *"Schematic illustration of flow patterns in midtroposphere leading to confluence over North America. Lines represent streamlines; arrows in (a), subsequent weekly motion of trough and ridge systems; arrows in (b), flow of warm and cold air." (After Namias, 1947).*

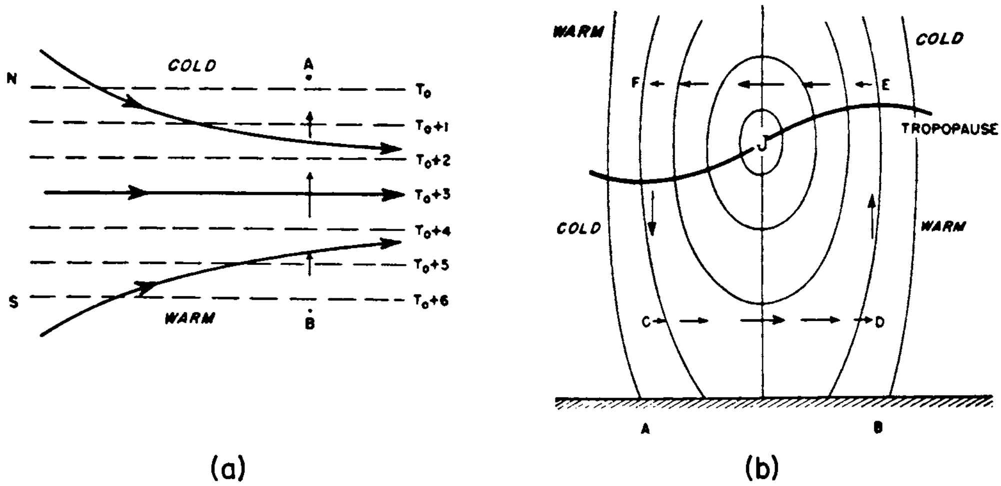

Figure 2. *(a) Streamlines and isotherms in confluence region, and (short arrows) transverse flow induced in upper levels. (b) Isotachs in vertical section across jet stream, and cross-stream and vertical circulation resulting from confluence. (After Namias and Clapp, 1949.)*

The Confluence Theory

As noted by Namias and Clapp (1949; hereafter NC), their concept was motivated as a "physical mechanism to account for some rapid fluctuations in the zonal circulation, particularly rapid increases which led to fast, narrow currents in the midtroposphere." In an earlier paper (Namias, 1947) confluence had been invoked to explain fluctuations of the middle-latitude westerlies, by a mechanism which would concentrate their energy "in fairly narrow bands where the peak speed of the westerlies is reached."

The characteristic pattern of confluence, according to Namias (1947) and NC, is associated with out-of-phase waves as in Fig. 1b, with "low-latitude troughs surmounted by high-latitude ridges. This deformation of the upper-level flow patterns is brought about by the differential motions of waves in different latitude belts." (Fig. 1a). Thus the confluence over central North America, "where warm-air currents from the south are advected beside cold polar flows", is a transient condition associated with wave movements and accordingly accounts for large temporal as well as spatial variations in the strength of the upper-level westerlies. The evolution of the flow pattern in Fig. 1 is not unique, and several others are illustrated by Namias (1947). Another common form is the fracture of a meridionally extensive trough, with its southern portion lagging and the northern part moving more rapidly eastward to a position north of a lower-latitude ridge.

The basic mechanism of the accelerating flow is illustrated by NC in Fig. 2. Confluence concentrates the lateral temperature gradient (Fig. 2a) and, as a consequence of the increasing solenoid intensity, a direct circulation is brought about. The corresponding transverse flow is from B toward A in upper levels where the winds are "continually out of balance with the strengthening pressure gradient in the sense of being subgeostrophic...This cross-isobar flow...becomes greatest at the axis of confluence where the pressure gradient is increasing most rapidly, and decreases to zero at some distance on either side in areas where there is no change in thermal gradient. The result of this cross-isobar flow is an accumulation of air to the north of the axis and a deficit to the south...Continuity considerations then require sinking motion in the cold air north of the axis and rising motion in the warm air to the south." NC acknowledge a related description of vertical motions associated with confluence, in a 1940 memorandum by R.C. Sutcliffe; the principles are developed further by Sutcliffe and Forsdyke (1950).

This circulation is portrayed, in a vertical section across the confluent flow, in Fig. 2b. The transverse components of ageostrophic flow in upper and lower levels generate vertical shear and thus represent an accomodation to the increasing thermal wind. NC also postulate an opposite transverse circulation in the stratosphere (also solenoidally direct since the temperature gradient concentrated by confluence is opposite to that in the troposphere). They point out further that the characteristic high tropopause of the warm air and the low tropopause of the cold air would be brought together by confluence, and that the resulting increase in tropopause slope across the jet stream would be further enhanced by the distribution of vertical motions in Fig. 2b.

Relation to Climatological Distribution of Upper-Level Winds

A valuable aspect of the NC paper was publication of the first maps of the mean zonal geostrophic winds over the Northern Hemisphere at jet-stream level, shown for January in Fig. 3. The jet stream "is intensively developed along and off the east coasts of the continents and practically vanishes as a sharp stream in the eastern oceans". The third wind maximum over north Africa "may result from the confluence of cold Atlantic maritime air flowing into the Mediterranean meeting hot air masses of the Sahara. Farther east the semipermanent solenoid field between the cold Asiatic continent and the Bay of Bengal and the Indian Ocean probably revitalizes the jet stream."

Later analyses of much more abundant radiosonde and wind observations have altered some aspects of Fig. 3, principally in showing that the Asia-Pacific wind maximum is centered farther north. All analyses, however, confirm the main features of the wind variation around the hemisphere, as shown by comparison of the isotachs

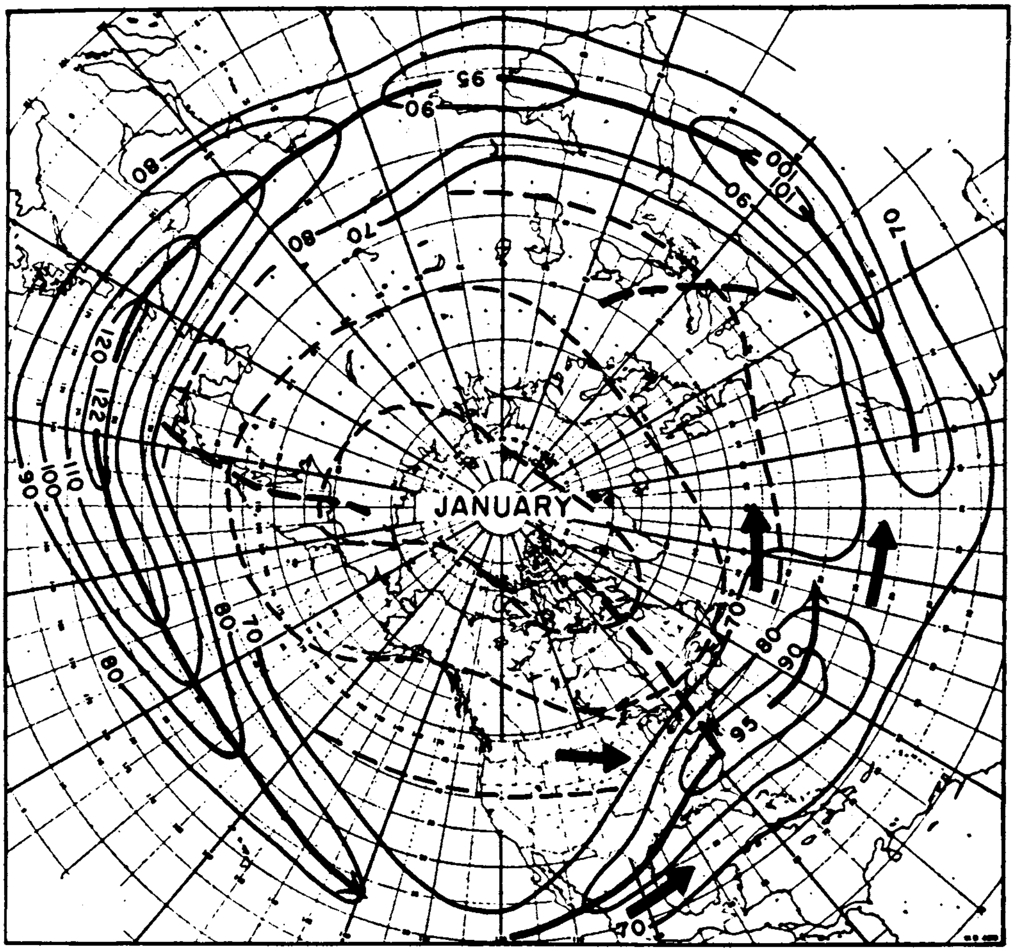

Figure 3 *"Average position and strength of jet stream for January, prepared from 18 hemispheric meridional cross sections". Solid lines, geostrophic wind at level of maximum speed in miles per hour (2.24 mph = 1 m/s); arrows, jet axis. Three mean geopotential contours (from another figure) are superimposed as dashed lines; short arrows emphasize regional confluence and diffluence. (After Namias and Clapp, 1949.)*

in Figs. 3 and 4a. Figure 4b shows further that the meridional wind components in the midtropics are out of phase with those in the midlatitude mean waves of Fig. 3. Thus (Fig. 4c) the wind maxima in subtropical latitudes are downstream from confluence regions as described by NC, with sustained strong zonal winds south of Eurasia and, as they also emphasized, marked decelerations over both the Pacific and Atlantic Oceans where pronounced diffluence is present. Figure 3 also portrays the character of the mean jet stream as a spiral around the hemisphere, beginning near 20°N and extending into middle latitudes over the Atlantic. The corresponding downstream acceleration and deceleration at the extremities of this spiral are consistent with the 10-km isobaric chart in NC which, as in Fig. 4a, displays confluence at lower and diffluence at higher latitudes over the Atlantic.

The wind distribution in Fig. 3 was adduced by Palmén (1951, 1954) as primary evidence for the existence of two major hemispheric wind systems, namely a subtropical jet stream (STJ) distinctive from the polar jet (PJ) associated with the polar-front

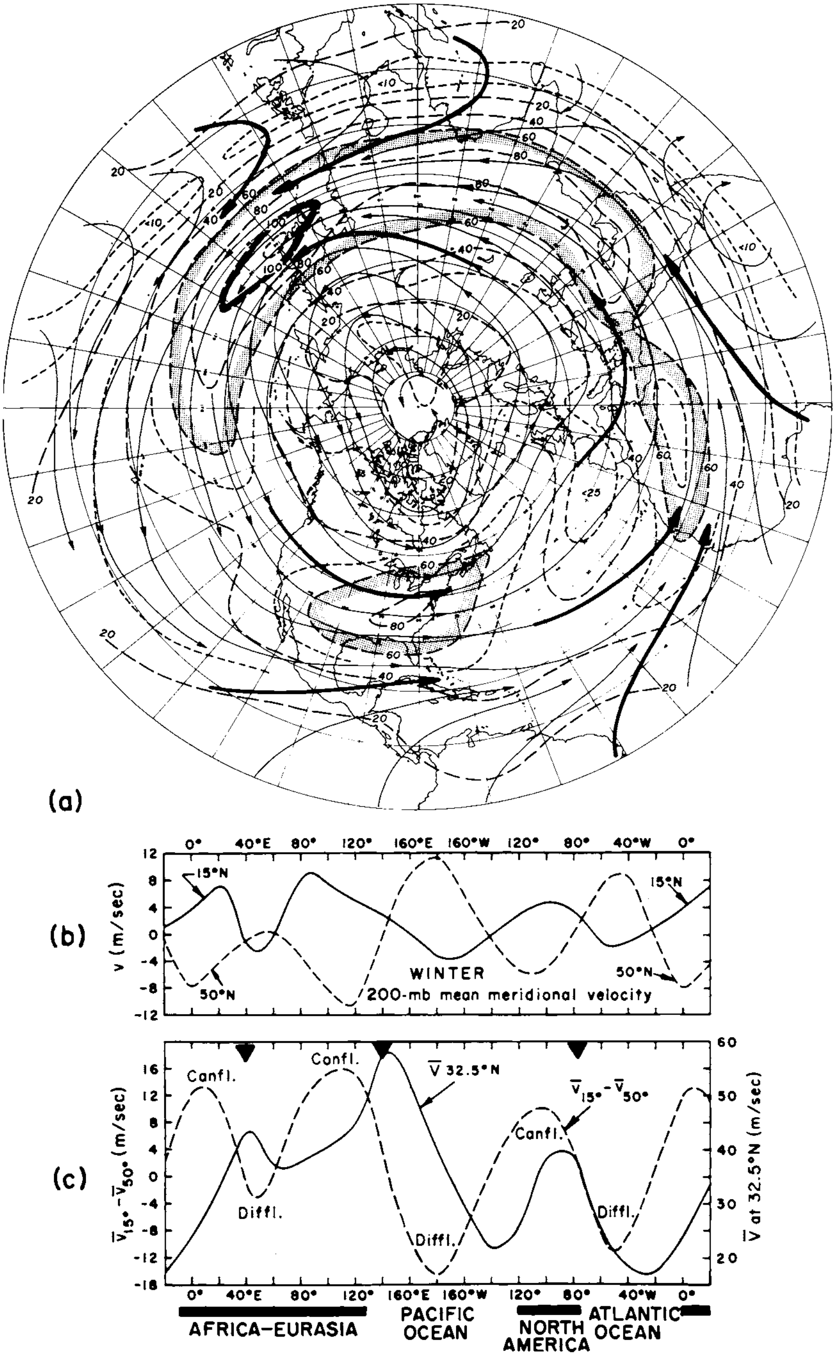

Figure 4. *(a) Streamlines at surface of mean maximum wind in winter (DJF), and isotachs (knots, stippled where vector mean speed exceeds 60 knots or 30 m/s). (b) Mean meridional wind components at 200 mb at 50° N and 15° N. (c) The corresponding confluence and diffluence (dashed) and mean zonal wind at the intermediate latitude 32.5° N. Wedges indicate longitudes of wind maxima in Krishnamurti's (1961) mean winter subtropical jet stream. (After Palmén and Newton, 1969; based on data in an atlas by H.L. Crutcher, map analyzed by R.L. Coleman).*

baroclinic zone, which had been emphasized in most earlier studies. By contrast with the PJ which is not prominent on seasonal mean charts because of its association with mobile large-amplitude waves, the STJ is steadier in location and dominates the mean isotach pattern. With Palmén's interpretation that the STJ is a basic westerly current generated by the tropical Hadley cell and thus located near its poleward boundary, the confluence theory then accounts for velocity variation along this basic current. It is historically interesting to note that both NC and Palmén (1949) challenged Rossby's theory (University of Chicago, 1947) that the jet stream is produced and maintained by lateral mixing of absolute vorticity within a "polar cap" in extratropical latitudes. Although this process was demonstrated to be consistent with the wind profile on the poleward side of the jet stream, NC and Palmén turned the proposition around. They argued that the lateral mixing (or eddy transfer) processes must be a consequence rather than a cause of the jet stream, which they attributed instead to the conversion of potential to kinetic energy by direct solenoidal circulations.

Ageostrophic and Eddy Accelerations of the Mean Zonal Flow

In daily analyses of the subtropical jet stream for winter 1955-56, Krishnamurti (1961) found that wind maxima along the stream showed a strong preference for certain longitudes. These were near the longitudes of the three main isotach maxima in Fig. 3, showing that the steadiness, as well as the prominence, of these features of the STJ accounts for the mean wind distribution.

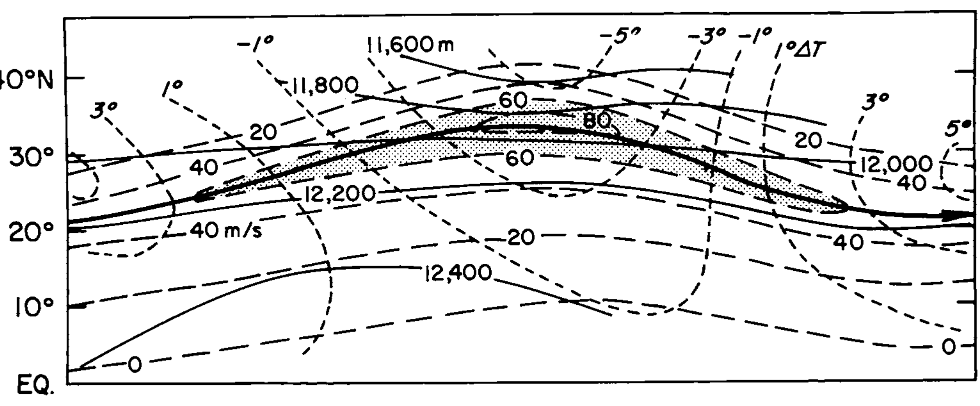

Figure 5. *Composite structure of waves on the subtropical jet stream at 200 mb in December 1955. Solid, contours at 200-ft (60 m) intervals; heavy dashed lines, isotachs (m/s) and jet axis; thin dashed lines, departure (C°) of the 850-200 mb layer-mean temperature from the mean over the length of the jet streak, averaged in a curvilinear coordinate system referred to the jet axis. The isotachs have been altered to correspond to mean speed variations between trough and crest for the three-month winter. (After Krishnamurti, 1961, redrawn.)*

Figure 5 illustrates the structure of a mean wave on the STJ, derived by Krishnamurti by compositing observations relative to the jet-stream axis and with respect to waves on the STJ (rather than by time-means at geographical locations). Consistent with NC, this shows the confluence-diffluence pattern of the upper level contours, the increase of tropospheric temperature gradient (thermal wind) to a maximum downstream from the confluence, and cross-contour meandering of the jet axis as necessary to account for maximum wind speed in the wave crest. The kinematic nature of this

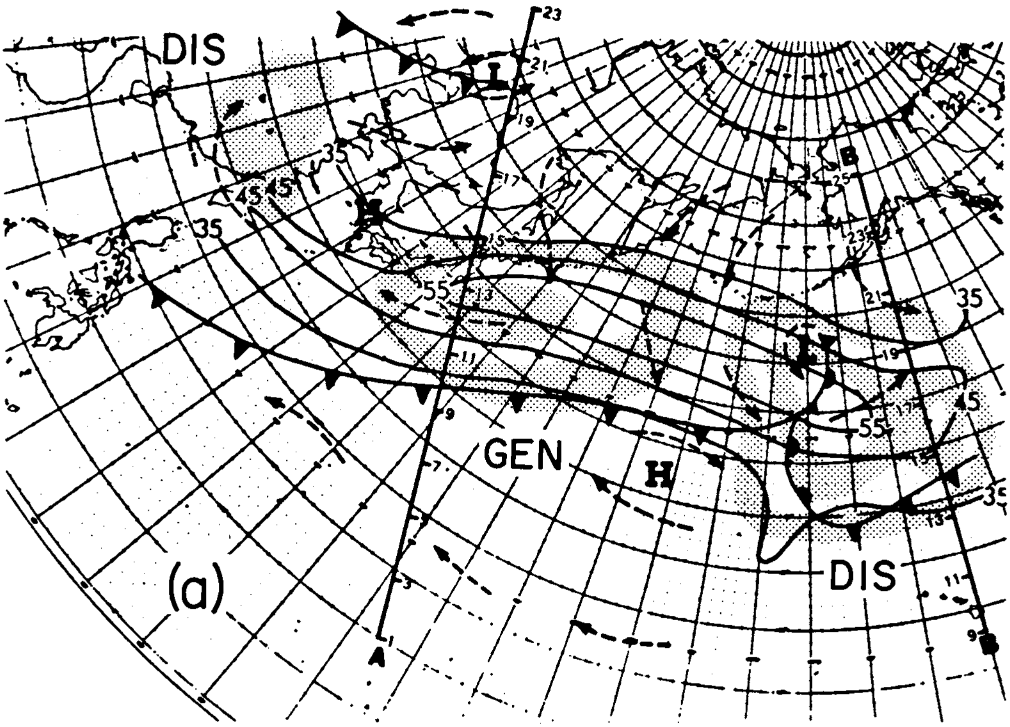

Figure 6a. *Isotachs at 200 mb, and surface fronts and directions of surface geostrophic flow, over the North Pacific at 00 GMT 20 November 1979. Stippling indicates kinetic energy generation (or in exit zone, dissipation) in the intervals 2 - 4 and 6 - 8* $\times 10^{-2}$ *J* $kg^{-1}s^{-1}$. *(After Cressman, 1981.)*

meander through a wind maximum or "jet streak" (Newton, 1959) is examined in Section 6.

The transverse circulations in Fig. 2 have been confirmed in several modelling simulations based on idealized jet streak structures (see, e.g., Uccellini and Johnson, 1979), and by Cressman (1981) from NWP model diagnoses of observations over the western Pacific. The direct circulation in the confluent entrance of the jet streak of Fig. 6a is shown in Fig. 6b. As stated by NC, "In accordance with the circulation theorem of V. Bjerknes this represents a direct transformation of part of the potential energy of mass distribution into kinetic energy", appearing "as an increase in the speed of the westerlies [downstream]." In Fig. 6b the KE production is greatest near the core of the jet stream, owing to the combination of strong transverse ageostrophic flow and geopotential gradient.

Although Fig. 6 and other examples by Cressman confirm the cross-stream circulations deduced by NC for instantaneous jet streaks, their application of the principle to the time-averaged pattern in Fig. 3 requires further examination. This application embodied the implicit assumption that, along the axis of the mean jet stream,

$$\frac{d\overline{u}}{dt} \approx \overline{u}\frac{\partial \overline{u}}{\partial s} \approx f\overline{v}_a \tag{1}$$

where overlines denote the time mean and v_a represents the cross-contour ageostrophic flow. While the day-to-day steadiness of the wind field (noted above) suggests the broad

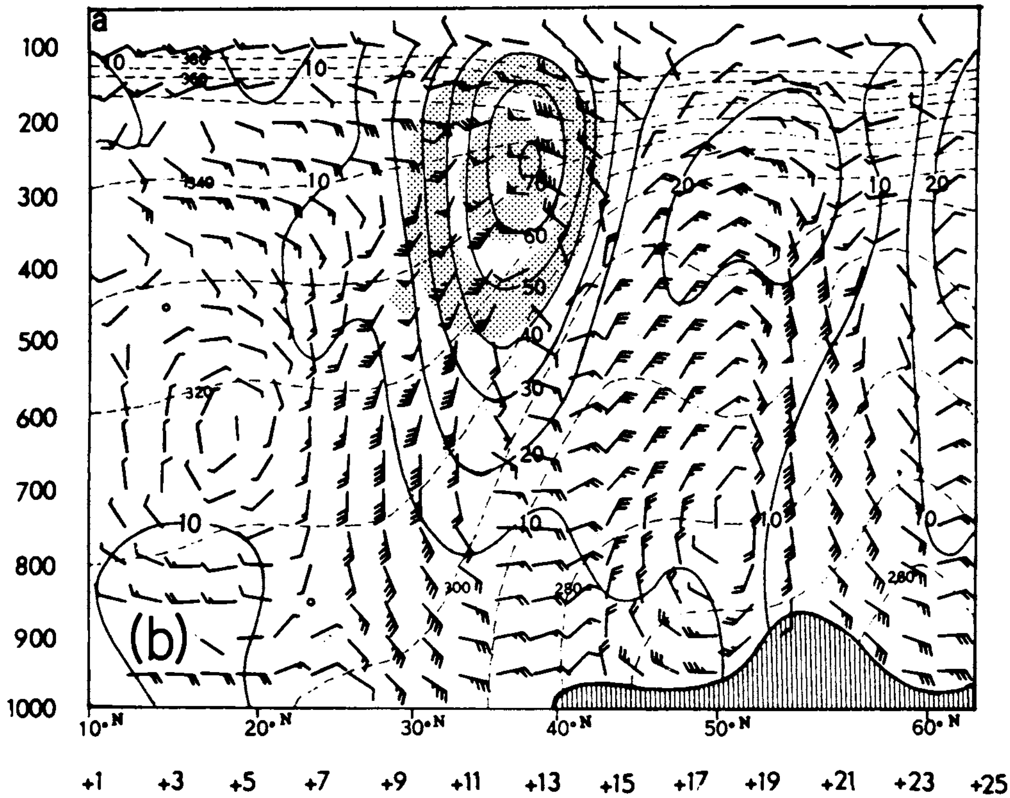

Figure 6b. *Vertical section showing isotachs and transverse circulation at line A in the jet-streak entrance. Wind symbols are scaled to the vertical dimensions of the figure: for lateral motions a full barb represents a transverse ageostrophic component of 1 m/s and a pennant 5 m/s, and for vertical motions a barb represents a value of* $\omega = 0.32 \mu b/s$. *Stippling indicates kinetic energy generation in the intervals 2 - 4 and 6 - 8* $\times 10^{-2}$ *J* $kg^{-1}s^{-1}$. *(After Cressman, 1981.)*

validity of this assumption, the possibility existed that eddy fluxes might contribute in a major way to the momentum and kinetic energy variations expressed by the mean pattern. That is, symbolically,

$$\overline{u}\frac{\partial \overline{u}}{\partial s} = f\overline{v}_a + convergence\ of\ eddy\ fluxes \tag{2}$$

where the eddy fluxes are represented by the components $u'v'$, $u'u'$ and $\omega'u'$ and primes denote local deviations from the time mean associated with transient eddies.

The relative magnitudes of the mean-flow and eddy contributions to the momentum balance were examined by Blackmon *et al.* (1977) and further by Lau (1978), based on twice-daily NMC analyses for 11 winters. These studies showed that the main eddy activity (wind variance) in upper levels is located generally over the oceans poleward of and downstream from the climatological mean wind maxima. Poleward eddy momentum fluxes $(\overline{u'v'})$ are greatest south of these "cyclone tracks" (with separate $\overline{u'v'}$ maxima of a different nature over the western continental regions). North of the cyclone tracks, there are weaker southward momentum fluxes, such that there

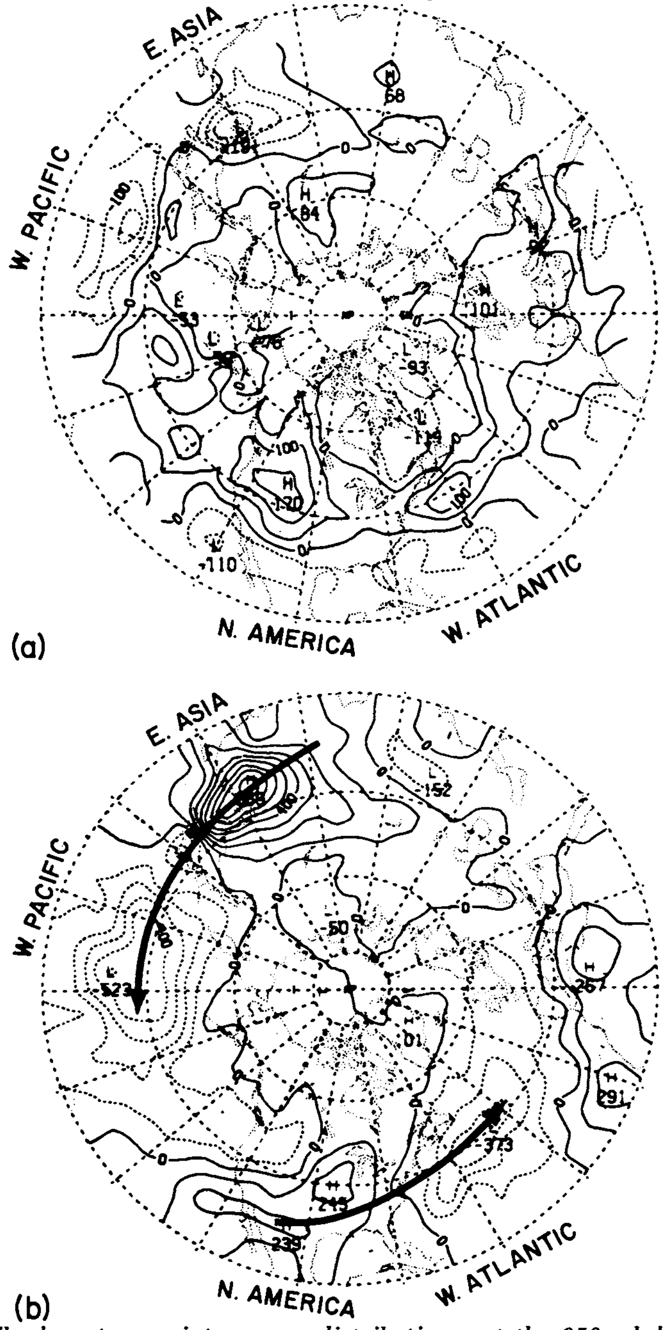

Figure 7. *The long-term winter mean distributions, at the 250-mb level, of (a) horizontal convergence of transient eddy* $\overline{u'u'}$ *and* $\overline{v'u'}$ *flux of westerly momentum; and (b) coriolis acceleration associated with the time-mean meridional ageostrophic flow,* $f\overline{v}_a$*, derived from an evaluation of terms in the momentum budget. Units are* 10^{-6} $m\ s^{-2}$*, isopleths at interval of 50 units in (a) and 100 units in (b). Arrows denote axes of Asia-Pacific and North America-Atlantic jet streams. The main storm tracks (as measured by eddy fluctuations of the v component), not shown, are poleward of and downwind from the mean wind maxima. (After Lau, 1978.)*

is a meridional convergence of the eddy flux $\overline{u'v'}$. Considering both this and the component $\overline{u'u'}$, the convergence of the transient-eddy momentum fluxes is shown in Fig. 7a for the 250-mb level. The $f\overline{v}_a$ term in (2) is shown in Fig. 7b. This was evaluated by Lau (1978) by an indirect method from calculations of the other significant terms of the momentum budget equation (direct estimates show a generally similar pattern but much smaller values, evidently owing to a strong gradient-wind constraint in the initialization of NMC analyses).

Comparison of these distributions reveals, as noted by Lau, that "the $f\overline{v}_a$ term is typically larger than the momentum flux convergence term by a factor of 3 to 4". Moreover, the transient-eddy contribution is generally opposite in sign to the zonal accelerations associated with the transverse ageostrophic flow. Thus, as concluded earlier by Blackmon *et al.* (1977), the meridionally direct circulations in the entrance regions and the indirect circulations over the oceanic exit regions are consistent with the Namias and Clapp mechanism, which is necessary to account for the climatological mean wind distribution.

Transverse Circulations Forced by Confluence in Frontal Zones

Although the process of frontogenesis by deformation in a horizontal plane was proposed by T. Bergeron in 1928 and later elaborated especially by S. Petterssen, the extension to three-dimensional circulations forced by confluent flow was introduced by Sawyer (1956). He calculated the transverse circulations, in a variety of prescriptions of confluent fields, with the general result summarized schematically in Fig. 8 which is essentially similar to Fig. 2b. The primary confluent wind field was prescribed as geostrophic, and the "geostrophic momentum" assumption (due to A. Eliassen) was made, in which the principal component of wind (U) is taken to be in geostrophic balance but cross-contour ageostrophic flow (v_a) accounts for acceleration of the current. As a consequence of the transverse ageostrophic motions which are greatest near the jet axis, secondary confluence is induced at A and B. This concentrates baroclinity in the sloping layer A-B, rather than in a vertical zone such as would be implied by the geostrophic confluence alone. (In the middle troposphere, frontogenesis of this kind is counteracted by the solenoidally direct vertical motions; see Section 6). The calculations showed also that condensation in the warm air increases the vertical motions there, and, since this heat source further enhances the thermal gradient, the direct solenoidal circulation associated with confluence is intensified (dashed arrows in Fig. 8b).

Eliassen (1962) extended and generalized the theory, whose essential physical interpretation is embodied in an elegant expression he introduced for the "source density" Q forcing the transverse circulation. Omitting diabatic effects,

$$Q = \frac{2}{g\rho}\left(-\frac{\partial Ug}{\partial z}\frac{\partial v_g}{\partial y} + \frac{\partial U_g}{\partial y}\frac{\partial v_g}{\partial z}\right) \tag{3}$$

Here g is gravity, ρ is density, z is height, U_g is the principal component of the geostrophic wind (e.g. with the x-axis along a front or jet stream) and v_g the component along y to its left (in the Northern Hemisphere). Here I shall consider only the first term. It states that the concentration of the solenoid field represented by Q, to which the wind field adjusts through the transverse ageostrophic circulation, is proportional

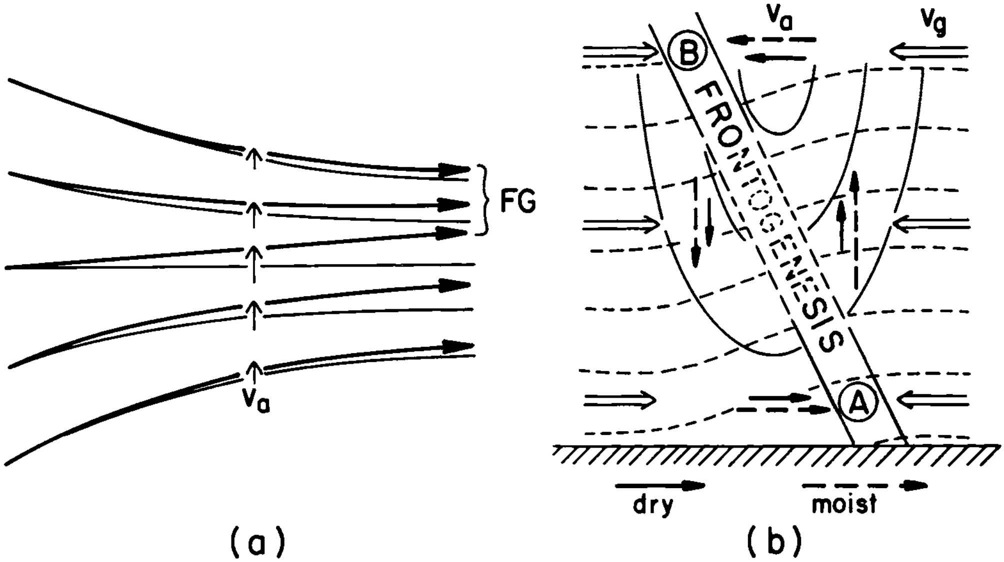

Figure 8. *Schematic features related to frontogenesis in a confluence zone. In (a) thin lines are isobaric contours at an upper level, heavy arrows are streamlines resulting from cross-contour flow v_a, and FG denotes region of frontogenesis. In vertical section (b), solid lines are isotachs of the principal component of wind U_g (into the page); dashed lines, isotherms. Double shafted arrows represent confluent component of geostrophic velocity field v_g, and thin arrows the forced ageostrophic circulation without (solid) and with (dashed) condensation heating on the warm-air side. (After Sawyer, 1956, redrawn.)*

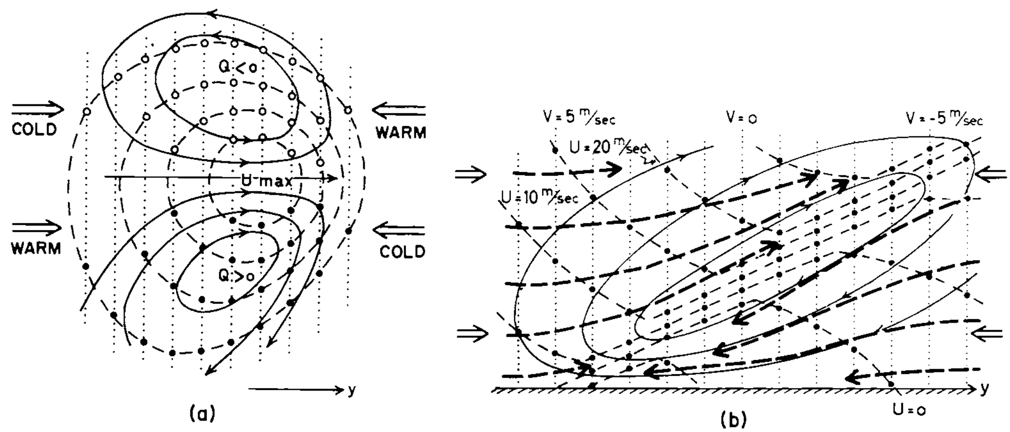

Figure 9. *(a) Isotachs of jet stream (dashed, wind toward reader), transverse component of geostrophic wind (dotted), and forced circulation in a vertical section across the current. (b) The forced circulation about a frontal layer and (long-dashed arrows) streamlines of the total transverse motion including the confluent geostrophic wind component. (After Eliassen, 1962.)*

to both the magnitudes of the temperature gradient (represented by $\partial U_g/\partial z$ through the thermal wind relationship) and of confluence ($\partial v_g/\partial y < 0$).

This being the case, the source density is greatest where the cells between U_g

and v_g isopleths are smallest, as illustrated in Fig. 9a for a jet stream with uniform geostrophic confluence. With $\partial U_g/\partial z > 0$ beneath the jet, $Q > 0$ in (1), and with $\partial U_g/\partial z < 0$ above it, $Q < 0$. Considering that the horizontal temperature gradient reverses at jet level, the forced circulations above as well as below are in a direct solenoidal sense, as suggested by NC. Although NC expressed uncertainty as to the level of maximum cross-contour flow in Fig. 2b, Fig. 9a implies that this is at the jet core. Slantwise asymmetry of the transverse circulations arises from the (realistic) form of the U_g isotachs, with greater vertical shears on the cyclonic flank.

In the case of a frontal layer in a confluence zone (Fig. 9b), the source density is most intense within the front where $\partial U_g/\partial z$ is large. Hence the transverse direct circulation slopes in the manner shown, with forced upslope motion above and downslope motion beneath. The secondary ageostrophic circulation also shows lateral contraction near the ground, which reinforces the contraction in the geostrophic wind field and thereby enhances low-level frontogenesis. The process has a "positive-feedback" since, as the front becomes more concentrated, so do the velocity and temperature gradients that force the transverse circulation. Thus Eliassen points out that once frontogenesis is under way, it is a "self-sharpening" process.

As noted, the circulations in Fig. 9 comprise only the first rhs term of (3). Ordinarily the atmospheric structure in confluent flow is such that the second term is also significant, and sometimes locally dominant (either in a reinforcing or opposing sense). This term expresses modification of the solenoid field due to differential advection when there is a temperature gradient along the jet stream-frontal zone (represented by the transverse component $\partial v_g/\partial z$ of the thermal wind, associated with turning of wind direction with height). With concentrated cyclonic shear $\partial U_g/\partial y$ within a frontal layer, Eliassen shows that this contribution to circulation forcing in its immediate vicinity is like that in Fig. 9b for warm advection, and the opposite for cold advection, distinguishing this aspect of warm and cold front circulations.

It should be stressed that the examples above were chosen to illustrate the simplest cases, and while analogous features are common to later investigations, jet stream-frontal processes are generally more complex. Circulations resulting from the combined forcing terms of (3), in a variety of circumstances, are illustrated by Keyser and Shapiro (1986) as part of a comprehensive review of the conceptual and observational aspects of fronts and jet streams. This review also discusses in detail an additional important accompaniment of confluence, namely the extrusion of stratospheric air into the tropospheric frontal zone in the vicinity of B in Fig. 8b.

Confluence in Distinctive Wave Types: Gradient Winds and Inertial Meanders

As discussed in Section 4, the NC Confluence Theory described the essential characteristics of jet streaks which, as in Fig. 5, can be viewed as waves that meander across the contours with pronounced *variations* of wind speed along the jet axis. Confluence and diffluence are also the leading characteristic of the different kind of upper-level wave described by Bjerknes (1937) and Bjerknes and Holmboe (1944). The essential properties of this type of wave were derived on the assumption of flow parallel to the contours, with *uniform* wind speed along the current.

The 250-mb chart in Fig. 10 was selected to illustrate approximations to these two wave types, as well as other waves having their mixed characteristics. In the subtropical jet streak over North America, similar to Fig. 5, the contours are concentrated in the maximum-wind region near the wave crest and far apart in the troughs up- and

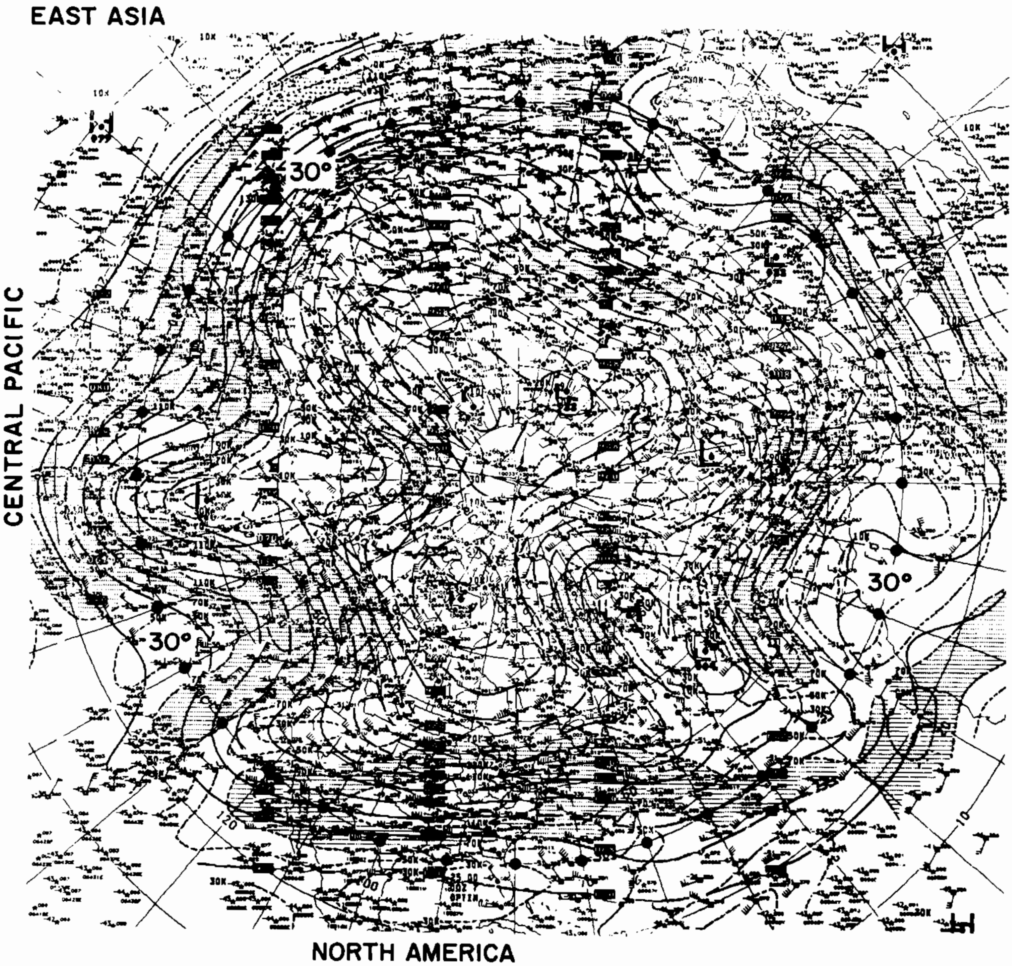

Figure 10. *250-mb chart at 00 GMT 7 January 1982. Solid lines, geopotential contours at 120-m interval; dashed lines, isotachs at 20-knot interval, hatched for winds between 70 and 110 kn (2 kn ≈ 1 m/s). (Analysis from National Meteorological Center, National Weather Service.)*

downstream. The wave over the central Pacific, associated with a deep surface cyclone, has an entirely different form. Although jet streaks are superimposed on its inflections, jet-stream speeds are comparable in the trough and neighboring ridges but the contours are most concentrated in the trough. Over east Asia, where a jet streak is superimposed on a deep polar trough extending into subtropical latitudes, the flow pattern has mixed characteristics of the waves described above. Other waves at higher latitudes around the hemisphere in Fig. 10 also have complex structures, as is typical.

Considering the various forms of observed waves, it is of interest to examine the general properties of the two primary wave types, which are sketched in Fig. 11. In a "gradient wave" or GW (Fig. 11a) the airflow is everywhere parallel to the contours and, with no acceleration of wind speed along the current, convergence (C) and divergence (D) depend on the difference of the flux $U\Delta n$ through the contour channel whose width Δn is, in the upper troposphere, more constricted at a trough than a ridge (Bjerknes and Holmboe, 1944). This difference of channel width depends, through the

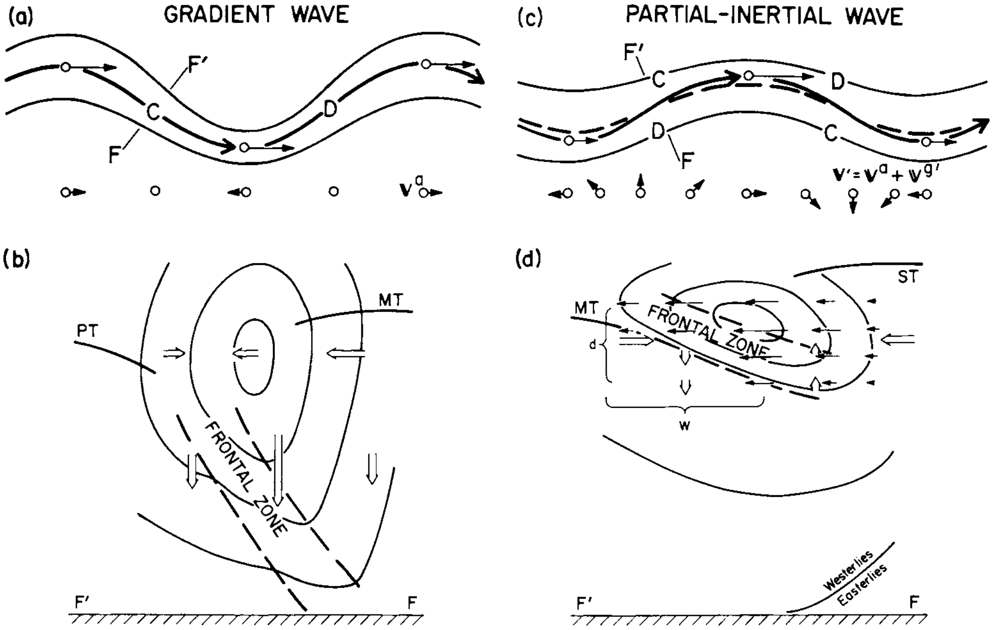

Figure 11. *(a) Features of a gradient wave at an isobaric surface in the upper troposphere. Heavy line, jet axis; neighboring lines, geopotential contours; arrows indicate wind speeds at wave crest and trough. Beneath, ageostrophic winds. (b) Vertical section at FF' in the confluent flow, with isotachs and tropopauses. Lateral arrows indicate confluence; vertical arrows, associated vertical motions. (c) As in (a) for a partial-inertial wave. Thick arrow, jet axis at level of maximum wind; dashed line, jet axis at a higher or lower level where cross-contour meander is smaller. Beneath, the oscillation about a mean velocity, comprising both the ageostrophic wind and a similar variation of the geostrophic wind. (d) As in (b) but for the PIW. Broad arrows indicate geostrophic confluence; thin arrows, transverse ageostrophic winds. In this case, the vertical arrows represent vertical motions associated with the ageostrophic flow, which are weak in proportion to the ratio d/w. The concentration of vertical shear in the uppermost troposphere, although generally characteristic of wave crests of the STJ, is usually less pronounced and distinct fronts are not always present. However ageostrophic oscillations are generally prominent in a shallower and broader layer than in the polar jet. (After Newton and Trevisan, 1984.)*

gradient wind relationship, upon the coriolis parameters f and trajectory curvatures at trough and ridge, which in turn are related to the length and amplitude of the wave and its movement. For a stationary wave with no divergence at a midtropospheric level where the wind speed in U_M, the wavelength L_{GW} is, derived through different but compatible considerations originally by Rossby (1939) and by Bjerknes and Holmboe,

$$L_{GW} = 2\pi \left(\frac{U_M}{\beta} \right)^{1/2} \alpha \left(\frac{1}{\cos\phi} \right)^{1/2} \tag{4}$$

where $\beta = df/dy = 2\Omega\cos\phi/a$, a being earth radius. (Since U increases with height, the divergence pattern in Fig. 11a is present in the upper troposphere in such a wave.) Characteristics of a wave like Fig. 5 are shown in Fig. 11c. Considering the ageostrophic winds at trough and ridge, together with the cross-contour flows at inflections, an air parcel traveling through the wave undergoes a velocity variation as shown beneath. This is similar to an inertial oscillation about the mean wind $\overline{U}_J$ in the jet core at the level of maximum wind. However (since the geostrophic winds also varies in a similar manner) its period exceeds that $(2\pi/f)$ of a pure inertial oscillation (Newton, 1981). Considering this characteristic, the wave type in Fig. 11c will be called a "partial-inertial wave" (PIW). The actual time of the oscillation can be estimated from isotach analyses as $T = L_J/(\overline{U}_J - c)$ where c is the movement of the jet streak. For a sample of 250-mb charts over the vicinity of North America, the modal period was found to be twice the inertial period. Although individual cases are highly variable, this indicates a systematic association between trajectory wavelength and wind speed which is (Newton, 1981)

$$\overline{L}_{PIW} \approx 4\pi \frac{\overline{U}_J}{f} \alpha \frac{1}{\sin\phi}. \tag{5}$$

(For a progressive jet streak, the instantaneous wavelength is shorter.) Corresponding to an oscillation period of two days at latitude 30°this is approximated by the STJ streaks over eastern North America and Asia in Fig. 10 (with L $\approx$ 9,000 km and $\overline{U}_J \approx$ 50 m/s). The different forms of Eqs. (4) and (5) reflect the physically distinctive natures of the wave types: In a gradient wave (Fig. 11a) the dynamics are controlled by the opposing effects of curvature and of the variation of coriolis parameter f with latitude (β); whereas in a PIW (Fig. 11c) the essential feature is a velocity oscillation related to the inertial period and thus to f itself. Thus, for given wind speeds $\overline{U}_J$, (5) indicates that jet streak meanders tend to be longer in subtropical than higher latitudes as in Fig. 10.

The distinctive physical characters of gradient and partial-inertial waves also confer different distributions of divergence and vertical motions with, in turn, differing frontogenetical processes in confluence zones. The divergence (D) and convergence (C) patterns are shown for a GW in Fig. 11a according to the Bjerknes theory in which the sign of divergence depends on the streamwise variation of ageostrophic wind, and for a PIW in Fig. 11c according to the NC theory in which the transverse component dominates. These distributions are the same as those deduced by Riehl *et al.* (1952) from vorticity-divergence relationships in waves and jet streaks.

In a GW, Newton and Trevisan (1984) show that the magnitudes of convergence and divergence depend on wind speed as well as wave dimensions (being greatest for short waves with large amplitudes). Correspondingly, beneath the confluent region upstream from a trough (Fig. 11a), descent is strongest at the jet axis (Fig. 11b). In this case "tilting", with greater adiabatic heating on the warm-air side of the frontal zone on the cyclonic-shear flank of the jet stream, augments the concentration of baroclinicity by confluence. Thus both processes favor tropospheric frontogenesis in the confluence zones of waves that are dominated by gradient winds. This is, as shown in principle by Palmén and Nagler in 1948, necessary to account for the greater baroclinicity, and upper-level geopotential concentration, in their troughs. By contrast in a PIW, solenoidally direct tilting (similar to that in Figs. 2b, 8b) partially counteracts the effect of confluence in concentrating the midtropospheric temperature gradient. Hence in this case frontogenesis is most favored in the uppermost troposphere where the transverse ageostrophic motions induced by confluence are most pronounced and associated vertical motions weak, as sketched in Fig. 11d. This conclusion is in accord with the observation that frontal layers in the crests of STJ waves are (when present) confined to the high troposphere, often just beneath a sloping layer of maximum wind as shown.

Although some additional factors have not been mentioned, the above comparison shows that the NC confluence concept implies a type of frontogenesis, in Fig. 11d, that is distinctive from the kind in Fig. 11b. This discrimination makes it clear that, for example, there is no incompatibility between the solenoidally-indirect vertical motions that have been deduced by Reed and Sanders and others in midtropospheric frontogenesis; and the direct circulations deduced by NC, since the essential characters of confluence zones may differ in the two cases. More generally, as noted earlier, the perturbation types in Figs. 11a and 11c are likely to be superimposed in varying ways. Depending upon the prominence of these wave components, one or the other may dominate: with a weak jet streak superimposed on a short-wave pattern of appreciable amplitude, the transverse circulation should be like Fig. 11b; whereas in the case of a prominent jet streak (with strong axial variations of wind speed) superimposed on a basic-current long wave of modest amplitude, a circulation like Fig. 11d is to be expected. In this light, it is not surprising that exceptionally strong jet streams are often not associated with significant tropospheric fronts.

The two kinds of waves in Fig. 11 may also, depending upon the relative phases in which they are superimposed, either interact constructively or adversely in respect to frontogenesis. It appears that their combined aspects may be necessary for the formation of a midtropospheric frontal layer. As shown by Newton and Trevisan, processes in the confluence zone of a GW can account for "clinogenesis", or simultaneous enhancements of lateral temperature gradient, vertical shear, and lateral shear on the cyclonic flank of the jet stream. It cannot, however, account for the generation of high thermal stability as the additional property of a frontal layer. For this, a variation of the transverse ageostrophic flow like that in Fig. 11d is necessary, which also enhances the vertical shear in the frontal zone. Thus the total process of midtropospheric frontogenesis is favored by suitable combinations of the two types of confluent circulations, such as superposition of a jet streak on the trough of a gradient wave. With this arrangement, confluence and tilting as in Fig. 11b concentrate the lateral gradients of temperature and momentum, and transverse ageostrophic motions as in Fig. 11d effect increases of static stability and shear (confluence also being enhanced in the entrance zone of the jet streak). Since the magnitudes of divergence and vertical motions depend on wave dimensions, frontogenesis is then most favored by short wavelength and large amplitude of the basic wave, and by a strong acceleration of upper-level wind speed in its confluence region. These considerations are apparently in accord with the association of midtropospheric fronts downstream from the confluence zone of a short wave advancing through the long-wave pattern, during which, as analyzed in detail by Bosart (1970), contributions of the various processes evolve according to the location of the short wave.

Conclusion and Appreciation

Subsequent investigations have thoroughly confirmed the Confluence Theory of Namias and Clapp. This is not surprising, since this theory comprised a straightforward and unimpeachable statement of physical expectations with an observational basis. It should nonetheless be gratifying to the authors that this paper, after the passage of almost forty years, has achieved increasing notice and that it has had such wide applications to basic atmospheric phenomena, only some of which have been mentioned above. As another example, the transverse circulations in the exit zones of upper-level jet streaks have been linked by Uccellini and Johnson (1979) to processes in the formation of low-level jets in the transverse lower branch of this circulation, and in turn to the generation of an environment favorable to severe convective storms.

At the time NC was published, the theory of long waves in the middle-latitude westerlies had been formulated by Rossby (1939), and Namias and Clapp (1944) were the first to apply it systematically to the evolution of flow patterns on five day mean charts (which isolated the long waves from the faster moving short-wave perturbations). Rossby's formula was developed with the simplifying assumptions of a zonally and meridionally uniform westerly current at a level of nondivergence in the middle troposphere, although it was empirically applicable in situations wherein these conditions were not strictly met. Namias' exposition of the confluence theory endowed a new dimension to the interpretation of upper-air flow patterns, in bringing out the significance and physical nature of baroclinity and wind variations arising from interactions of different wave systems.

While (as illustrated by Namias and Clapp) the process is important both in extratropical and subtropical latitudes, it is particularly in the subtropical jet stream that the confluence theory accounts for the dominant features of the mean wind distribution. An essential aspect, for the operation of the global circulation, is that confluence concentrates the subtropical wind maxima in the proximity of midlatitude troughs, such as to bring about a conjugation of the source of and the mechanism for effective eddy transfer of westerly momentum poleward. This has been shown by the observational studies cited, and others. Consistent with these, Frederiksen (1979) demonstrates by use of an analytical model that eddy momentum transfer, by disturbances initiated in the regions where confluence concentrates the baroclinity, is localized downstream from the midlatitude troughs.

By extension of the original description, it has been shown above that the NC concept implies a type of wave distinctive from Bjerknes-Rossby waves, with different transverse circulations in their confluence zones. Viewed in this way (consistent with deduction of the divergence fields from the vorticity patterns of waves and jet streaks, but with a different emphasis), the frontogenetical processes that alter the wind and temperature structure can be distinguished in respect to the effects associated with changes of curvature or with acceleration along the current (or the combined processes, where these waves are superimposed).

This retrospective view of the Confluence Theory has illustrated its relevance to atmospheric phenomena on scales ranging from fronts to the global circulation. This theory is, of course, only one of the many contributions by Jerome Namias on an impressively wide range of subjects. We stand in debt for the inspirations this remarkable scientist and cherished friend has given us.

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Surface-Atmosphere Interactions over the Continents: The Namias Influence

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Land surface variables such as snow cover and soil moisture are currently under active investigation in the diagnosis and prediction of short-term climatic fluctuations. The major diagnostic challenge in the study of surface-atmosphere associations is the determination of the extent to which surface fluctuations contribute rather than simply respond to atmospheric fluctuations. This challenge is being addressed with a variety of empirical strategies, many of which can be traced to the ideas and published work of Jerome Namias. In particular, many of the specific events and case studies first cited by Namias have led to more general quantitative evaluations of feedbacks involving snow cover and soil moisture.

Introduction

Land-surface variability can influence the atmosphere to the extent that the low-level heating and moisture fields are altered by snow cover and soil wetness. The diagnosis and prediction of short-term climatic fluctuations over land areas must therefore include considerations of the state of the land surface, especially since the distribution of snow cover and soil moisture vary relatively slowly in comparison with the atmospheric circulation fields. As in the case of air-sea interaction, however, the distribution of land surface variables is determined to varying degrees by the large-scale atmospheric circulation. Thus the mere existence of temporal correlations between atmospheric and land surface states does not prove that the surface variables influence or "feed back" to the atmosphere. An evaluation of the casual links is therefore a major diagnostic challenge in the study of surface-atmosphere interactions over land as well as over the oceans.

The study of surface-atmosphere interactions over land has been shaped by the work of Jerome Namias in much the same way as has the study of large-scale air-sea interaction. While Namias is perhaps better known for his research on and utilization of sea surface temperatures, the recent surge of interest in possible land-surface influences on climate has drawn heavily upon Namias' ideas and published work dating back to the late 1950's. This review is intended to show that many of the diagnostic strategies and case studies first presented by Namias have led in recent years to more general and more quantitative delineations of the short-term climatic roles of snow cover and soil moisture. While the emerging consensus is that land surface variables generally play smaller roles in short-term climatic fluctuations than do sea surface temperatures, the existence of even local influences of land surface variables may have a more direct bearing on the earth's populated regions.

In the following survey of land surface influences, snow cover and soil mositure will be discussed separately. The strategy in each case will be to highlight Namias' major contributions and then show their relevance to the more recent work. It will be apparent that the recent work has borne out many of the ideas and inferences made years and decades earlier by Namias. In order to provide a focus, the discussion here will be limited to data-based or empirical studies. The results of modeling studies will be left to other papers in this volume (e.g., Anthes, 1986) and to other recent evaluations of the boundary sensitivities of specific models (e.g., Ross and Walsh, 1986).

Snow cover

In discussions of the influence of snow cover on the atmosphere, it is convenient to distinguish the local effects from possible synoptic-scale effects. While snow cover and surface air temperature are negatively correlated locally, correlations alone are not evidence of a casual influence of snow cover. Anomalies of both snow cover and air temperature may, for example, be consequences of the same pattern of atmospheric circulation. Perhaps the earliest and most striking illustration of snow cover's actual influence on air temperature was provided by Namias (1962). The snow influence was demonstrated through a circulation-derived specification technique pioneered by Namias and still used extensively as a tool in long-range forecasting (Klein, 1983; Gilman, 1983). In this procedure, regression equations derived from gridded values of 700 mb height are used to estimate the surface air temperature over a predetermined period (e.g., 5 days, one month).

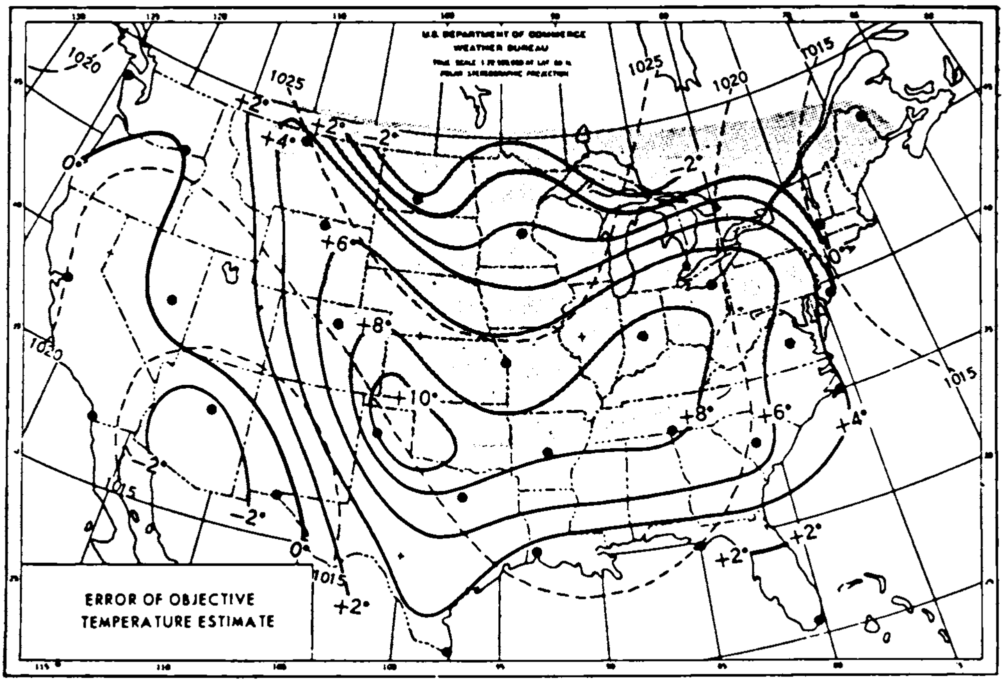

Figure 1. *Isopleths of error (°F) of Namias' 700 mb-derived specifications of surface air temperature (solid contours), and isobars of mean sea level pressure (broken contours) for the perid mid-February to mid-March 1960. Shading indicates prevailing snow cover. [Namias, 1962, Fig. 10.]*

The errors in these specifications represent the portions of the surface temperature anomalies not explained by the large-scale mid-tropospheric circulation. Surface boundary influences from anomalous snow cover and soil moisture are prime candidates for explanations of the errors. Fig. 1 shows the errors found by Namias (1962) in 700 mb-derived specifications of surface temperature for mid-February to mid-March of 1960, when snow cover extended 300-1000 km south of its normal position over much of the eastern United States. The coincidence of the region of largest errors (8-10°) and the area of anomalous snow cover indicates a substantial local influence of snow cover on air temperature. Namias (1985) demonstrated similar influences over the central and eastern United States during the winter of 1983-84. To the extent that snow cover persists, influences of this magnitude can be relevant to long-range forecasts for large areas.

While the magnitudes of the apparent snow influences in Namias' case studies are striking, the results do not indicate the generality of snow impacts. In recent work based on a 30-year sample of winter months, Walsh et al. (1982) have found that the magnitudes of specification errors near the normal snow edge are typically 2-4°F (1-2°C) when averaged over all months in which the snow anomaly exceeds 2°latitude (Fig. 2). The occurrence of the largest errors near the normal snow boundary, together with the tendency for the errors to be larger in late winter and early spring, provides general support for Namias' (1962) contention that an albedo-induced reduction of absorbed insolation is the primary mechanism by which snow cover depresses local air temperature.

The existence of a surface influence is clearly indicated by the results in Figs. 1 and 2. However, the vertical extent of the snow influence had not been adressed prior to a study published only last month by Namias (1985). Namias' strategy, which is intriguing both for its simplicity and for its result, was to plot the monthly mean vertical profiles of temperature (and dewpoint) for a station which experienced anomalous snow cover for a portion of the winter of 1983-84. As shown in Fig. 3, the monthly soundings for December 1983 and January 1984 differed substantially from the normal soundings only below 850 mb. The divergence of the observed and normal soundings below 850 mb did not occur during February 1984, when the surface had returned to its normal snow-free state. Thus, while snow cover suppressed the surface temperature by 3-5°C, the suppression was limited essentially to the boundary layer. Findings such as these are highly relevant to questions concerning possible synoptic-scale influences of snow cover, and they are also essential for establishing the validity of atmospheric model responses to surface boundary anomalies.

Just as snow cover and air temperature may be correlated because both are largely determined by the large-scale circulation, the empirical associations between snow cover and the intensities or trajectories of synoptic-scale systems cannot be easily interpreted in terms of casual mechanisms. In one of the first experiments directed at this question of causality, Namias (1962) evaluated the errors of a barotropic model's 500 mb forecasts during two Februaries with contrasting snow cover in eastern North America: 1959 (light snow) and 1960 (heavy snow). The model contained no explicit information about snow cover. Thus the systematically larger errors in 1960 over the northeastern United States could be attributed by Namias to the greater extent of snow. The patterns of errors were indeed consistent with the notion that an enhancement of coastal baroclinicity by snow cover favors rapid intensification and/or more northward trajectories of coastal storms. A similar strategy was recently used with a much larger dataset (1947-1980) by Ross and Walsh (1986), who stratified the errors of daily forecasts according to the extent of snow in eastern North America and (in separate experiments)

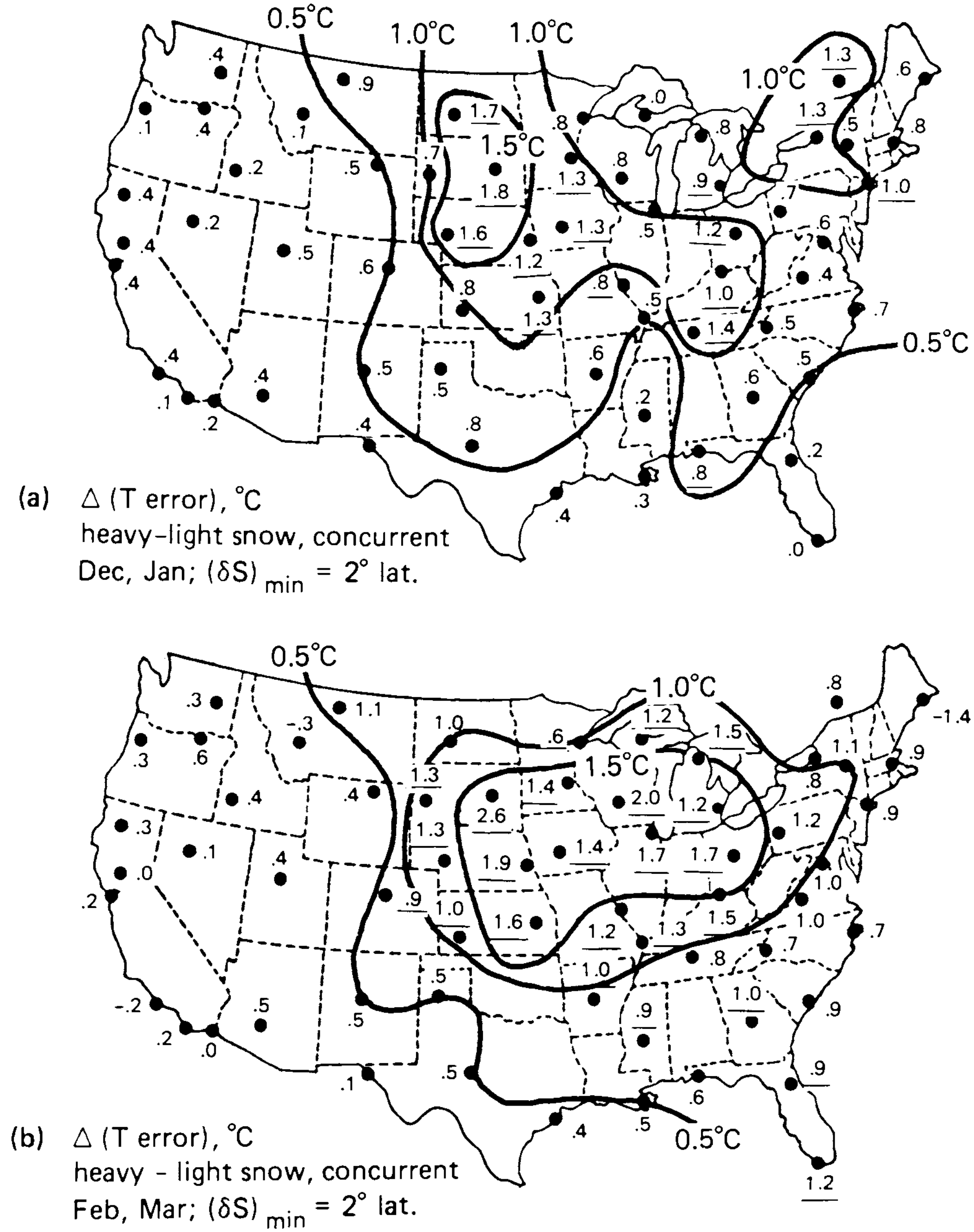

Figure 2. *Composite differences of errors in objectively specified air temperatures (° C) during months when snow extent was above- and below-normal by at least 220 km at nearest 5° longitudinal meridian; signs correspond to months with above-normal snow cover. Difference fields are shown for (a) December-January and (b) February-March. [Walsh et al., 1985, Fig. 6.]*

according to the extent of North Atlantic and North Pacific sea ice. Not only were the barotropic model results consistent with those of Namias for eastern North America, but corresponding results obtained from forecasts of simple persistence also contained similar patterns. The apparent signal was stronger in the sea level pressure fields than

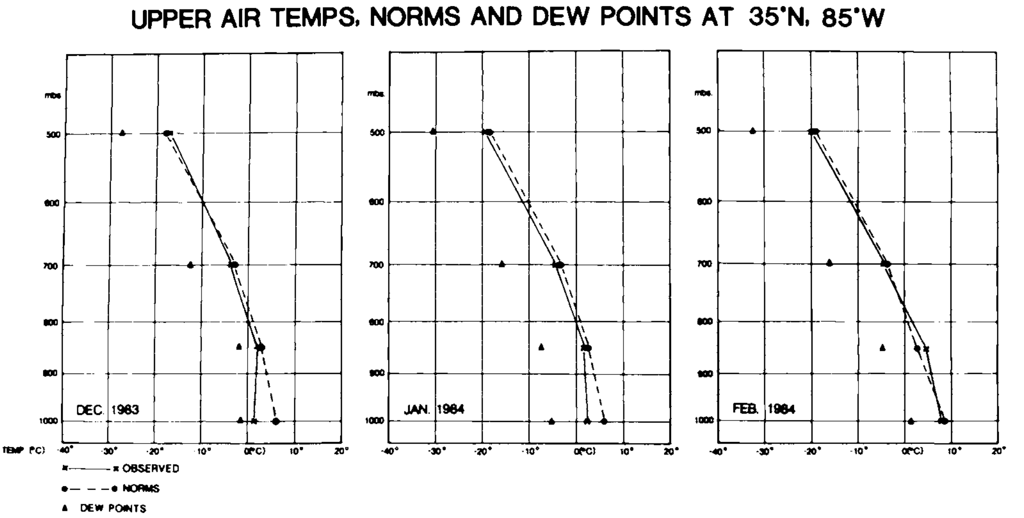

Figure 3. *Mean lapse rates (solid) contrasted to the normal (broken) for December 1983, January and February 1984, and the corresponding observed dew points (triangles). Values are for 35° N, 85° W as computed from NOAA's twice-daily upper-air analyses. [Namias, 1985, Fig. 7.]*

		Summer Temperature		
		Cold	**Normal**	**Warm**
Only Temperature	Cold	101	70	40
	Warm	57	70	87
Temperature and Precipitation	Cold, Dry	29	21	10
	Cold, Moderate	31	18	19
	Cold, Wet	41	31	11
	Warm, Dry	9	27	50
	Warm, Moderate	18	22	22
	Warm, Wet	30	16	16

Table 1. *Summer temperature classes over the western Great Plains of the United States following different combinations of springtime temperature and precipitation (from Namias, 1960, Tables 1 and 2).*

in the 500 mb fields. Moreover, as shown in Fig. 4, the dependence of systematic errors on the location of the snow/ice boundary was very similar in the experiments based on North American snow cover and North Atlantic sea ice. In each region, the errors were larger (i.e., intensification of the surface lows was more seriously underpredicted) when snow/ice cover was more extensive, while the largest differences between cases with heavy and light snow/ice were found downstream of the major baroclinic zone. These results indicate, as Namias suggested decades ago, that an anomalous distribution of snow/ice cover does influence the evolution of synoptic-scale cyclones. The influences (Fig. 4) are sufficiently large that forecasts of weekly or monthly pressure

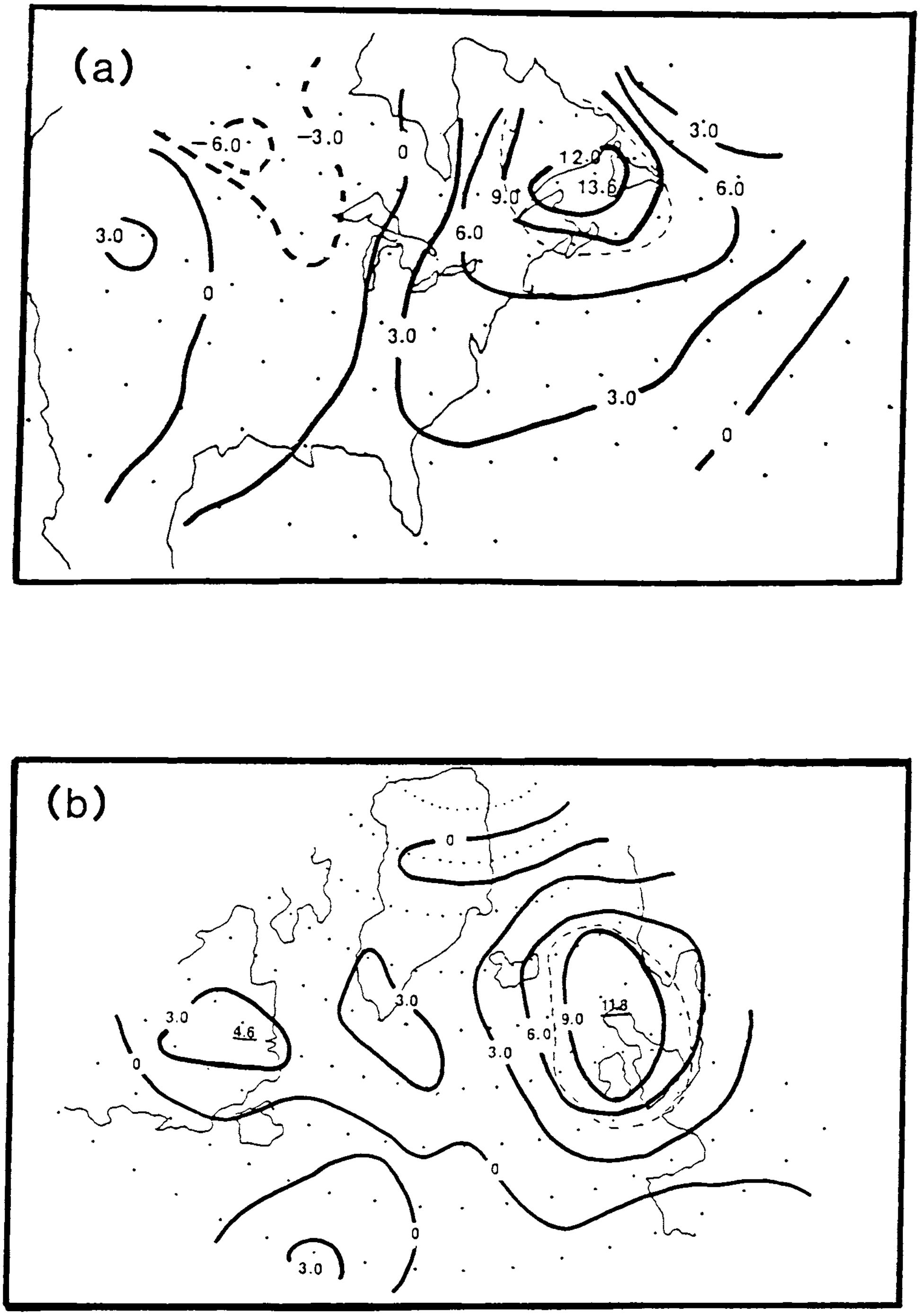

Figure 4. *Composite differences of 48-hour errors in persistence forecasts of sea level pressure (mb). Contoured values represent differences between means for cases with "heavy" and "light" coverage of (a) eastern North American snow cover, (b) North Atlantic sea ice. Values inside thin dashed line are statistically significant locally at the 95% level according to two-tailed t-test. [Ross and Walsh, 1986, Figs. 6b and 9b.]*

patterns may, in situations of extreme snow/ice cover, be improved by consideration of observed snow/ice anomalies if these anomalies persist through the forecast period.

Soil Moisture

Many of today's efforts toward the use of soil moisture in predictive applications can be traced to an analysis by Namias (1962) of seasonal climatic anomalies in the Great Plains of the United States. The results, which are summarized here in Table 1, indicate that above- (below-) normal temperatures are much more likely to persist from spring to summer when the spring is drier (wetter) than normal. For example, a cool wet spring is followed by cooler-than-normal and warmer-than-normal summers in the ratio of 41:11; a warm dry spring is followed by warmer-than-normal and cooler-than-normal summers in the ratio of 50:9. Namias argued that these tendencies are indicative of a role of soil moisture, which should modify the Bowen ratio of sensible and latent heating at the surface. In extreme manifestations of soil moisture influences, the desiccation of the soil during a warm dry spring may be a contributing factor to summer drought over the central United States (Namias, 1960). Similar reasoning concerning a soil moisture deficit has recently been used by Sweeney (1985) in an analysis of the 1984 drought over the Canadian prairies.

While the results in Table 1 are suggestive, they were obtained without explicit information on soil moisture. Direct evaluation of the climatic role of soil moisture has indeed been hampered by the absence of systematic measurements of soil water content. Much of the recent work has therefore relied on proxy indices of soil moisture computed from precipitation and temperature data. Perhaps the most common of these is the Palmer Drought Severity Index (PDSI). Unfortunately, the PDSI is based partially on a "look-ahead" feature which limits its predictive and diagnostic applications. Karl (1986) has recently computed time series of the Water Capacity (WC), an intermediate parameter evaluated prior to the "look-ahead" stage of the PDSI computational procedure. As shown in Fig. 5, the springtime values of the WC correlate significantly with *subsequent* seasonal temperatures. The precipitation- and temperature-dependent WC outperforms persistence as a predictor of summer temperatures, thereby supporting the earlier findings of Namias. Karl's results delineate the regional and temporal dependencies of the apparent soil moisture influence, which tends to shift westward from the northern Great Plains in late spring to the western Great Plains and plateau region in summer (Fig. 5).

A soil moisture index based on a simple hydrology model was used by Walsh et al. (1985) in a series of monthly temperature specification experiments similar to those performed with snow cover (see Section 2). As indicated in Fig. 6, the summer specifications tended to be too high during months with wet soil and/or too low during months with dry soil. The composites of the errors over dry and wet months were quite small, as the composite mean errors generally did not differ from 0.0 by more than 0.2-0.3°C. However, the maximum differences were found in the central Great Plains, in agreement with the results of Namias and Karl. Moreover, despite the small magnitudes, many of the differences were statistically significant at the 5% level according to a two-tailed t-test. In further support of a role of soil moisture, the number of stations with significant differences was a maximum in the May-July period. The number of such stations during winter was approximately 5% of the total number of stations.

The experiments described above provide no information about possible feedbacks

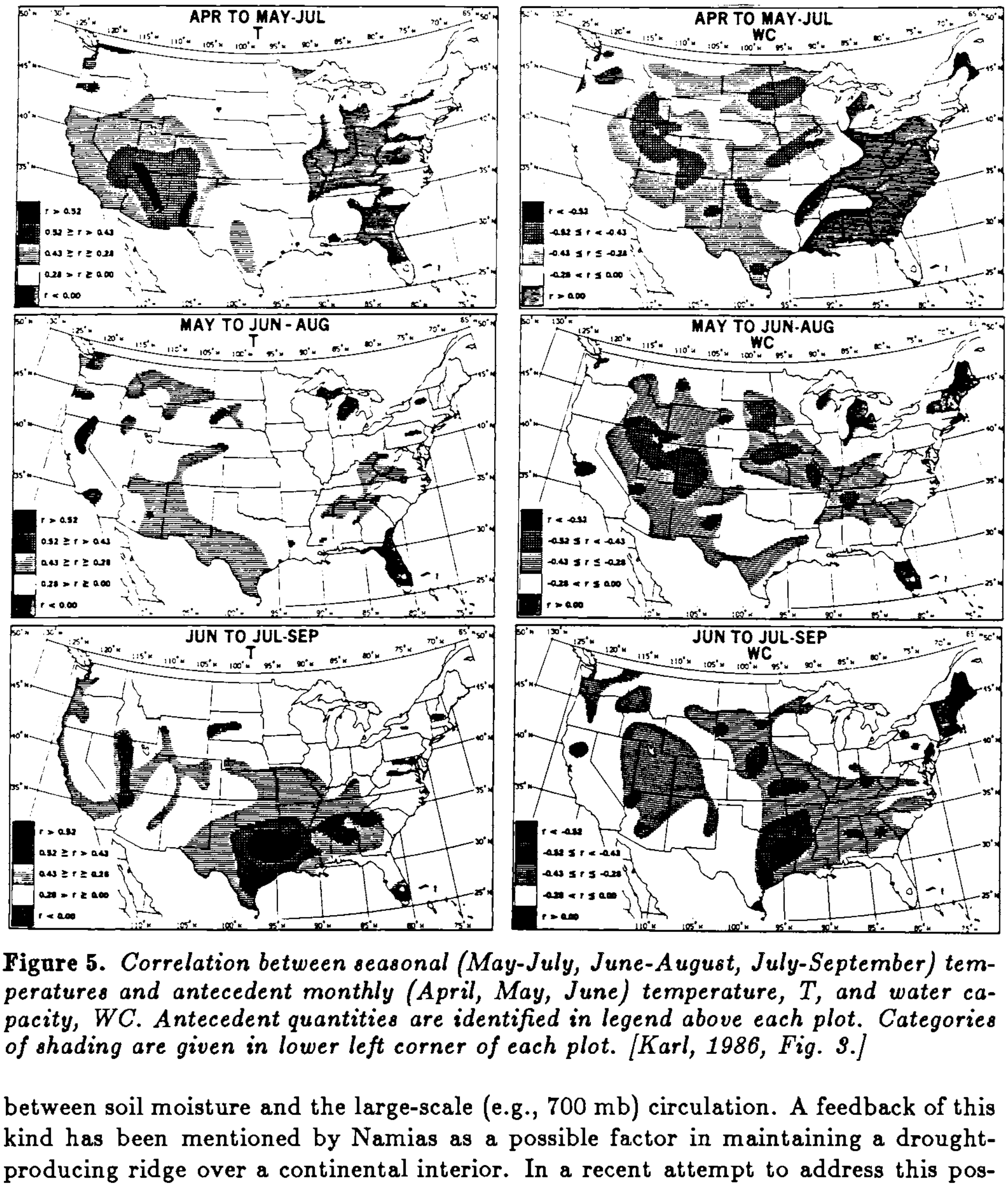

Figure 5. *Correlation between seasonal (May-July, June-August, July-September) temperatures and antecedent monthly (April, May, June) temperature, T, and water capacity, WC. Antecedent quantities are identified in legend above each plot. Categories of shading are given in lower left corner of each plot. [Karl, 1986, Fig. 3.]*

between soil moisture and the large-scale (e.g., 700 mb) circulation. A feedback of this kind has been mentioned by Namias as a possible factor in maintaining a drought-producing ridge over a continental interior. In a recent attempt to address this possibility quantitatively, van den Dool et al. (1986) have decomposed time series of monthly temperature anomalies for the same set of stations shown in Fig. 6:

$$T'(m, y, s) = T_c(m, y, s) + T_r(m, y, s). \tag{1}$$

where T' is the normalized departure from the detrended mean temperature for calendar month m (=1,...,12) in year y (=1947,...,1980) at station s, T_c is the 700 mb-derived specification of T', and T_r is the residual or the error in the specification. The anomaly pattern autocorrelation at lag τ can then be written as

$$PC(m, \tau) = \frac{1}{34}\sum_{y=1}^{34}\frac{1}{61}\sum_{s=1}^{6} T'(m, y, s)T'(m+\tau, y, s)$$

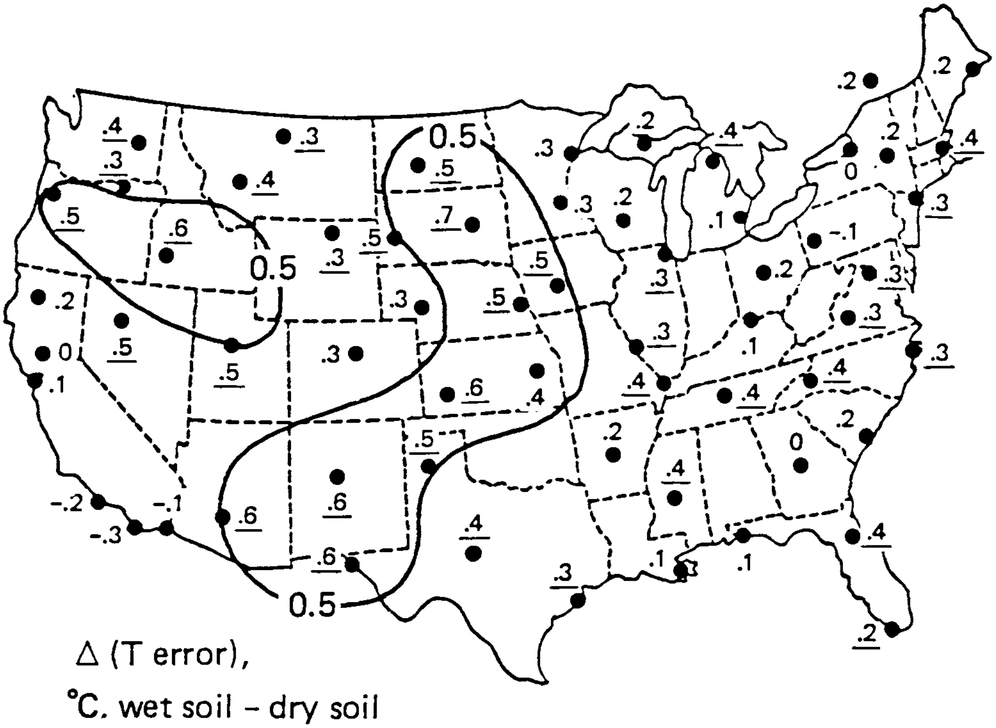

Figure 6. *Composite differences of errors in objectively specified air temperatures (° C) during summer months (June, July, August) when soil moisture was above- and below-normal; signs correspond to months with above-normal soil moisture; underlined values are statistically significant at 95% confidence level.*

or using (1),

$$PC(m,\tau) = \frac{1}{34}\frac{1}{61}\sum_{y=1}^{34}\sum_{s=1}^{61}[T_c(m)T_c(m+\tau) + T_c(m)T_r(m+\tau)$$

$$+T_r(m)T_c(m+\tau) + T_r(m)T_r(m+\tau)]. \tag{2}$$

where each T_c and T_r is also a function of y and s. The term T_cT_c in (2) represents the contribution to the pattern autocorrelation from the persistence of the large-scale circulation, while the second term results from the correlation between the circulation-derived anomaly and a subsequent residual. The third and fourth terms, which represent correlations between the antecedent residual anomaly and *subsequent* circulation-derived and residual anomalies, respectively, are the measures of possible feedbacks involving surface influences (soil moisture, snow cover). As shown in Fig. 7, the total pattern correlation, PC, contains a winter maximum and a summer maximum. While the winter maximum is attributable to the components T_{cr} and T_{cc} involving the antecedent circulation (i.e., T_c leading), the summer peak results from the components T_{rr} and T_{rc} depicting correlations with the antecedent *residual* anomaly. Since soil moisture is apparently a source of summer residuals (Fig. 6), the results are consistent with a feedback–albeit very weak–of soil moisture anomalies onto subsequent surface temperature anomalies and onto the 700 mb circulation. It should be noted that the pattern correlations were based on data for the entire United States, thereby diluting the possibly stronger regional associations between T_r and subsequent anomalies. Moreover, because only certain circulation regimes may be conducive to soil moisture feedback, the use of data for all years (1947-1980) in obtaining Fig. 7 is likely to have diluted even further the statistical signal.

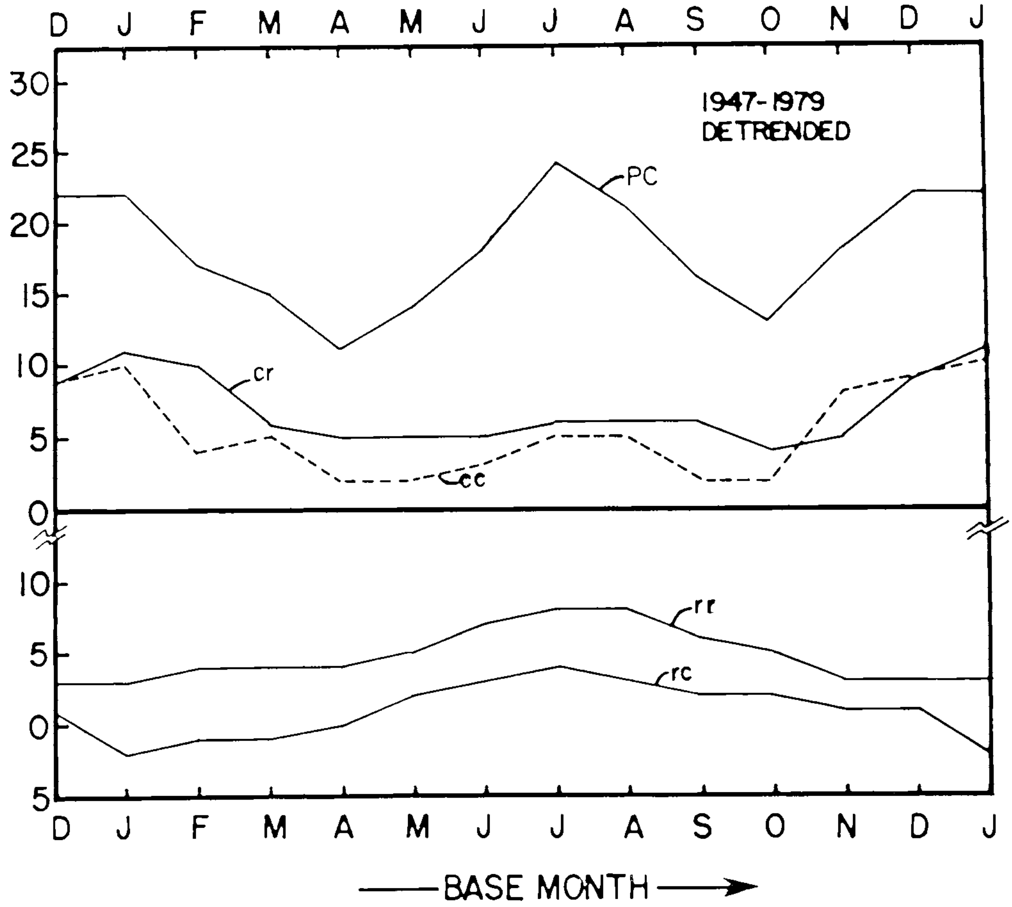

Figure 7. *The decomposition of the pattern correlation (PC) of monthly mean surface air temperatures over the United States. The cc, cr, rc and cr terms represent lagged products of circulation-specified and residual anomalies (see text). All values are percentages.*

Conclusion

The focus of this review has been the detection by empirical techniques of climatic signals or feedbacks involving snow cover and soil moisture. Numerical modeling, which has not been addressed here, offers attractive possibilities for controlled experiments with prescribed surface anomalies over land areas. However, model experiments will require the parameterization of essential quantities such as the albedo of snow cover and the evaporative fluxes involving soil moisture. In view of the uncertainties in these quantities, model experiments with surface boundary forcing will most likely have to proceed hand-in-hand with the types of empirical analyses summarized in the previous sections. The work of Namias is therefore likely to serve as a benchmark well into the foreseeable future.

Among the other topics not addressed here is the possible interaction between various types of surface anomalies. Snow cover, for example, may contribute to *subsequent* anomalies of soil moisture in a manner that is exploitable in diagnostic and predictive studies. Namias (1957) suggested such an association with regard to the climatic anomalies over Scandinavia during the 1950's. Model simulations have also indicated that the link between snow cover and soil moisture is potentially important for summer climatic regimes over the continents (Yeh et al., 1983), especially in the context of possible future climatic changes induced by CO_2.

Finally, many of the more recent studies cited in Sections 2 and 3 are based on analyses in which large samples of cases from all available years are "pooled" or composited. The signals of snow cover and soil moisture are generally weaker in these composites than in the specific cases that have been effectively illustrated by Namias. The climatic roles of land surface variables are likely to depend not only on region and season, but also on the regime of the large-scale circulation. An understanding of the regime-dependencies will be essential if diagnostic and predictive efforts are to take full advantage of the expanding databases on land surface variations. Namias' success in identifying specific regimes susceptible to land surface influences will likely aid future attempts to treat systematically the challenges posed by regime-dependence.

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The Influence of Soil Moisture on Circulations over North America on Short Time Scales

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National Center for Atmospheric Research

Three-day numerical simulations using a mesoscale model are run over the North American continent during summer. The model is initialized with an atmosphere at rest in order to isolate regional circulations generated by variations in topography, moisture availability, albedo, and other surface parameters. After three days, a monsoon-like circulation is generated with maximum low level winds exceeding 10 ms^{-1}. *Moisture availability is shown to be a very important parameter in generating circulation on these time and space scales.*

Introduction

More than 25 years ago, Namias hypothesized that unusually dry springs in the Southern Plains of the United States would favor a drier-than-average summer. The proposed mechanism can best be described in Namias' own words:

"Now when the Southern Plains have been dominated by a very dry regime (which is customarily warm) and the soil is desiccated, it would seem that the opportunity for persistent lodgement of the upper level anticyclone in early summer would be favored, for the area may assume characteristics akin to the deserts overlying which upper level anticyclones are found. On the other hand, following a wet spring some of the heat normally used to raise the temperature of the ground surface might be used to evaporate the excess water in and on the soil, and thus not be available for the sensible heating of the air perhaps necessary to sustain the upper level anticyclone." (Namias, 1960).

Namias presented empirical evidence which showed that cold, wet springs tended to be followed by colder–than–average summers, while warm, dry springs were generally followed by warmer–than–average summers (Table 1). While highly suggestive, the data shown in Table 1 could be a result of other factors unrelated to soil moisture (such as persistence of upper–air flow anomalies from the spring into the summer due to other causes). Namias recognized that various hypotheses were possible and foresaw the time when dynamical models would be available to test the alternatives:

"Accepting for the moment that the influence is geographically fixed, one might inquire what might have fixed it. Here one can speculate ad infinitum, but until dynamic computational methods are developed it will be impossible to test the validity of any hypothesis. The author submits that when such dynamic models are available, it might be possible to test the idea that in such cases as the Texas regime, the early spring heavy rains and the resulting moist soil have served as a cooling reservoir by using for vaporization some of the heat normally associated with the spring to summer building of the upper level anticyclone in that area." (Namias, 1959).

Spring Temperature	Spring Precipitation	Summer Temperature		
		Cold	Normal	Warm
Cold (211)		*101*	*70*	*40*
	Light	29	21	10
	Moderate	31	18	19
	Heavy	41	31	11
Normal (208)		*53*	*74*	*81*
	Light	12	18	34
	Moderate	18	33	27
	Heavy	23	23	19
Warm (209)		*57*	*65*	*87*
	Light	9	27	50
	Moderate	18	22	22
	Heavy	30	16	16

Table 1. *Summer temperatures classes over the Western Great Plains following combinations of spring temperatures and precipitation (Namias, 1960). The number of classes is determined from data from nine states in the Great Plains for periods of records ranging from 60 to 84 years. For example, of 211 cases of "cold" spring temperatures, 101 of the following summers were "cold," 70 were normal, and 40 were "warm." Of the 101 "cold" summers, 29 of the previous spring classes had "light" precipitation, 31 had "moderate" precipitation, and 41 had "heavy" precipitation.*

As Namias predicted, dynamical models have been developed and have been used to investigate the effect of soil moisture on atmospheric circulations (Mintz, 1984). Most of these studies have been conducted with relatively low-resolution global models (Kurbatkin *et al.*, 1979; Walker and Rowntree, 1977; Shukla and Mintz, 1982; Miyakoda and Strickler, 1981; Rind, 1982; Rowntree and Bolton, 1983). For example, Rind (1982) used a general circulation model with $8^\circ \times 10^\circ$ latitude-longitude resolution to study the influence of the amount of ground moisture present on 1 June over the United States on the climate during the following three months. When the ground moisture was reduced to 25% of the value used in a control simulation, the surface air temperature was higher and the precipitation lower throughout most of the summer.

On much shorter time scales, Benjamin and Carlson (1986) and Benjamin (1986) showed the importance of variations in cloud cover, elevation, and surface characteristics, including moisture availability, in generating meso-α (200–2,000 km) scale perturbations through differential heating at the surface for two cases of severe convective storm development over the central United States. Differences in sea-level pressure and boundary-layer winds between 12-h simulations with and without surface heat fluxes exceeded 4 mb and 7 m s^{-1} in some locations.

This paper examines the role of surface moisture availability in affecting differential surface fluxes of heat over varying topography which produce meso-α and synoptic-scale circulations during the summertime on time scales of 0–3 days over North America. We use a medium-resolution (160-km grid) limited-area model to study quantitatively the production of perturbation circulations by land-sea contrasts and differences in elevation in this region. The model is initialized with no motion and a temperature and moisture sounding typical of summertime conditions over the central United States and integrated for three days. We study the importance of evapotranspiration over land on this scale by running two simulations—one with a land-surface moisture availability parameter M equal to 0 (no evaporation) and another with $M = 0.3$. A practical goal of this study is the determination of the sensitivity of a regional numerical model to evapotranspiration on these time and space scales. In addition, these simulations contribute toward a quantitative evaluation of Namias's hypothesis concerning the importance of soil moisture on atmospheric structure over the North American continent.

Summary of model

The model is based on the one described by Anthes and Warner (1978). The vertical coordinate is $\sigma = (p - p_t)/(p_s - p_t)$, where p is pressure, p_s is surface pressure, and p_t is the constant pressure at the top of the model (100 mb). These simulations use a bulk aerodynamic formulation of the planetary boundary layer (PBL) following Deardorff (1972). For these simulations, there are 11 σ-levels (0.0, 0.1, ..., 1.0) with equal spacing, which gives ten layers of equal thickness at which the temperature, moisture, and wind variables are defined. The horizontal grid contains 46 points in the north–south direction and 61 points in the east–west direction.

In the bulk aerodynamic PBL formulation, the surface fluxes of heat, moisture, and momentum are given by the following equations:

Surface Heat Flux

$$H = \rho_a C_p C_\theta C_u (\theta_g - \theta_a) V, \tag{1}$$

Surface Moisture Flux

$$E = \rho_a C_\theta C_u M (q_s(T_g) - q_a) V, \tag{2}$$

Surface Momentum Flux

$$\tau_s = \rho_a C_u^2 V^2, \tag{3}$$

$$V \equiv V_a + V_c, \tag{4}$$

where ρ is density, θ is potential temperature, V_a is the horizontal wind velocity at the lowest level in the model, q is mixing ratio, C_p is the specific heat at constant pressure, C_u and C_θ are exchange coefficients for heat and momentum, and M is the moisture availability. The subscripts g and a refer to values at the ground and lowest level in the model ($\sigma = 0.95$), respectively. The exchange coefficients C_u and C_θ are functions of a bulk Richardson number, R_{iB}

$$R_{iB} = \frac{g}{\theta_a} \frac{h(\theta_a - \theta_g)}{V^2}, \tag{5}$$

where g is gravity and h is the thickness of the PBL, assumed to be the thickness of the lowest model layer (about 850 m). The quantity V_c represents a subgrid-scale eddy wind speed which is included in convectively unstable conditions, and is given by

$$V_c = K(\theta_g - \theta_a)^{1/2}, \tag{6}$$

where $K \approx 2.0$ m s^{-1} K$^{-1/2}$. This term is important in regions of low wind speeds, which occur over the interior of the continents away from mountains in these simulations. The ground temperature is predicted from a surface energy budget and a slab model (Zhang and Anthes, 1982).

Shortwave radiation and long–wave radiation are considered in the surface energy budget but not in the free atmosphere. These radiative fluxes depend on the model–simulated cloud cover in a parameterization developed by Benjamin and Carlson (1986).

Parameter	Value
Horizontal array size	46 × 61
Number of layers in vertical	10
p_t	100 mb
Δs	160 km
Time step Δt	320 s
Roughness length over land	0.1 m
Albedo over land	0.1
Moisture availability	0.0 or 0.3
Soil conductivity λ	10^{-3} kJ m^{-1} s^{-1} K^{-1}
Soil heat capacity per unit volume C_s	1000 kJ m^{-3} K^{-1}
$C_g = 0.95\left(\frac{\lambda C_s}{2\omega}\right)$	8.3×10^4 J m^{-2} K^{-1}
Angular velocity of earth ω	7.27×10^{-5} s^{-1}

Table 2. *Constants and parameters in model simulations*

The cumulus parameterization and treatment of nonconvective precipitation follow methods developed by Kuo (1974) and Anthes (1977). In the convective parameterization, the total latent heat release is proportional to the vertically integrated moisture convergence; the vertical distribution of the convective heating is specified from a variable profile depending upon the location of cloud top and cloud base. The static stability is checked to ensure that convective heating occurs only when the sounding is convectively unstable. A summary of the constants in the model for these simulations is given in Table 2.

The domain and terrain used for these simulations are shown in Fig. 1. For this horizontal resolution, the maximum elevations are 2419 m over the Rocky Mountains of Colorado and 2055 m over the Mexican Plateau.

Results

Moisture availability equal to 0 over land

In the first simulation, the moisture availability is zero. Over the three–day period, the land is heated more than the water and this heat is transferred to the lower layers of the model. Figure 2 shows a time–height cross section of potential temperature over central Illinois, a location far from the influences of water or mountains. The time section shows a strong diurnal cycle in heating, with a gradual growth of the top of the PBL at the end of each day during the 72–h period. Because of the absence of evapotranspiration, the PBL depth exceeds 700 mb (about 3 km) which is typical for dry regions. Over water, the temperature of the surface is constant and the temperature in the PBL changes only slightly from the initial conditions (not shown).

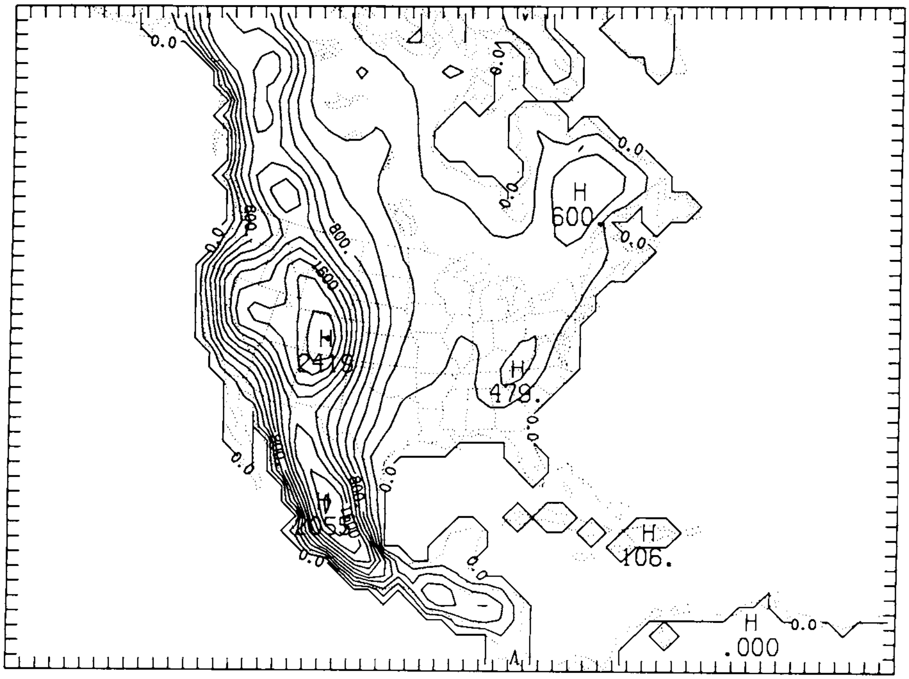

Figure 1. *Terrain elevations over domain of model integrations. Heights over Greenland and South America are set equal to a small value because of their nearness to the lateral boundary.*

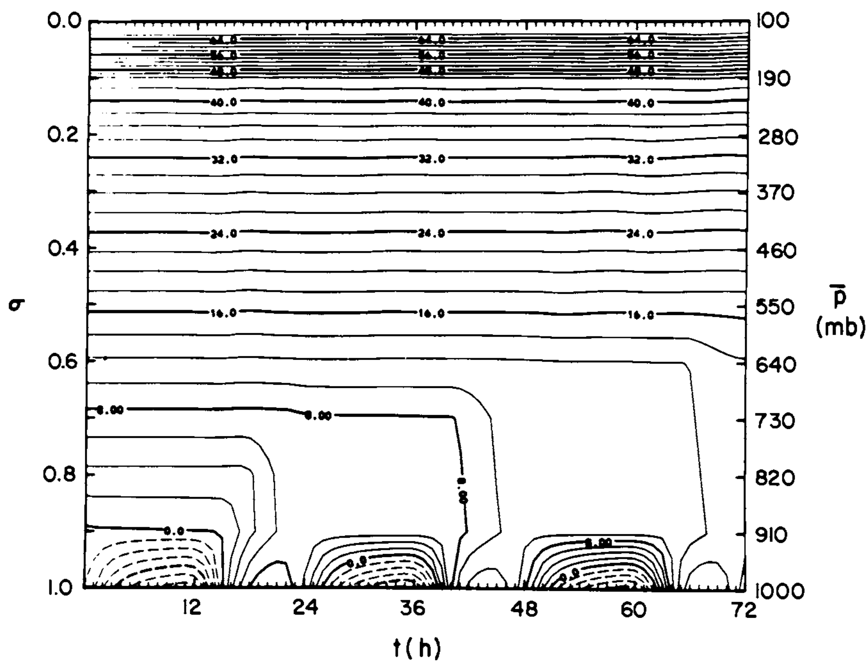

Figure 2. *Time–height cross section of potential temperature minus 300 K over the three-day period at a grid point in central Illinois for Exp. 1.*

	M=0	M=0.3
Δ SLP (mb) (Ocean=1014.3)		
Illinois	-5.3	-1.8
Colorado	-8.3	-3.3
Mexico	-7.3	-4.3
$T(\sigma = 0.95)$		
Ocean	25.0	25.0
Illinois	34.0	28.5
Colorado (Min)	20.6	15.6
Mexico (Min)	14.9	14.5
$q(\sigma = 0.95)$		
Ocean (Avg)	17.0	17.0
Illinois	7.0	12.0
Colorado (Min)	4.0	8.3
Precip Water (cm)		
Ocean (Avg)	4.4	4.4
Illinois	3.0	4.5
Colorado (Min)	1.5	2.2
$V(\sigma = 0.95)$ (m/s)		
North Carolina Coast	11.3	5.8
Gulf Coast	9.0	4.4
Mexico	13.3	11.0
Colorado	4.5	7.0
$V(\sigma = 0.45)$ (m/s)		
North Carolina Coast	4.8	1.5
Gulf Coast	4.8	1.0
Mexico	11.6	13.3
Colorado	11.2	10.4
Rainfall Maximum (cm)		
Florida	0.02	0.07
Hispaniola	0.14	0.25
Mexico	5.04	7.23
Colorado	0.31	0.94
Max 500 mb Height Anomaly (m) (Ref. = 5848 m)	65.0	36.0

Table 3. *Summary of experiments at 72 h*

The result of the heating of the lower troposphere over land is an upward expansion of the isobaric surfaces, generation of upper tropospheric divergence, and a decrease of surface pressure over land (Fig. 3). The maximum pressure decrease is about 7 mb. The decrease in pressure over land results in a low-level pressure gradient directed from water to land and a "macro sea breeze," consisting of an onshore flow with maximum magnitude about 10 m s^{-1} (Figs. 3 and 4 and Table 3). The convergence along the coasts associated with this macro sea breeze produces upward vertical motions with typical magnitudes 4 μb s^{-1} (not shown). Convergence also occurs along the ridge of high elevation in western North America (Fig. 4). On the largest scales, the low-level flow shows cyclonic inflow over the continent. This low-pressure system and the associated cyclonic circulation are similar to the monsoon circulation appearing in the composite July analysis of Tang and Reiter (1984).

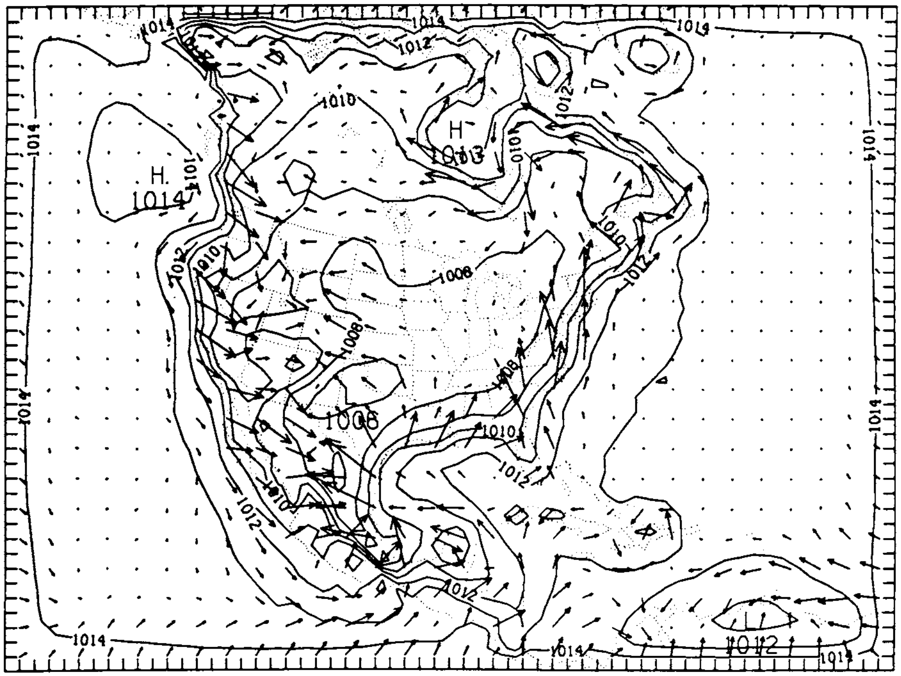

Figure 3. *Sea-level pressure (solid lines, contour interval 1 mb) and wind vectors (maximum length=13.4 m s^{-1}) in the boundary layer (σ = 0.95) at 72 h of Exp. 1.*

This simulation shows strong PBL winds directed into the domain where land intersects the lateral boundary. These winds are generated by the strong pressure gradient, which in turn is an artificial result of holding the values of temperature and surface pressure constant on the boundaries. These strong winds do not occur where water intersects the boundary, because temperature and pressure changes near the boundary are small over water. Fictitious winds near the boundaries of similar magnitude can also be generated in real-data forecasts if the changes in model temperature and pressures near the boundary by the physics of the model are not compatible with the specified lateral boundary conditions.

Figure 5 shows the 500-mb height field at 72 h. The heating over the three days has produced an increase in the average surface to 500-mb temperature over the continents, resulting in 500-mb heights more than 60 m higher than over the oceans. While the low-level circulation associated with the differential heating in the case of elevated terrain is characterized by cyclonic inflow with maximum convergence over the highest terrain, the upper tropospheric flow is dominated by divergent anticyclonic flow (Fig. 6). The total circulation represents the North American monsoon circulation.

The lack of evaporation over land, together with the lower tropospheric warming, results in a rapid decrease of relative humidity in the boundary layer at the end of

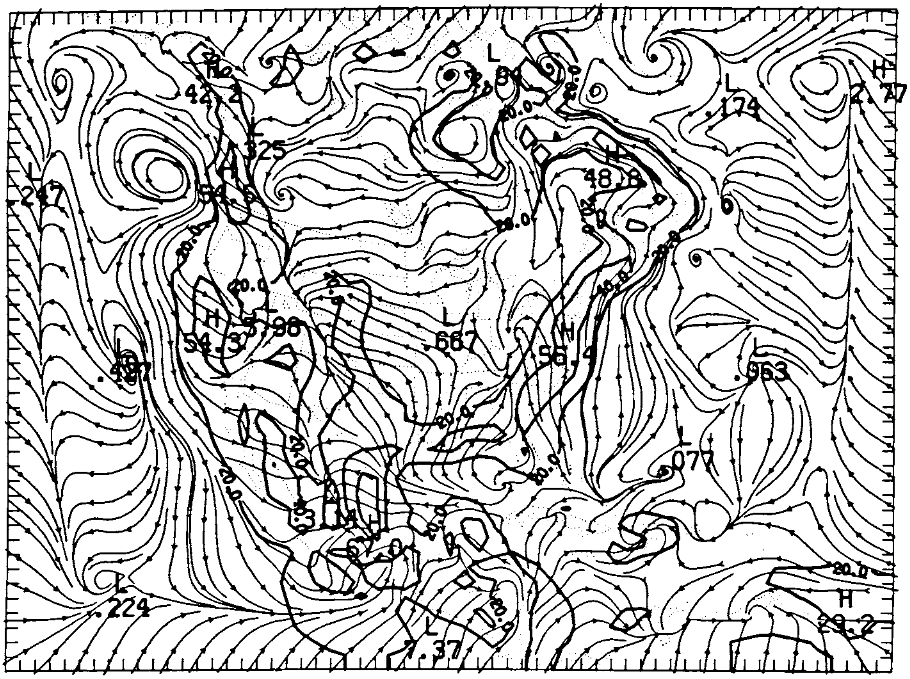

Figure 4. *Streamlines (thin lines) and isotachs (heavy lines in m s^{-1} times 5) in the boundary layer (σ = 0.95) at 72 h of Exp. 1.*

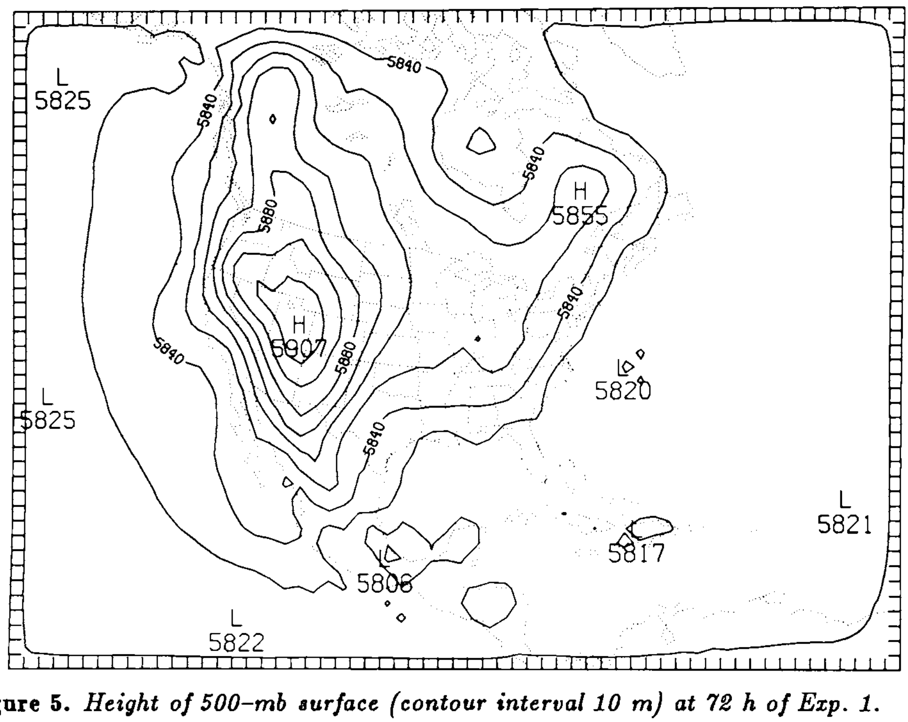

Figure 5. *Height of 500–mb surface (contour interval 10 m) at 72 h of Exp. 1.*

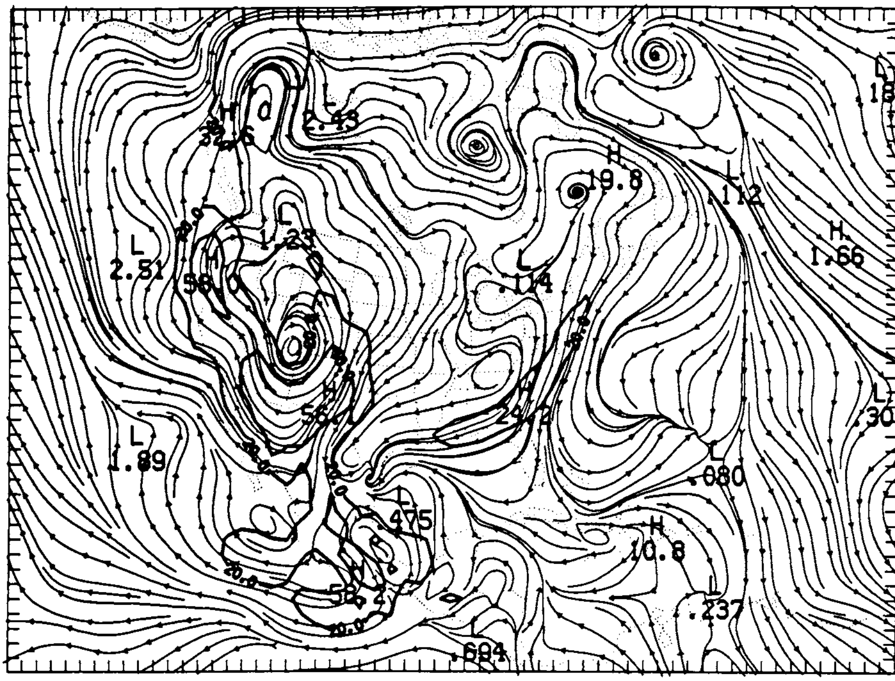

Figure 6. *Streamlines (thin lines) and isotachs (heavy lines in m s^{-1} times 5) at σ = 0.45 at 72 h of Exp. 1.*

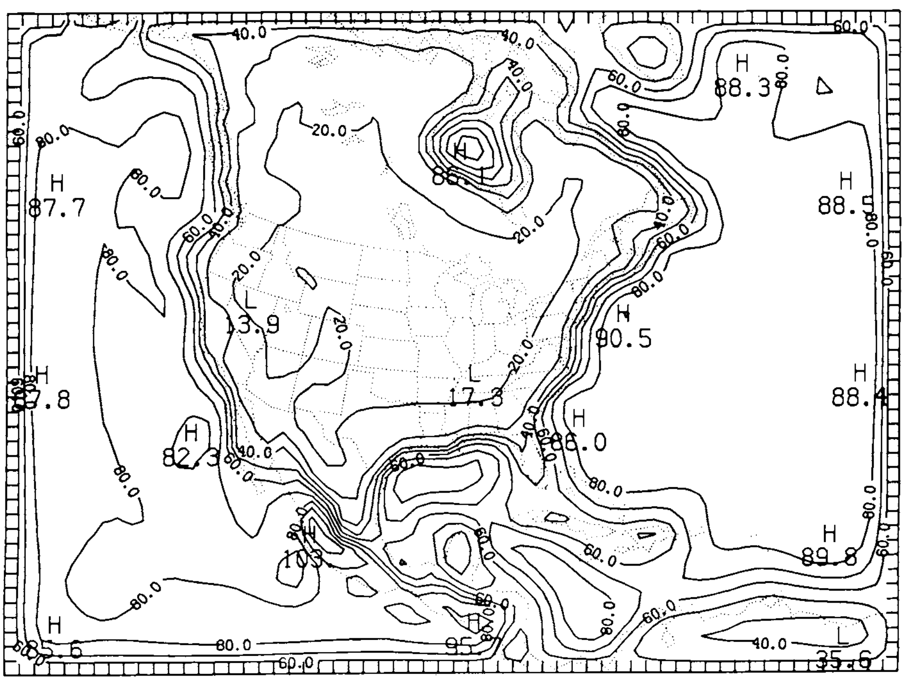

Figure 7. *Relative humidity (contour interval 10%) in boundary layer (σ = 0.95) at 72 h of Exp. 1.*

three days (Fig. 7). Relative humidities have decreased from an initial value of 50% everywhere to less than 20% over the interior parts of the continents. In contrast, evaporation over the oceans has produced an increase of boundary–layer relative humidity to over 80%. Horizontal transport of water vapor from the oceans to the land associated with the sea breeze is capable of moistening only a narrow band inland along the coasts.

The rapid drying of the PBL in only three days when evaporation is suppressed shows the importance of local evapotranspiration during the summer when horizontal transport is weak. Upward mixing of water vapor through the boundary layer can rapidly produce a sounding very unfavorable for moist convection. Without convective rainfall, surface moisture availability and evaporation would remain low, further inhibiting convection. This positive feedback between convective rainfall and surface moisture availability is another possible explanation for Namias's observations of the persistence of droughts in the central United States.

In spite of the lack of evaporation, strong low–level convergence over the mountains, and to a lesser extent along the coasts and over the Florida peninsula, produces local regions of convective precipitation (Table 3). Amounts are less than 0.5 cm except over the narrow highlands of Mexico, where more than 5 cm occurs.

Moisture availability equal to 0.3 over land

In sensitivity studies with a one–dimensional model of the planetary boundary layer, Zhang and Anthes (1982) found that the most important parameter in determining the surface fluxes of sensible and latent heat was the moisture availability. Experiment 2 is identical to Exp. 1 except that the moisture availability over land is 0.3 rather than 0.0. A value of M equal to 0.3 is a relatively moderate value; for example, Carlson *et al.* (1981) estimated typical summertime values of M over St. Louis, Missouri, to be 0.5, while Nappo (1975) found good agreement of predictions of boundary–layer structure with observations for values of M in the range 0.5 to 0.7. The addition of evaporation over land causes significant differences at 72 h in the low–level PBL structure. The time–height cross section of potential temperature (Fig. 8) indicates a less rapid growth of the PBL height (compare with Fig. 2). The vertical temperature and moisture sounding over Illinois (Fig. 9) indicates a considerably cooler and more humid boundary layer. On the average, the ground and PBL air temperatures over land are 3–4°C lower with evaporation and the PBL mixing ratio is 4–5 g kg^{-1} higher (Table 3). The relative humidity in the PBL is much higher in Exp. 2 (compare Figs. 10 and 7), with maxima over the high terrain in western North America.

The slightly cooler air over the land reduces the intensity of the heat–induced low–pressure region, with a corresponding reduction in PBL winds (compare Figs. 11 and 3). The pressure reduction is reduced from 5.3 to 1.8 mb over Illinois, 8.3 to 3.3 mb over Colorado, and 7.3 to 4.3 over Mexico (Table 3). Maximum PBL velocities decrease from 11.3 to 5.8 m s^{-1} along the N. Carolina coast, from 9.0 to 4.4 m s^{-1} along the Gulf of Mexico coast, from 13.3 to 11.0 over Mexico, and 14.5 to 7.0 over Colorado (Table 3).

Although the evaporation reduces the land–sea temperature contrasts and the strength of the monsoon circulation slightly, the rainfall increases considerably over the simulations with no evaporation (Table 3). Rain maxima occur over Florida (0.07 cm), Hispaniola (0.25 cm), the Rockies of Colorado (0.94 cm), and Mexico (7.23 cm). The

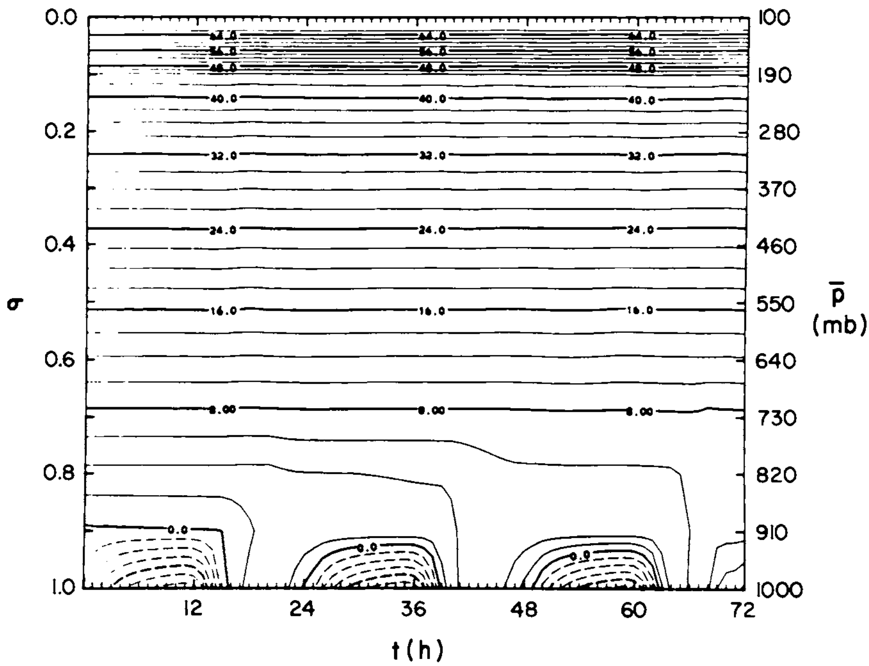

Figure 8. *Same as Fig. 2, except for Exp. 2.*

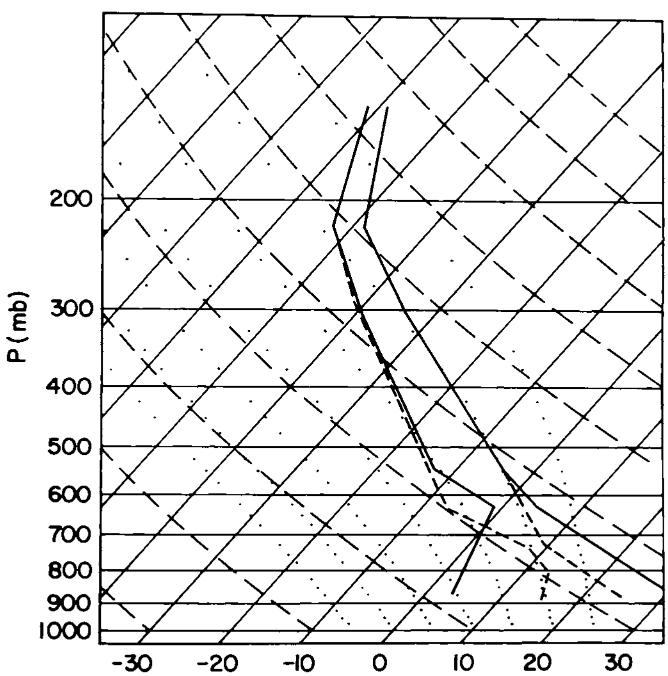

Figure 9. *Temperature and dewpoint soundings (solid lines) at 72 h for Exp. 1 and for Exp. 2 (dashed lines). The straight lines sloping upward from left to right are isotherms; the curved dashed lines are isentropes, and the dotted lines are wet adiabats.*

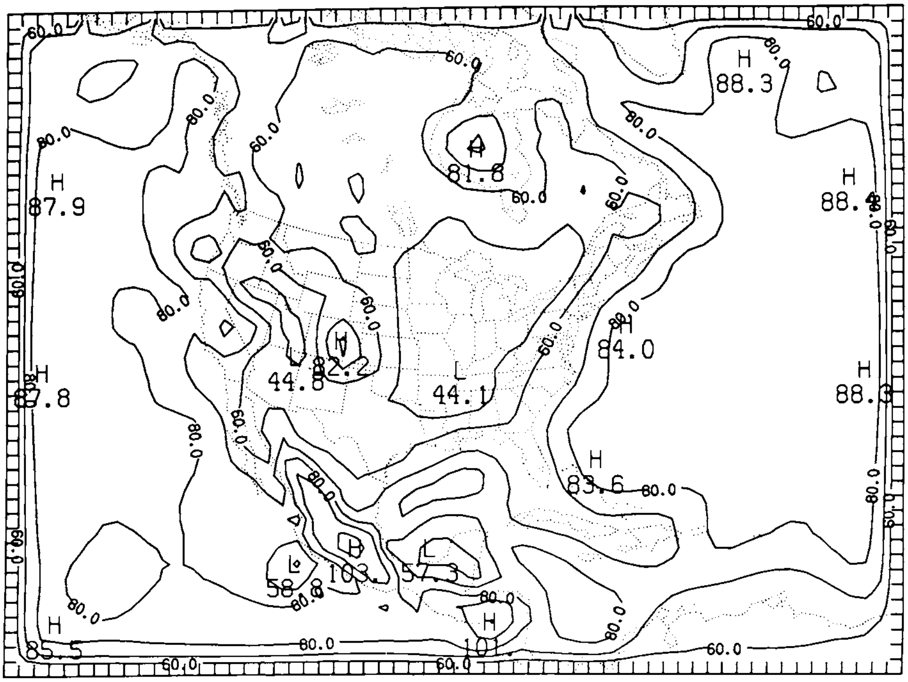

Figure 10. *Same as Fig. 7, except for Exp. 2.*

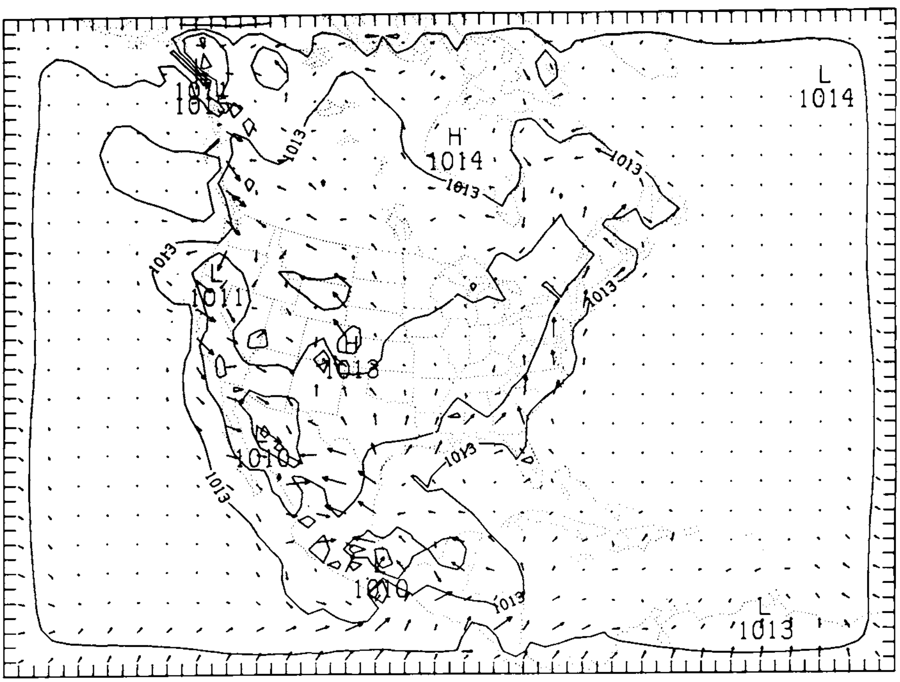

Figure 11. *Same as Fig. 8, except for Exp. 2.*

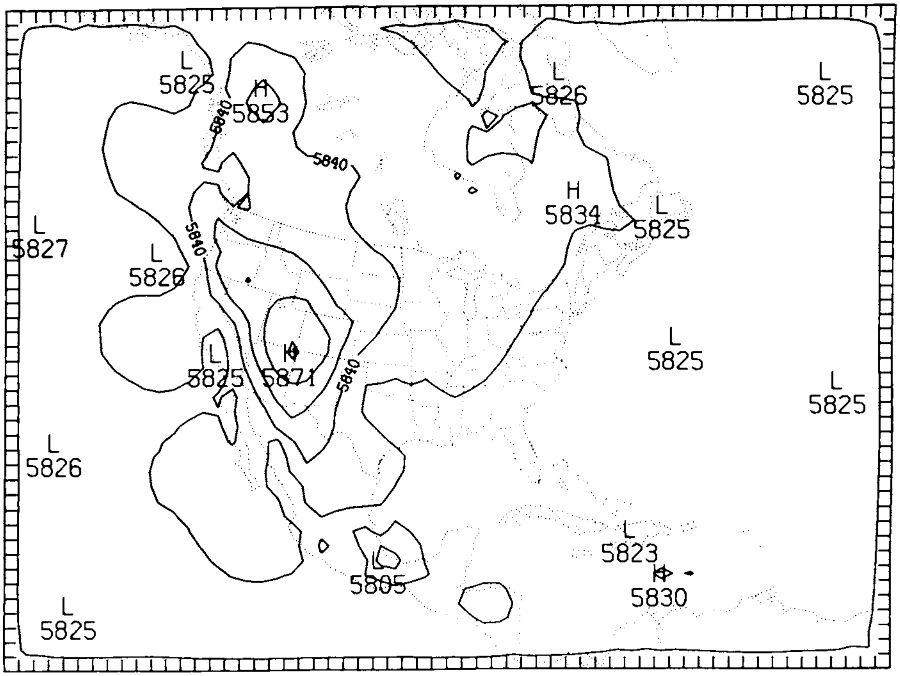

Figure 12. *Same as Fig. 5, except for Exp. 2.*

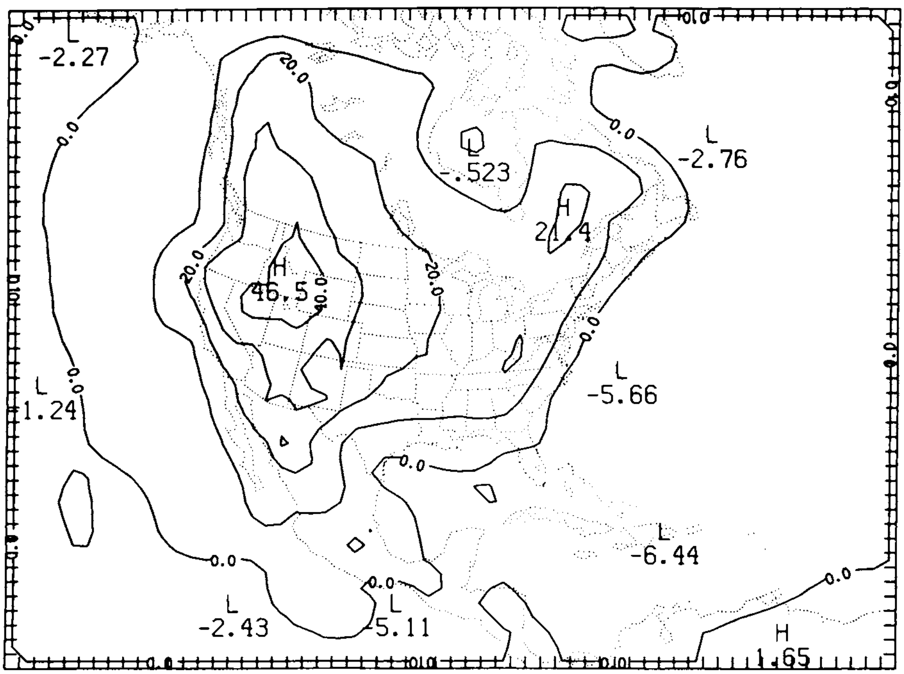

Figure 13. *Difference in height (m) of 500-mb surface between Exp. 1 (dry) and Exp. 2 (moist).*

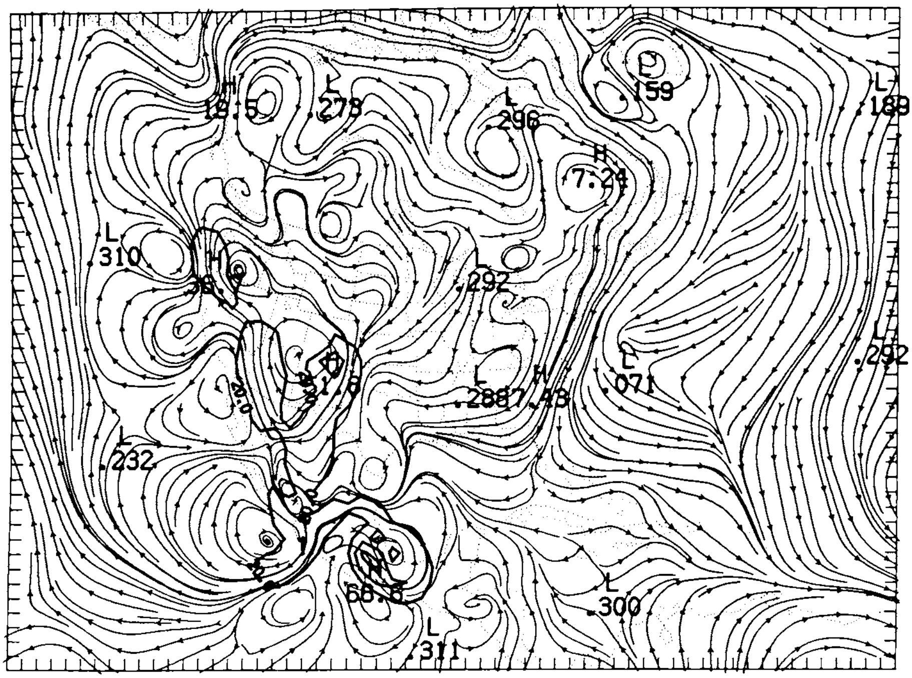

Figure 14. *Same as Fig. 6, except for Exp. 2.*

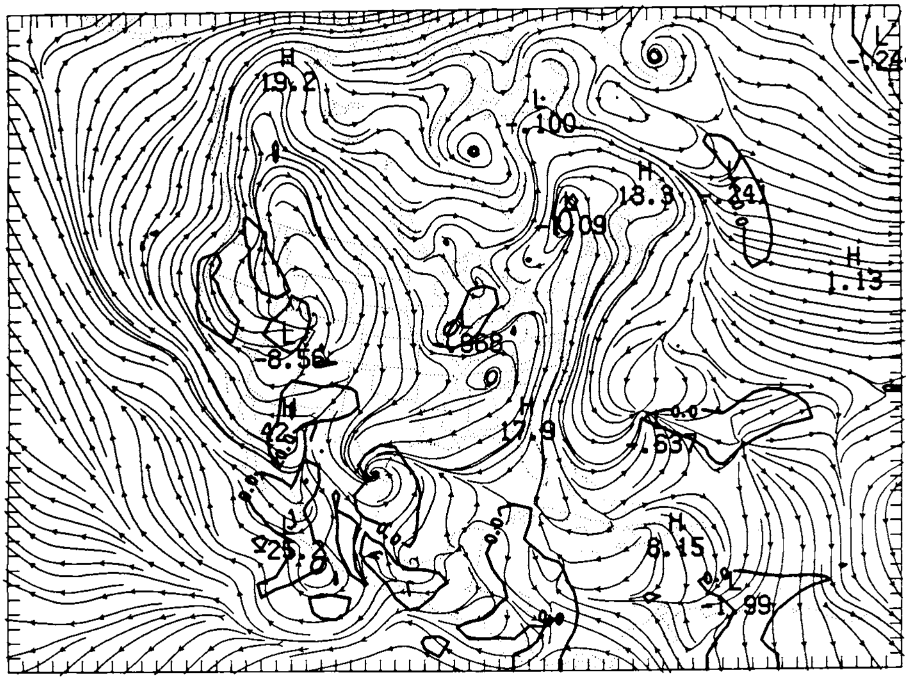

Figure 15. *Difference in circulation at $\sigma = 0.45$ at 72 h between Exp. 1 (dry) and Exp. 2 (moist). Heavy lines are isotachs of differences in m s^{-1}, multiplied by 5.*

convective rainfall maximum over the high terrain of Colorado and New Mexico, with a sharp decrease toward the east, compares favorably with the average midday thunderstorm activity as revealed by the composite GOES satellite infrared imagery for August 1982 at 2100 GMT (Fig. 4 of Tang and Reiter, 1984).

In agreement with Namias's hypothesis summarized in section 1, the upper–level anticyclone is reduced significantly in the experiment with evaporation compared to that without evaporation. Although an upper–level anticyclone still develops in Exp. 2 (Fig. 12), its intensity is considerably weaker. Figure 13 shows the difference in 500–mb heights between the dry and moist simulations. The maximum difference of 46.5 m occurs over Idaho, with 500–mb heights greater in the dry simulation over most of the continent. This reduction in intensity of the upper–level high is accompanied by a corresponding weakening of the upper–level anticyclone (Fig. 14). The difference in intensity is seen in Fig. 15, which shows the difference in the winds at $\sigma = 0.45$ at 72 h between the dry and moist simulations. Figure 15 indicates a much stronger anticyclonic circulation over the continent in the dry simulation.

Summary

Two 72–h simulations over North America in summer, initialized with a horizontally homogeneous atmosphere at rest, indicate the importance of evapotranspiration from the surface on the dynamics of the circulation on this scale. In the first experiment, the moisture availability is set to zero over land and no evaporation occurs. In the second simulation, the moisture availability over land is assigned a uniform value of 0.3.

In both simulations, a large–scale circulation resembling the North American monsoon develops after three days, with low pressure over the continent at the surface and high pressure aloft. The wind circulation consists of generally cyclonic inflow at low levels from the ocean to the continent and anticyclonic outflow aloft. The greatest wind speeds occur near the coasts and in regions of strong terrain gradients, particularly in western North America.

The monsoon circulation is considerably stronger in the simulation without evaporation, as all of the incoming solar radiation is used to heat the air rather than evaporate water. The maximum low–level pressure departure over land and the upper–tropospheric height anomalies are about two times greater without evaporation. This result is consistent with Namias's hypothesis that a persistent, stronger–than–normal upper–level anticyclone over the Great Plains during the summer is favored by abnormally dry soil conditions in this region during the spring.

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Some SST Anomalies I Have Known, Thanks to J. Namias

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Jerome Namias' investigations into the causes and effects of large scale SST anomalies had an early and lasting influence on the direction and scope of much of my research in ocean modeling. In this presentation, I will summarize some of that research, and I will describe the significant influence that Jerome Namias had on all of it.

Introduction

I first met Jerome Namias (Jerry) when I came to work as a student trainee at the United States Weather Bureau (USWB) in June, 1960. As an undergraduate majoring in mathematics but very much interested in the weather, I had the extreme good fortune of "working" for four consecutive summers in the USWB's Extended Forecast Branch, where Namias was the Chief. No job could have been more valuable or enjoyable to an aspiring Meteorologist than the one I had under Jerome Namias. Indeed, it was my daily pleasure to observe the top professional Meteorologists in our country struggling to make long range weather forecasts for 5 and 30 days in advance. The present Chief, Don Gilman, who was also there during Jerry's tenure, has testified that the top professional Meteorologists of this day are still struggling with the problems of long-range forecasting (Gilman, 1985). Of course, today's challenges are different from those of 25 years ago; seasonal forecasts are now made by the Climate Analysis Center and others, and there is an expressed need for even longer range forecasts.

The one thing that impressed me the most during my student trainee days under Jerry Namias was the 5-and 30-day extended forecast "post-mortems". These were the post-forecast group sessions during which the latest long-range forecast was thoroughly analyzed, interpreted and carefully verified. Often, in the 30-day forecast post-mortems, the culprit for a missed forecast was a sea-surface temperature (SST) anomaly. Less frequently, and only in the case of an especially good forecast, was an SST anomaly considered a "hero". I am now convinced that it was during those occasionally tumultuous post-mortems that Jerry Namias discovered that the ocean surface temperatures were affecting atmospheric variability on monthly (and certainly longer) time-scales. Jerry Namias, and a large number of enthusiastic atmospheric and oceanic scientists have been working for the past 25 years to identify the specific dynamical and thermodynamical processes by which SST anomalies indeed influence the atmosphere on long time scales. Although considerable progress has been made (Namias, 1985), the job is still far from complete. My own contribution in this area has been very meager. In fact, my research has focused almost entirely on the problem of the generation of midlatitude SST anomalies, themselves, with very little attention devoted to studying the effect that such anomalies have on the atmosphere. In the following sections, I summarize some of the more significant results of these and other related studies. All of the studies, in one way or another, were inspired by the pioneering work in this area by Jerry Namias.

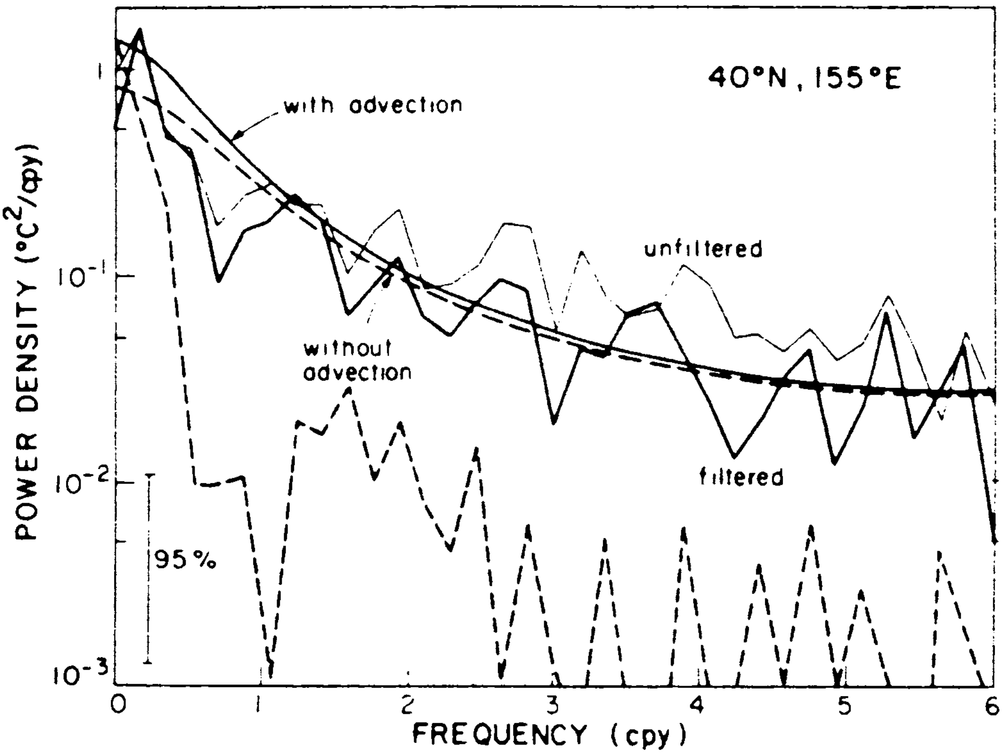

Figure 1. *SST anomaly spectrum at 40° N and 155° E (heavy line labeled "filtered") and model fits with advection (heavy line labeled "with advection") and without advection (upper dashed line). The lower dashed line shows the contribution of the advection terms. The SST spectrum before EOF filtering is also shown (light line labeled "unfiltered"). From Frankignoul and Reynolds (1983).*

Midlatitude SST Anomalies - a Zeroth-Order Theory

The mechanisms responsible for the formation and evolution of midlatitude SST anomalies have been studied extensively during the last two decades. As emphasized recently by Barnett (1981) and reviewed by Frankignoul (1985), the problem of the creation of large-scale SST anomalies is basically the problem of closing the heat budget of the upper ocean. This implies that the processes which influence the net heat flux across the sea surface, the turbulent vertical mixing of heat at the base of the seasonal thermocline, and the lateral advection of heat in the upper ocean, are all potentially important factors in understanding SST anomalies.

The present *observational* evidence suggests that midlatitude SST anomalies are generated by variations in the net surface heat flux on relatively short time scales, and that they are damped by heat flux variations on relatively long time scales. The time scale that separates "long" from "short" in this context is not clearly identified, but

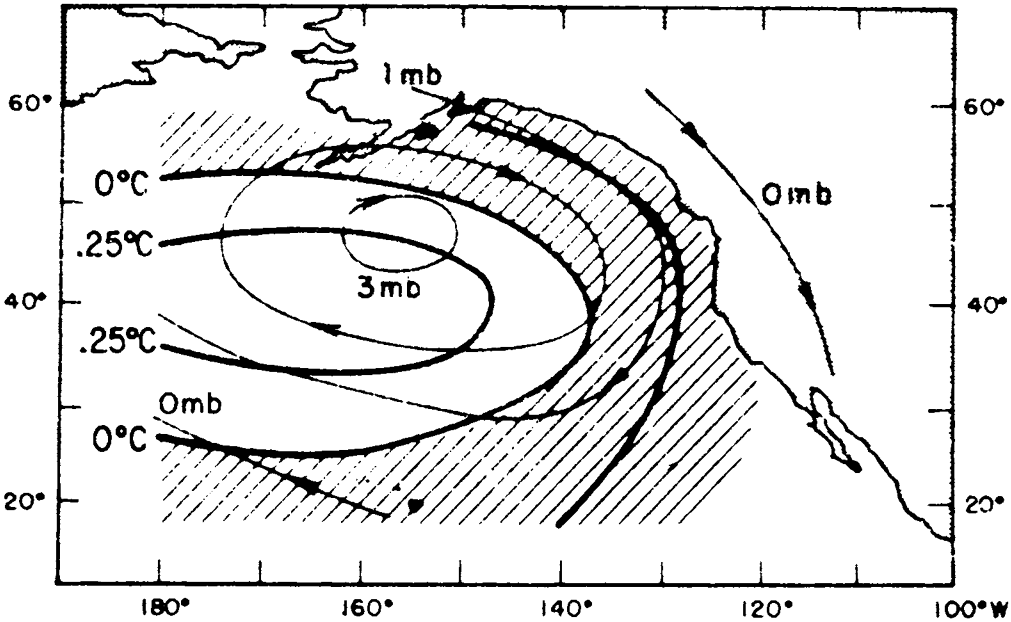

Figure 2. *Principal patterns for seasonal (three-month average) SLP anomalies predicting monthly SST anomaly one month after center month of SLP season. Principal patterns are based on year-round statistics. The SLP data pattern is responsible for 50% of the hindcast skill achievable from five SLP EOF's. From Davis (1978).*

it seems to be on the order of several weeks. The fact that the atmosphere forces the ocean on short time scales and damps it on long time scales was strongly suggested, perhaps for the first time, by the observational study of Kraus and Morrison (1966). They analyzed ocean weather ship data from the North Atlantic and North Pacific Oceans, and they found that on time scales shorter than a month the variance of air-sea temperature difference was primarily due to variations in the temperature of the *air*. On time scales greater than a month, the variance was due more to variations in the temperature of the *sea*.

Theoretical evidence also indicates that midlatitude SST anomalies are driven by atmospheric forcing on relatively short time scales and damped on longer time scales. The theory is based on the stochastic forcing models of climate variability (Frankignoul and Hasselmann, 1977; Frankignoul, 1979; Frankignoul and Reynolds, 1983; Herterich and Hasselmann, 1986). In the study by Frankignoul and Reynolds, a non-entraining slab model of the oceanic mixed layer was driven by a prescribed white-noise forcing from the atmosphere and damped by a Newtonian cooling term having a relatively long time scale. By adjusting the amplitude of the white-noise forcing and the time scale of the damping, Frankignoul and Reynolds were able to reproduce (i.e., to model) the frequency spectrum of observed SST anomalies over most of the midlatitude North Pacific Ocean (Fig. 1). Even more important was the fact that the required amplitude of the white-noise forcing resembled the observed estimates of atmospheric forcing over a large part of the North Pacific Ocean. In addition, the required damping time for the anomalies was of the order of several months. This value is consistent with direct estimates of damping by vertical mixing and feedback to the atmosphere (Frankignoul, 1985). Thus, today's zero order description of ocean-atmosphere interaction in midlatitudes is that of SST anomalies being generated by atmospheric forcing on time scales much less than several months, and being damped on time scales of several months or more. The damping is accomplished by vertical heat exchange with both the atmosphere and the deep ocean.

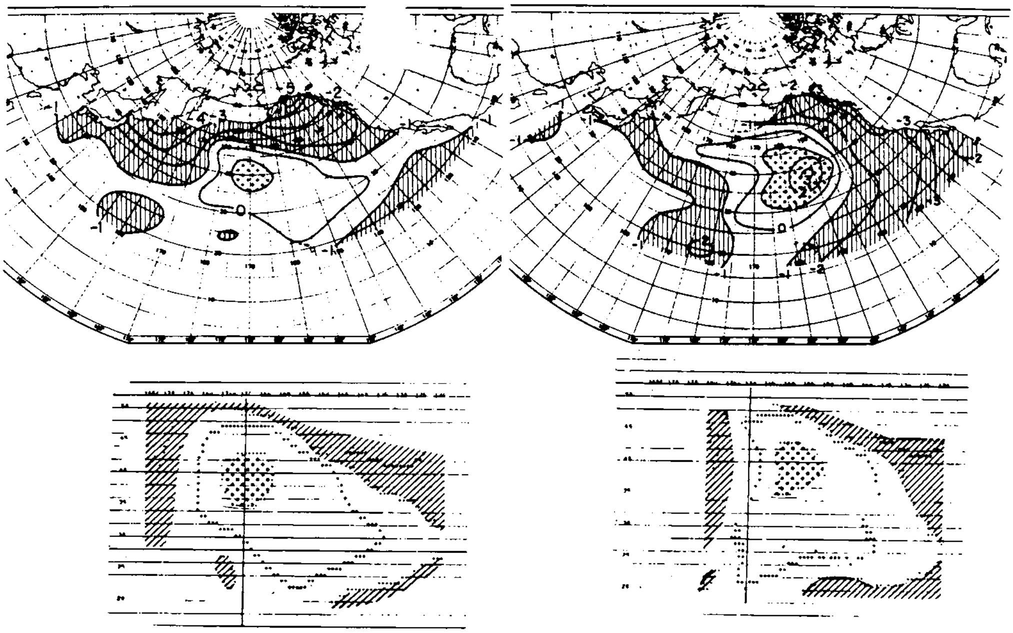

Figure 3. *SST anomalies for fall 1971 (left) and winter 1971-72 (right). The top panels show observed anomalies and the bottom panels show initial (left) and predicted (right) anomalies. Areas less than -1° F are hatched; areas greater than 1° F are stipled. Heavy vertical line in the lower panels marks 170° W. It is lined up with 170° W on the observed maps. From Namias (1972).*

Specific Forcing Mechanisms

While the above results concerning the time scales of ocean forcing and response are fairly well established, the evidence concerning the exact nature of the atmospheric forcing mechanisms that create SST anomalies is much less conclusive. For example, the relative importance of the two major forcing fields, the winds and the surface heat fluxes, is difficult to establish. This is because winds and heat fluxes are highly correlated, and because present estimates of surface heat fluxes on synoptic and monthly time scales are very uncertain (Husby, 1980; Barnett, 1981; Weare and Strub, 1981; Elsberry, *et al.*, 1982; Clancy and Pollak, 1983). Therefore, it is not possible to unambiguously identify the physical reason for the statistically significant lag correlation that Davis (1976; 1978) found between sea level pressure (SLP) and SST anomalies in midlatitudes. The dominant relationship, in which SLP anomalies lead SST anomalies in year-round average statistics, consists of a positive (negative) SLP anomaly south of the Aleutians followed a month later by a positive (negative) SST anomaly southwest of the SLP anomaly center (Fig. 2). The spatial pattern is clearly such that the relationship could either be due to the anomalous winds (through Ekman advection or turbulent vertical mixing) or due to anomalous surface heat fluxes.

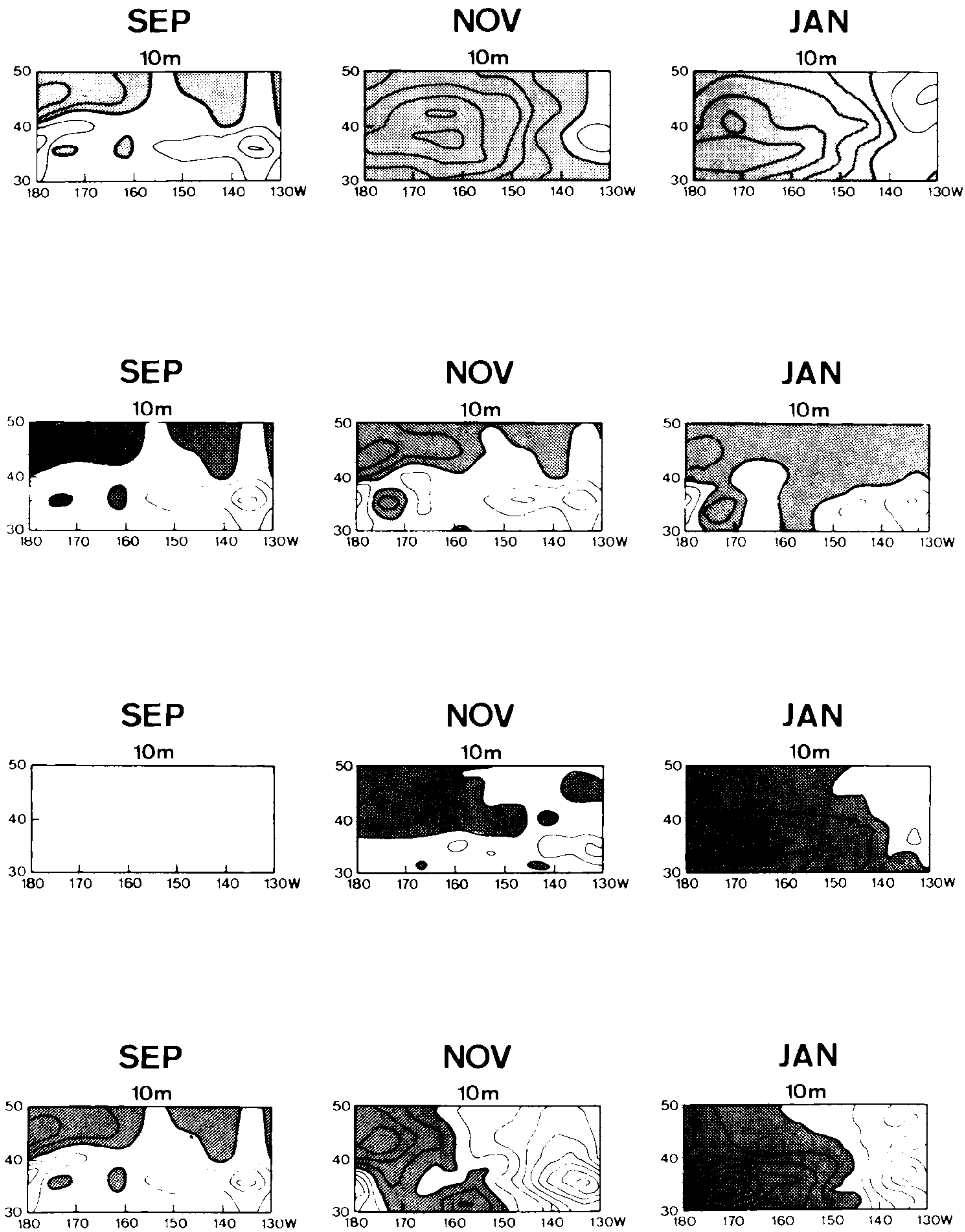

Figure 4. *SST anomalies in the midlatitude North Pacific Ocean during September 1976 (first column), November 1976 (second column) and January 1977 (third column). The top panel is observed; the second panel is predicted using the observed initial conditions but no anomalous atmospheric forcing; the third panel is predicted using only the observed anomalous wind stress forcing; and the bottom panel is predicted using observed initial conditions, anomalous winds, and anomalous surface heat fluxes. The contour interval is 0.5° C. From Haney (1980).*

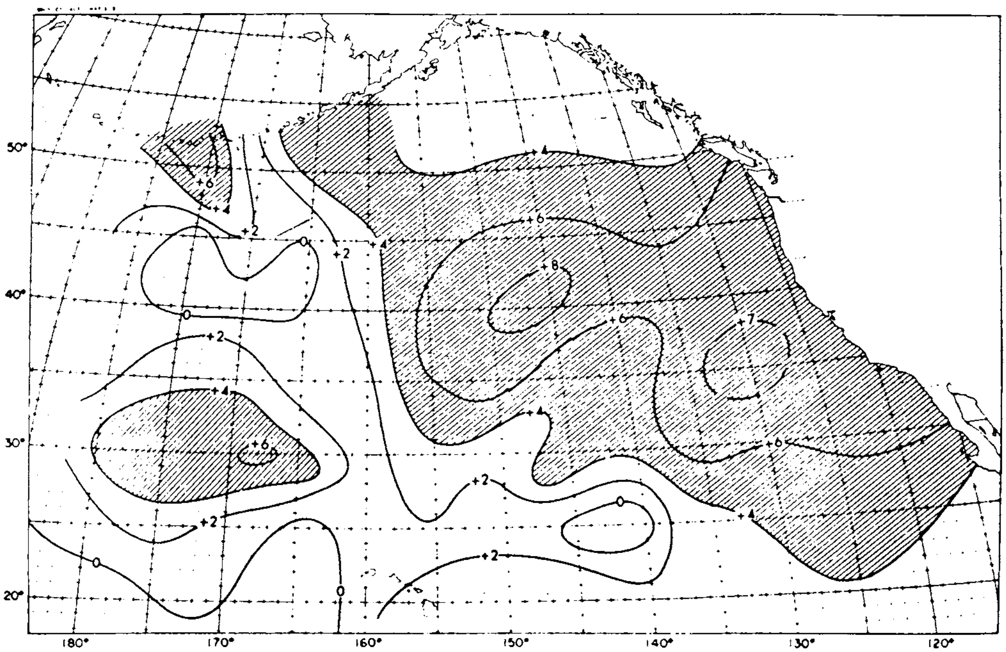

Figure 5. *Isopleths of correlation coefficient between time series of mean monthly computed (using advection of SST anomaly by the mean currents plus advection of mean SST by anomalous (Ekman) currents) and observed SST anomalies over the eastern North Pacific for the two years 1962-63. Shading represents the area reaching the 5% level of significance. From Namias (1965)*

Some success in identifying the separate role of winds and surface heat fluxes in producing midlatitude SST anomalies has been achieved by means of initial value simulations using numerical models (Haney *et al.*, 1978; Huang, 1979; Haney, 1980). However, these studies, like the earlier pioneering case studies by Namias and his colleagues (Jacob, 1967; Namias and Born, 1970; Namias, 1972; Clark, 1972; Daly, 1978), have not unambiguously demonstrated a dominant role for either forcing mechanism. In some of the cases, the surface heat fluxes seemed to dominate, but in other cases the winds alone were sufficient to explain the SST anomaly development. For example, Namias (1972) showed that the SST anomaly behavior in the North Pacific Ocean during the winter of 1971-72 could be explained by the simple horizontal advection of the initial SST anomaly by the climatological ocean surface currents (Fig. 3). This naturally implies that the net effect of anomalous atmospheric forcing was small by comparison. Motivated by Jerry's study, I carried out a numerical model simulation of the fall and winter of 1976-77 in the North Pacific. My results for this winter were just the opposite of what Jerry found for the 1971-72 winter. According to the model simulations (Fig. 4), anomalous wind stress forcing was the most important factor, with anomalous surface heat fluxes also contributing to the SST anomaly development. Because of the very strong atmospheric forcing in this case, the initial SST anomaly was of negligible importance.

Specific forcing mechanisms have been investigated by statistical studies as well. These are observational studies that attempt to demonstrate a statistical relationship between SST anomalies and certain atmospheric and oceanic forcing parameters by averaging over a sufficiently large number of SST anomaly cases. One of the first, and certainly one of the most important, studies of this type was Jerry's investigation of the atmospheric and oceanic anomalies during 1962-1963 (Namias, 1965). The significant result of that study was the high correlation between the observed SST anomalies and those "predicted" using only the two linear horizontal advection terms in the SST anomaly equation (Fig. 5). Looking back at this study some 20 years later, it seems likely that a *partial* explanation for the good correlations may be the forcing of SST anomalies by anomalous surface heat fluxes rather than advection.

In the late 1970s, an interesting controversy developed between the research results of Russ Davis and those of Jerry Namias (Davis, 1976, 1978; Namias, 1976). This controversy, which concerned the statistically established lag relationship between SLP and SST anomalies mentioned above, led me to undertake a somewhat similar observational study of the relationship between *wind* and SST anomalies. The goal was to determine whether midlatitude SST anomalies were caused only by the overlying wind, and if so, what physical processes were involved. The operational 6-hourly surface wind analyses prepared by Fleet Numerical Oceanography Center (FNOC) and the monthly SST anomalies prepared by Jerry Namias were used in the study. Both data sets covered the midlatitude North Pacific Ocean during the 10-year period, 1969-78. Using these data, a cross-correlation analysis was made between monthly anomalies of certain surface wind parameters and month-to-month changes in the SST anomalies. The results were somewhat surprising (Haney, et al., 1983). Changes in SST anomalies were indeed correlated with monthly anomalies of the surface friction velocity cubed, u_*^3, and the surface wind stress curl, curl τ. The correlation with u_*^3 suggests that midlatitude SST anomalies may be caused in part by monthly anomalies of turbulent vertical mixing in the upper ocean due to anomalies in atmospheric storminess. Similarly, the correlations with curl τ indicates that SST anomalies are also affected by large-scale horizontal advection. The effects of Ekman pumping and suction were found to be entirely negligible. Due to the rather long data record, the correlations are statistically significant. However, they are *not* large; typically only 0.25 to 0.35. This implies that although wind and SST anomalies are indeeed statistically related, and although the relationships can be easily and directly attributed to well-known physial processes, these processes are *not* the most important ones for the development of SST anomalies in midlatitudes. If the surface wind by itself was really the dominant mechanism, the correlations would have to be much higher.

A 10-Year Numerical Hindcast

In order to investigate further the specific role of winds as a possible cause of SST anomalies in midlatitudes, I recently carried out a 10-year numerical hindcast using a multi-level primitive equation (PE) ocean circulation model (Haney, 1985). As mentioned above, the separate effects of winds and surface heat fluxes cannot be determined from observations because winds and heat fluxes are known to be correlated, and because basin-wide fields of reliable surface heat fluxes are not presently available. This problem can be avoided, however, and the separate role of winds can be investigated quite easily, by means of a controlled numerical experiment.

The numerical experiment (hindcast) was patterned after those of Busalacchi and O'Brien (1981) and Busalacchi et al. (1983) who used a numerical model of an idealized equatorial ocean basin to study the linear response of the equatorial Pacific Ocean to observed winds. The present hindcast was of the midlatitude North Pacific Ocean, and it covered the 10-year period 1969-78. In order to model realistically the kind of upper

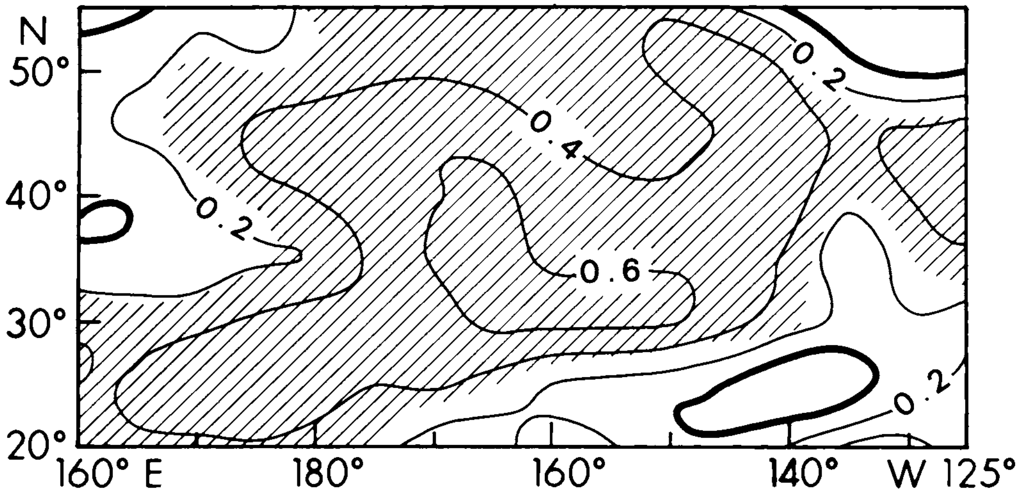

Figure 6. *Isopleths of correlation coefficient between the time series of observed and hindcast SST anomalies over the midlatitude North Pacific for the 10-year period 1969-78. Shading represents the area reaching the 5% level of significance. From Haney (1985).*

ocean thermodynamics that is considered important for midlatitude SST anomaly development, the PE model of Haney (1980), improved by having high vertical resolution and a new parameterization of upper ocean mixing processes, was used. Vertical mixing in the upper ocean was governed by a dynamic stability mechanism parameterized in terms of a critical Richardson number. The most important model feature in the design and interpretation of the hindcast experiment was the method of computing the surface forcing. The wind stress was prescribed from observations, while the surface heat flux was computed from climatological atmospheric conditions and the model-predicted SST. Thus, the surface thermal boundary condition served only to couple the hindcast SST to a regular annual cycle determined by the prescribed climatological atmospheric conditions (Haney, 1971). As a result, SST anomalies were produced in the model only by the horizontal and vertical redistribution of heat by currents and by parameterized turbulent mixing caused by the observed winds. The resulting hindcast SST anomalies were then analyzed and compared with the corresponding observed SST anomalies analyzed by Jerry Namias.

The hindcast and observed SST anomalies have similar space and time scales over the midlatitude North Pacific. The space scales correspond to those of the large-scale atmospheric forcing, and the time scales correspond to the time-integral of the atmospheric forcing. This result is the present model's representation of the physics of the stochastic forcing model of climate variability described above. Beyond that, the hindcast SST anomalies are positively correlated with the observed anomalies at a statistically significant level over most of the central midlatitude North Pacific Ocean (Fig. 6). The computed correlations are all positive, with values greater than 0.24 being positive at the 95% confidence level. The correlations are not significant in the northwest part of the North Pacific Ocean, perhaps owing to the effect on SST anomalies of the near-surface eddy field associated with the Kuroshio Extension, and in the southeast part of the North Pacific Ocean where wind forcing is generally weak all year. In the midlatitudes, the hindcast temperature anomalies are largely confined to the mixed layer, in agreement with Barnett's (1981) analysis of four years of AXBT data

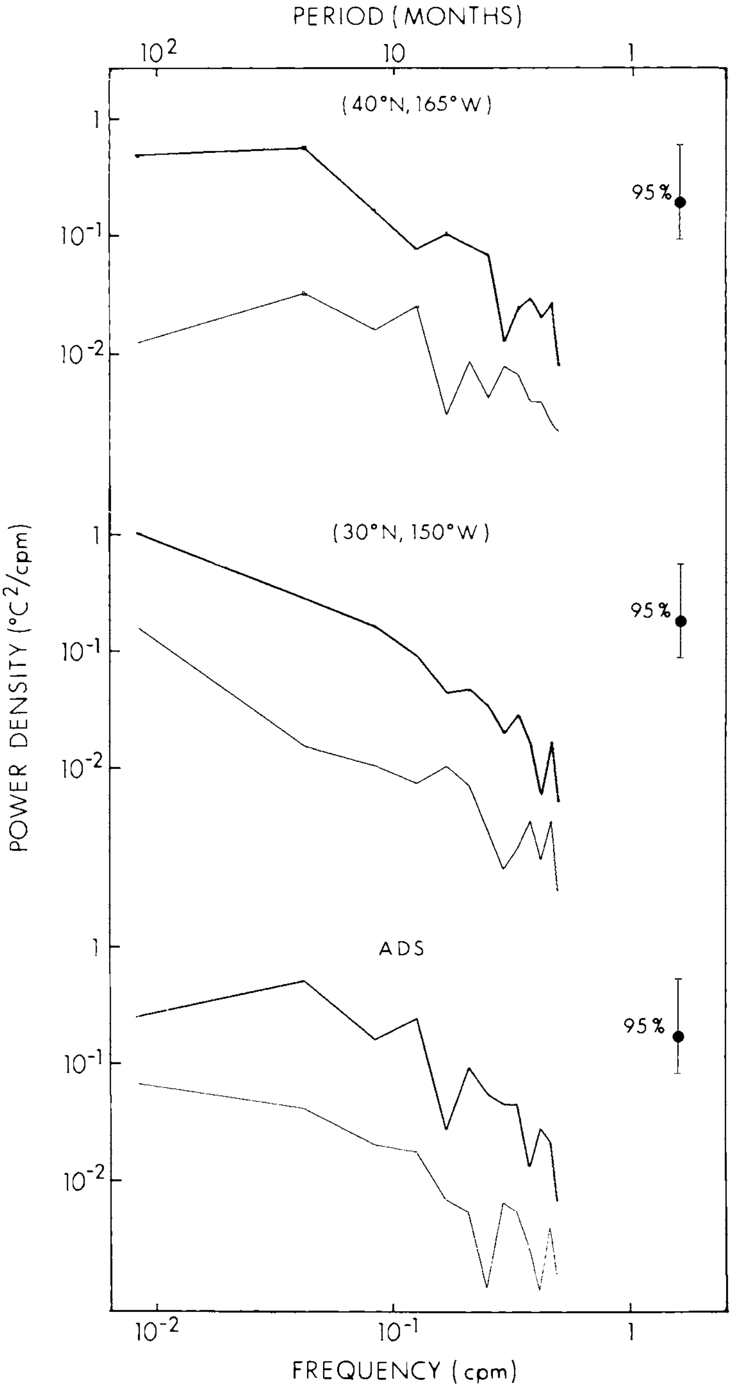

Figure 7. *Power spectrum of observed (thick lines) and hindcast (thin lines) SST anomalies. ADS is the Anomaly Dynamics Study region (30N to 50N and 180W to 125W). From Haney (1985).*

from the same area. These results strongly suggest that wind forcing *by itself*, through the mechanisms noted above, makes an important contribution to the development of large-scale SST anomalies in midlatitudes.

Although the hindcast and the observed SST anomalies are highly correlated, the hindcast anomalies are nevertheless only about one-third as intense as the observed ones. This is uniformly true over the entire mid-latitude North Pacific Ocean, and over the entire range of frequencies covered by the numerical hindcast (Fig. 7). We have carefully considered all possible reasons for this discrepancy, including many potential model deficiencies. As a result of these considerations, we have concluded that the most likely reason for the small amplitude of the hindcast SST anomalies is the absence of direct forcing by anomalies in the surface heat fluxes. This interpretation is also consistent with the results of Frankignoul and Reynolds (1983), who came to essentially the same conclusion about the dominance of anomalous heat flux forcing by an entirely different method. Their conclusion is based on the result that the variance of observed estimates of anomalous surface heat fluxes are much larger than the variance of observed estimates of anomalous Ekman advection.

In summary, the model hindcast results suggest that surface winds *by themselves* play an important role in SST anomaly development. This conclusion is based on the significant correlation between hindcast and observed anomalies. However, the *dominant* factor for SST anomaly development in the real ocean is *not* anomalous winds but rather, as inferred from our results and those of Frankignoul and Reynolds (1983), anomalous surface heat fluxes. Anomalous heat flux forcing was necessarily omitted in the present hindcast experiment due to inadequate observations. However this is the most likely explanation for the discrepancy between the amplitude of the hindcast and observed anomalies. In addition, the *variance* of anomalous surface heat fluxes is certainly large enough to provide the required forcing as demonstrated earlier by Frankignoul and Reynolds (1983). Therefore, if we are to understand and predict SST anomalies in midlatitudes, reliable synoptic estimates of anomalous surface heat fluxes on the scale of the ocean basins are needed. Hopefully for this problem, improved estimates of some of the heat flux components may soon be available through new techniques based on remote sensing (Gautier, 1981; Liu and Niiler, 1984).

Closing Remarks

I would like to close this short summary by noting that in many ways my research in modeling the ocean's large-scale response to atmospheric forcing has probably been concerned with the easier side of the coupled ocean atmosphere interaction problem. At the same time, atmospheric scientists who have been modeling the atmosphere's response to ocean surface temperatures which are in any way *prescribed*, also have been concerned with a much easier problem than that of the true coupled ocean-atmosphere system. If Jerry Namias' studies of both the atmosphere and the ocean have taught us anything, it is that we should be studying the behavior of the atmosphere and ocean as a coupled system. I, for one, am extremely grateful for that lesson from Jerry Namias, and I hope that some day I will be able to make a small contribution to that most challenging problem.

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The Characteristics of Sea Level Pressure and Sea Surface Temperature During the Development of a Warm Event in the Southern Oscillations

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National Center for Atmospheric Research

The year before a Warm Event in the Southern Oscillation the annual cycle in the circulation over the South Pacific Ocean is depressed. Associated with the depressed annual cycle, the surface water becomes abnormally warm in the southern winter and spring between about 10° S and 45° S, west of 135° W; and the warm water apparently affects the strength of the South Pacific Convergence Zone during the following seasons. The development of the opposite extreme, a Cold Event, tends to be the converse of a Warm Event.

Introduction

Namias examined the interaction between the Southern Oscillation (SO) and the atmospheric circulation over the North Pacific in a paper published in *Journal of Physical Oceanography*, 1976. He was then, as far as I know, the first to discern between the effects of both extremes of the oscillation.

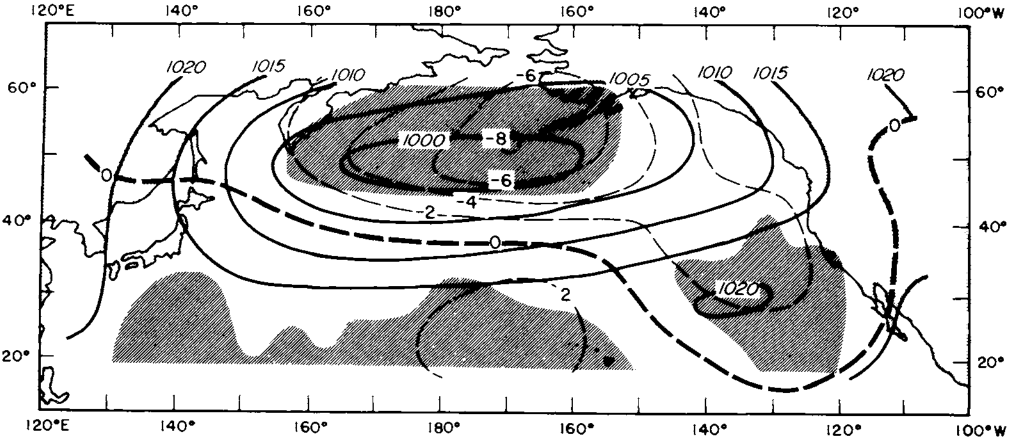

Figure 1. *Average sea level isobars (solid lines) in the North Pacific for seven warmest Puerto Chicama SST summers (J, F, M) and differences (broken lines) between these pressures and the mean for the seven coldest periods. Labels are millibars. Shading indicates significance at the 5% level (Namias, 1976).*

After outlining the statistical properties of the sea surface temperature (SST) at Puerto Chicama at 7°42'S on the Peruvian coast in El Niños and inverse El Niños, he described the mean anomalies of sea level pressure (SLP) in the Pacific Ocean north of 20°N which are associated with the extremes of the coastal sea surface temperature.

The map in Fig. 1 is reproduced from Namias' paper and shows that the pressure over the area of the Aleutian low in winter tends to be significantly lower during El Niños than during inverse El Niños, whereas it is significantly higher in subtropical latitudes.

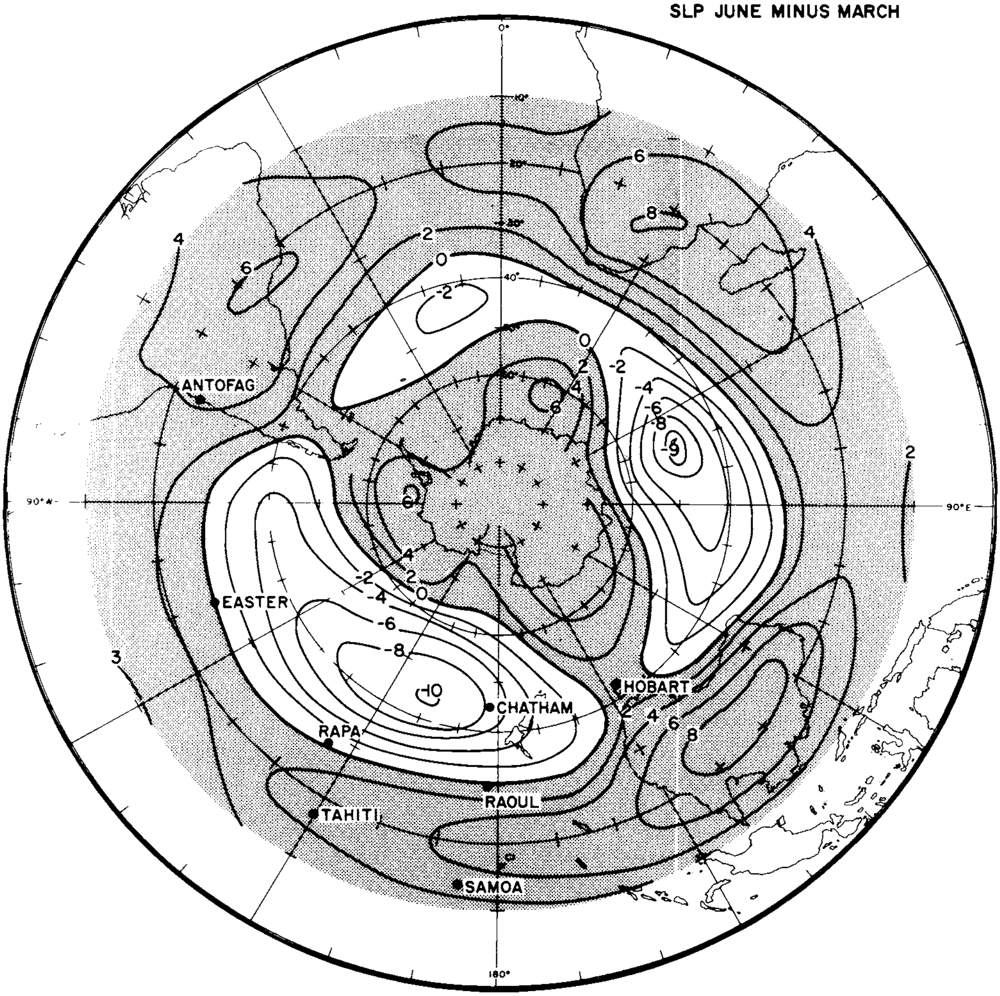

Figure 2. *The average difference in sea-level mean pressure (mb), June minus March (van Loon and Rogers, 1984).*

Namias ends his paper by saying that, "The extent to which the above phenomena are correlated with events in the Southern Hemisphere awaits adequate data and investigation." The adequate data will not be available for awhile, but as I shall show below, it is possible with the data available at present to obtain a fair picture of the development of the extremes of the SO on the Southern Hemisphere and to relate it to the circulation over the Northern Hemisphere.

Definitions, data, and analysis methods used in the following have been described in van Loon (1984) and van Loon and Shea (1985). In addition to the data mentioned in those papers, I have used the SLP for the Northern Hemisphere from the historical data set, beginning in 1899 north of 20°N, and from a U.S. Navy set for the area between the equator and 20°N, beginning in 1946. I shall speak of Warm Events and Cold Events instead of El Niños and anti-El Niños, warm and cold referring to the SST on the equator in the Pacific Ocean in the extremes of the Southern Oscillation. *The seasons in this paper are all seasons of the Southern Hemisphere.*

The Warm Events and the Annual Cycle of Sea Level Pressure Over the South Pacific Ocean

The mean change of SLP from March to June appears in Fig. 2 which shows that as pressure rises over Australia it falls in middle latitudes east of New Zealand, and a broad trough is thus impressed on the mean pressure pattern over the Pacific, reaching to 20°S. Consequently the trades weaken and the southerlies strengthen/northerlies weaken north of 45°S, west of 135°W, in the normal course of events.

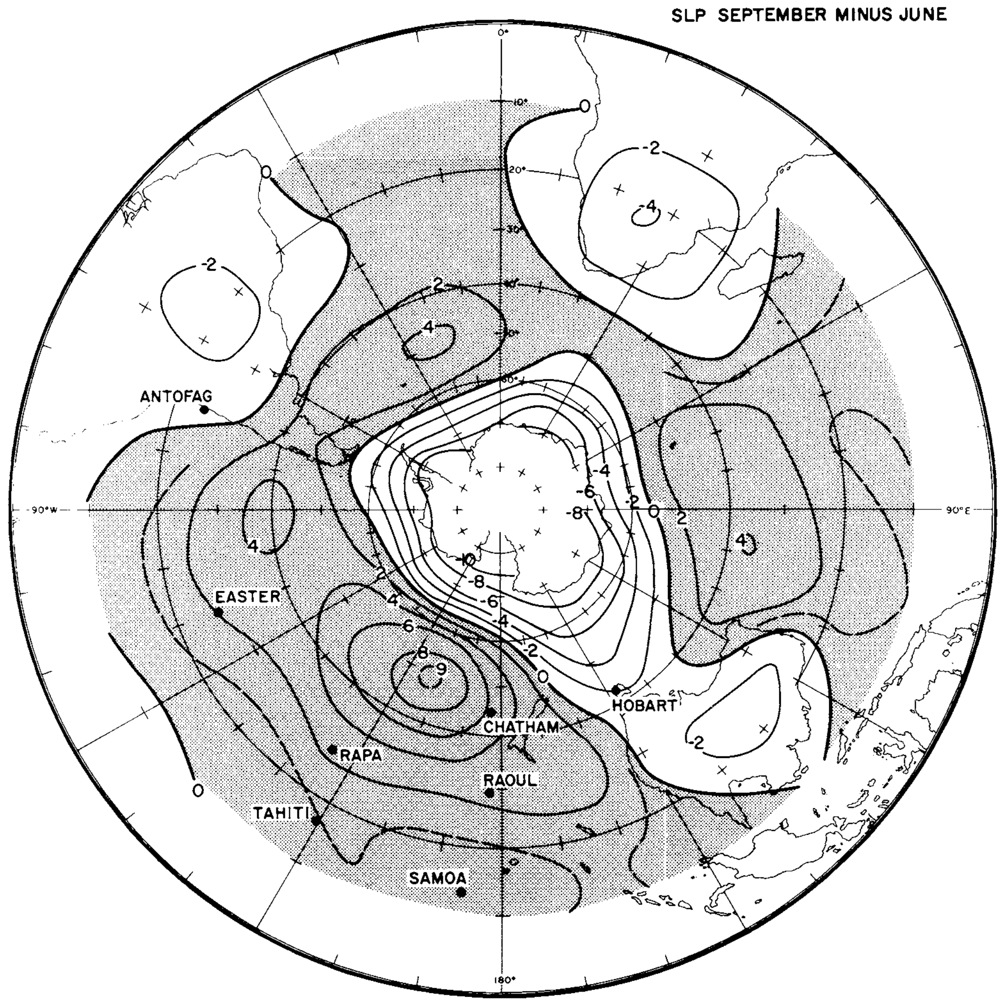

Figure 3. *The same as Fig. 2, but for September minus June (van Loon and Rogers, 1984).*

In contrast, the SLP falls from June to September (Fig. 3) over Australia but rises east of New Zealand with the result that the subtropical ridge strengthens, the trades pick up speed, and the southerlies weaken/northerlies strengthen over the western half of the South Pacific.

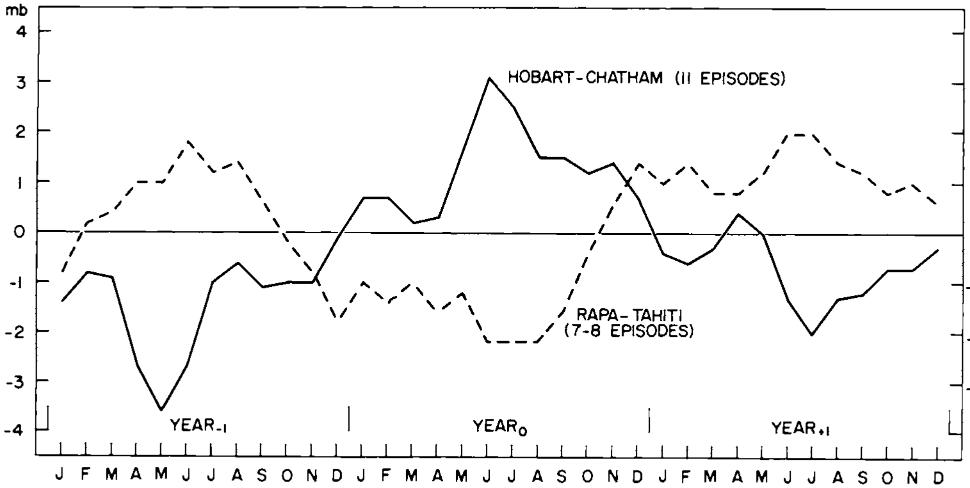

Figure 4. *Three-month running mean anomalies of the pressure differences Hobart minus Chatham and Rapa minus Tahiti during Warm Events (Year$_0$) and the years before and after. Eleven Warm Events were available for the former and seven to eight for the latter pressure difference. The values on the ordinate to the left refer to Hobart-Chatham, and on the right to Rapa-Tahiti.*

This part of the mean annual cycle in the South Pacific is markedly affected by the chain of events which lead to an extreme of the SO. The pressure differences between Hobart and Chatham and between Rapa and Tahiti (positions shown in Figs. 2 and 3) are a fair measure of the changes in the trades and in the meridional wind in the western Pacific. Figure 4 shows the 3-month running mean pressure anomalies of the differences Hobart minus Chatham and Rapa minus Tahiti in the year of a Warm Event, Year$_0$, and in the two surrounding years. I refer the reader to van Loon and Shea (1985) for similar information about Cold Events. In Year$_{-1}$ the difference between Hobart and Chatham is below average, especially in late fall and winter. The difference between Rapa and Tahiti is mostly above average in the same year. We may conclude from this evidence that as the trough is usually weak, or the ridge strong, in the year before a Warm Event we must expect enhanced trades and an anomalous northerly component in the wind over our area of interest.

In the year of the Warm Event we find the reverse circumstances: the trough is now strong with accompanying southerly anomalous winds and the trades are weaker than normal. In the year after the event we are back to similar but not quite so pronounced anomalies as those in Year$_{-1}$.

These anomalies of SLP differences between station pairs can be seen in a wider context in Fig. 5 and 6 which are compiled from three different data sources: North of 20°N the historical series beginning in 1899; between the equator and 20°N a U.S. Navy set beginning in 1946; and south of 10°S daily historical analyses from South Africa, 1951-1958, and daily numerical operational analyses from Australia, May 1972 and on. There are 19 Warm Events in the set north of 20°N; nine between the equator and 20°N; four for Year$_{-1}$ south of 10°S and six for Year$_0$. The long term mean which has been used to define the anomalies contains neither Warm nor Cold Events. North

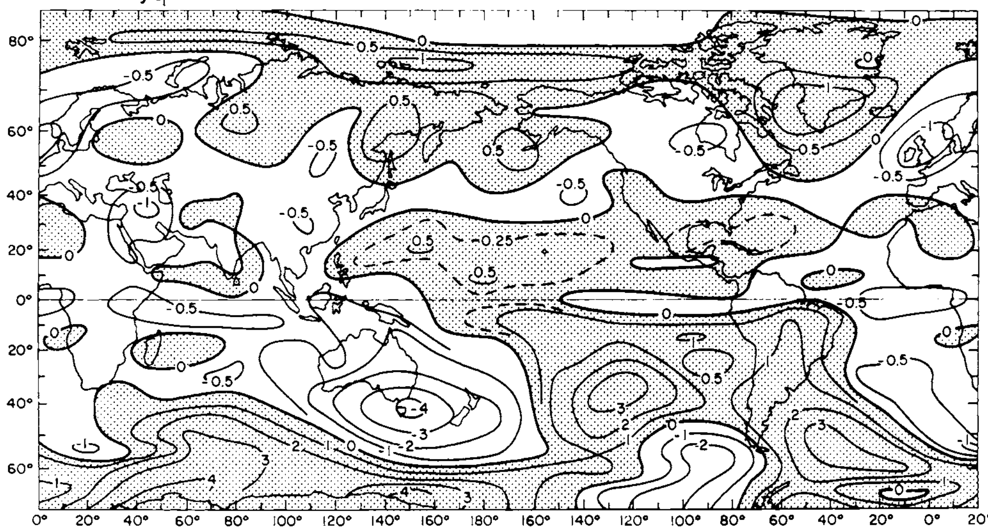

Figure 5. *Sea level pressure mean anomalies (mb) for June-July-August of the year before a Warm Event. See text for the data used to compile the map.*

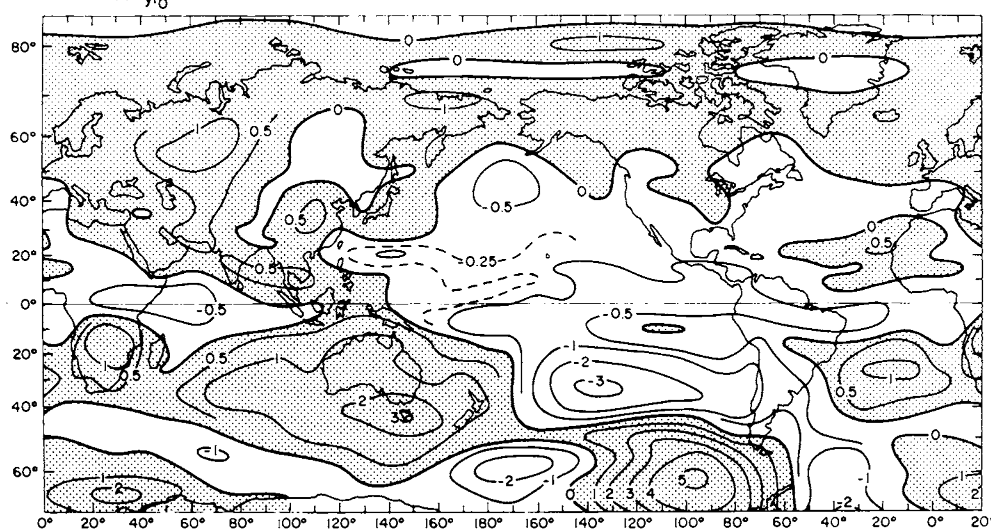

Figure 6. *As Fig. 5, but for June-July-August of the year of the Warm Event.*

of 20°N there are 50°years in this mean, between the equator and 20°N there are 23 years, and south of 10°S eleven years.

The anomalies are small on the Northern Hemisphere in June-July-August of Year$_{-1}$ and hardly reach the 95% confidence level anywhere, but on the Southern Hemisphere the anomalies are large, statistically significant, and systematically distributed, a fact that will become apparent when Figs. 5 and 6 are compared: In the winter of the year before a Warm Event, JJA$_{-1}$, pressures are anomalously low over Australia and high over the South Pacific north of 50°S. Twelve months later, JJA$_0$, this pattern has reversed and the well known SLP distribution in Year$_0$ of a Warm

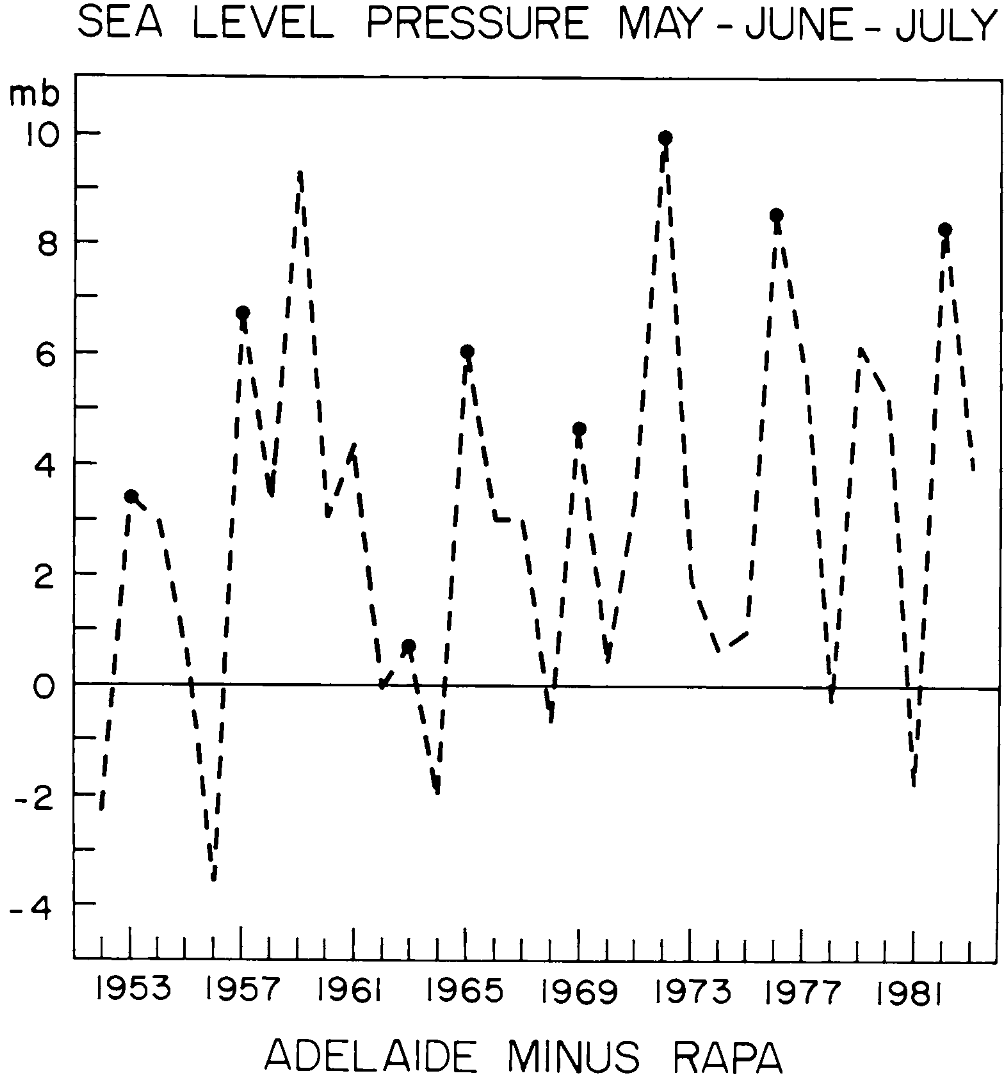

Figure 7. *Time series of the difference in sea level pressure (mb) between Adelaide and Rapa for May-June-July. The dots denote Years$_0$ of Warm Events.*

Event appears, with positive anomalies over Australia and negative anomalies over the Pacific. The anomalies on the Northern Hemisphere are now well beyond the 95% confidence level in the tropical North Pacific between 180°and the Caribbean, and between the Philippines and India.

The pattern of anomalies on the Southern Hemisphere suggests that the whole hemisphere is affected, although the values are largest and statistically most significant over Australia and the Pacific. They can be read as a wave train beginning in the Pacific, crossing South America, and ending in the southernmost Indian Ocean with the phase reversing from Year$_{-1}$ to Year$_0$. The pattern's reversal can also be seen as a modulation of wave 3 in the extratropical latitudes of the hemisphere such that the troughs of the wave in the anomalies which lie in the longitudes of the Tasman Sea, easternmost Pacific, and south of Africa in JJA$_{-1}$, are displaced 40°- 50°in JJA$_0$.

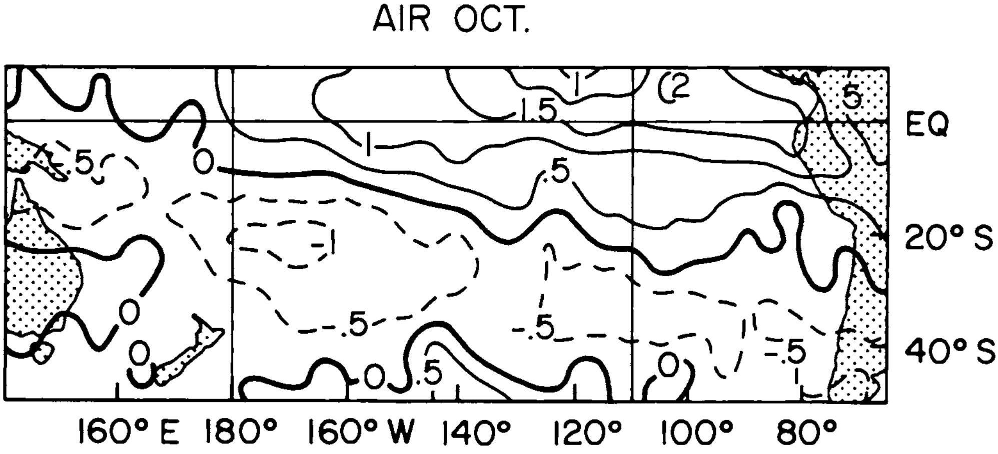

Figure 8. *The average difference in the air temperature (° C) of September-October-November for all Warm Events from 1925 to 1976: Year$_0$ minus Year$_{-1}$. Negative values mean that the temperature of September-October-November of Year$_{-1}$ was higher than that of Year$_0$.*

It is clear from Figs. 5 and 6 that the anomalous wind reverses from northerly directions in JJA of Year$_{-1}$ to southerly directions in JJA of Year$_0$ over the area between about 10°S and 45°S, west of 135°W. One gets an indication of this reversal in the single Warm Events by means of the pressure difference between Adelaide (35°S, 139°E) and Rapa (28°S, 144°W), which are located near the largest positive and negative anomalies in Figs. 5 and 6. The time series in Fig. 7 of this SLP difference for May-June-July since 1952, the first year available for Rapa, shows that all eight Warm Events during these 32 years, even the weak one in 1963, happened in a year when the pressure difference was larger than in the year before, i.e., when the geostrophic wind had a stronger southerly component than in the year before. Not one event took place under the opposite conditions, but there *were* years when the pressure difference between Adelaide and Rapa was larger than the previous year, such as 1959, 1961, and 1979, which were not associated with a Warm Event. *Such a sequence must therefore be a necessary but not sufficient part of the development of a Warm Event.*

One naturally expects that the reversal of the anomalous wind from Year$_{-1}$ to Year$_0$ will be reflected in the temperature of the underlying sea surface. In van Loon and Shea (1985) we demonstrated that the sea surface temperature over this area is indeed higher in the winter, spring, and early summer of Year$_{-1}$ than in the same seasons of Year$_0$. The biggest temperature difference between the two years is in July-August-September, but Year$_{-1}$ remains warmer into the following southern summer, and by the spring of Year$_{-1}$ this condition has spread east- southeastward to South America, as illustrated for September-October-November in Fig. 8. This figure is for the air temperature but it has the same configuration and size of the differences as has the corresponding illustration for sea surface temperature.

The core of the composite pattern in Fig. 8 where the temperature is higher in Year$_{-1}$ than in Year$_0$ is statistically robust, as demonstrated in Fig. 9. In this figure the air temperature at Rarotonga in July-August-September of Year$_0$ is plotted against that in Year$_{-1}$ for 17 Warm Events since 1907. The island lies in the region where the anomalous wind reverses from northerly directions in Year$_{-1}$ to southerly in Year$_0$.

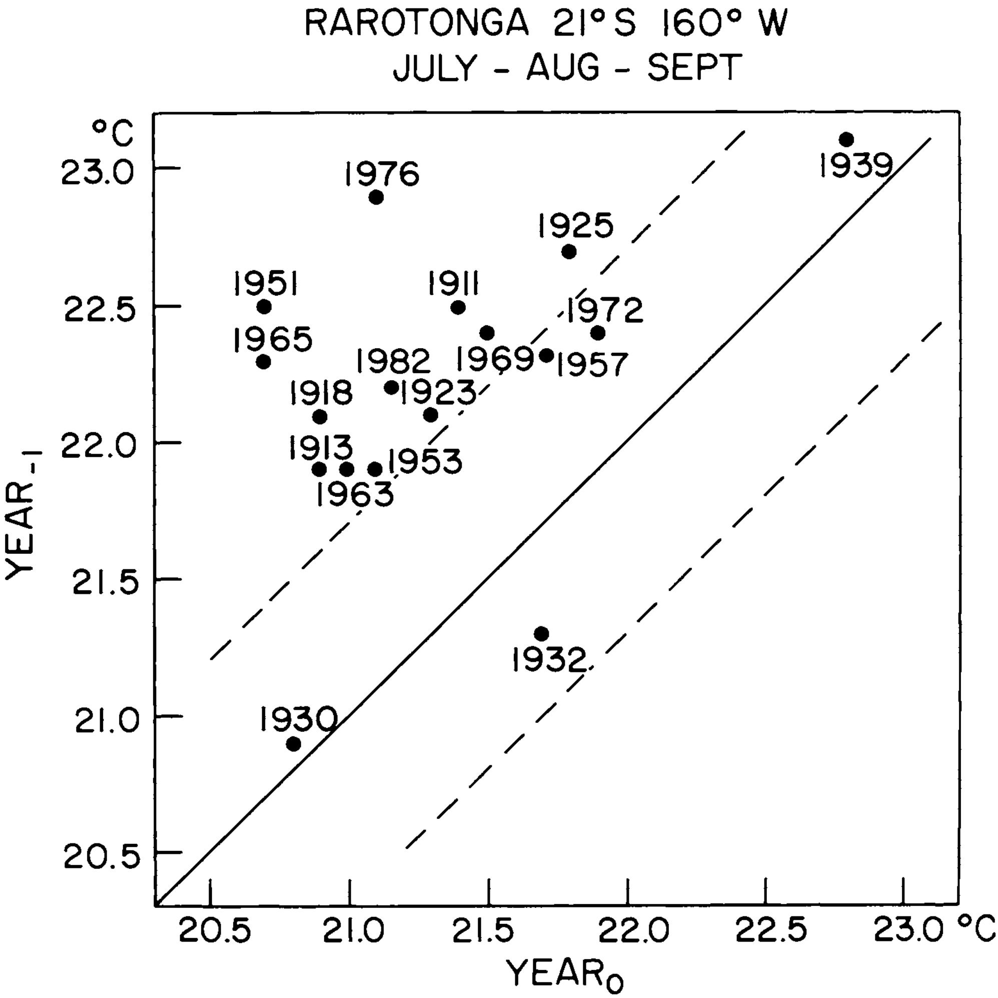

Figure 9. *The mean air temperature at Rarotonga in July-August-September of Warm Events (Year$_0$) plotted against that of the year before (Year$_1$). The year given is the year of the Warm Event. The distance between the diagonal and the dashed lines is one standard deviation from the long term mean.*

Sixteen of the 17 years were warmer in Year$_{-1}$ than in Year$_0$, 12 of these by more than one standard deviation of the 3-month mean.

When the South Pacific Convergence Zone expands toward the south from the southern winter to summer on its usual interseasonal course, during the year before a Warm Event it then expands over a comparatively warm sea surface, and one should thus expect the convection in the convergence zone to be comparatively intense. Figure 10 indicates that this is a plausible idea: The figure shows the difference in the average rainfall for November-December-January $(R_N+R_D+R_J/3)$ between Year$_0$ and Year$_{-1}$. The sources are the observations from continental and island stations, and it is not likely that the values are representative of the open ocean; but we may use Fig. 10 as an indication of the sign of the difference in rainfall between Year$_0$ and Year$_{-1}$. Note that the rains are indeed heavier in the spring and summer before a Warm Event over

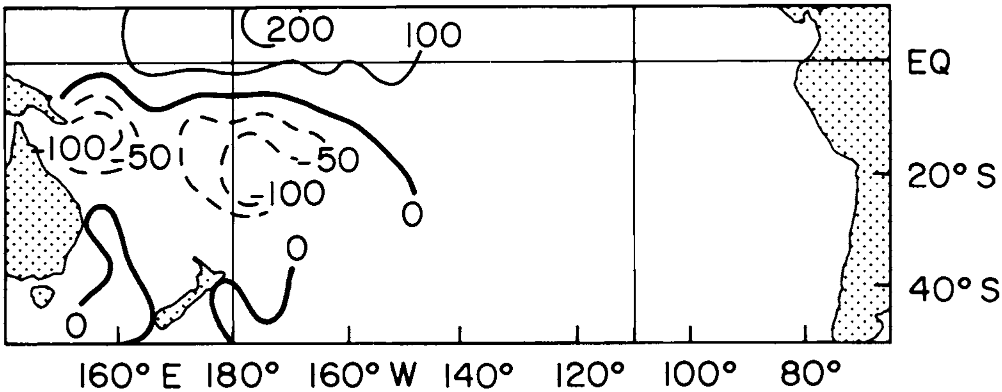

Figure 10. *As Fig. 8, but for rainfall (mm) in November-December-January. The values are $R_N + R_D + R_J/3$.*

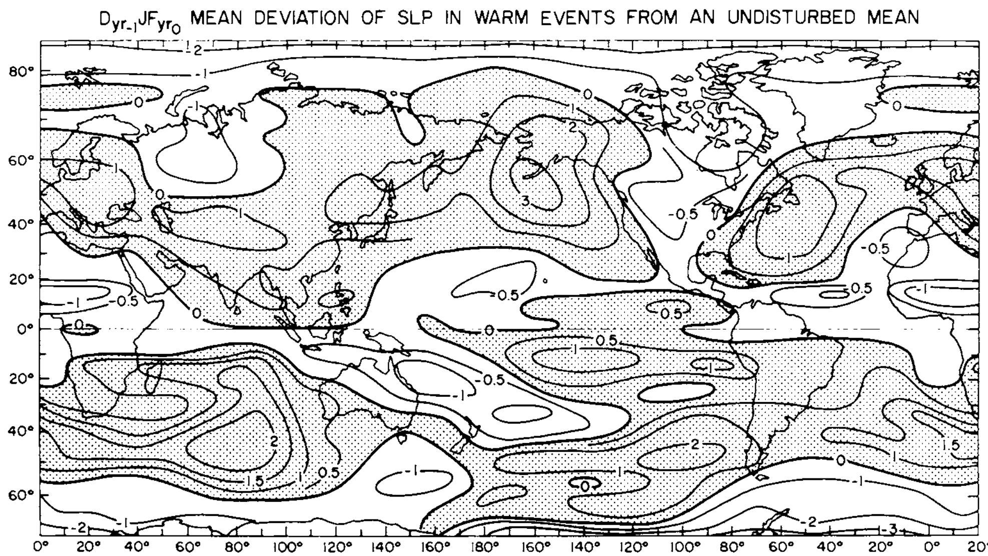

Figure 11. *As Fig. 5, but for December$_0$-January$_{+1}$-February$_0$ of a Warm Event.*

the area where the temperature is higher in Year$_{-1}$ (cf. Fig. 8). Stronger convection is associated with lower sea level pressure and it is evident in Fig. 11 that the mean pressure *is* below normal in the summer before a Warm Event over the zone of higher temperature (Fig. 8) and heavier rains (Fig. 10), in other words, that the South Pacific Convergence Zone is displaced toward the southwest and is more intense. The pressure anomalies in Fig. 11 imply that the trades over the South Pacific Ocean are weaker than normal (westerly anomalous winds) from New Guinea- Australia to South America, south of about 10°S. Note, however, that the trades in the equatorial belt-between 5°S and 5°N-are still strong (easterly anomalous winds). There are also negative pressure anomalies in the central North Pacific Ocean and although weak in comparison with those on the Southern Hemisphere, despite its being the winter of the Northern Hemisphere, they indicate a simultaneous weakening of the northeast trades, an observation made also by Namias (1973).

As mentioned in the discussion of Figs. 5 and 6, the pressure anomalies on the

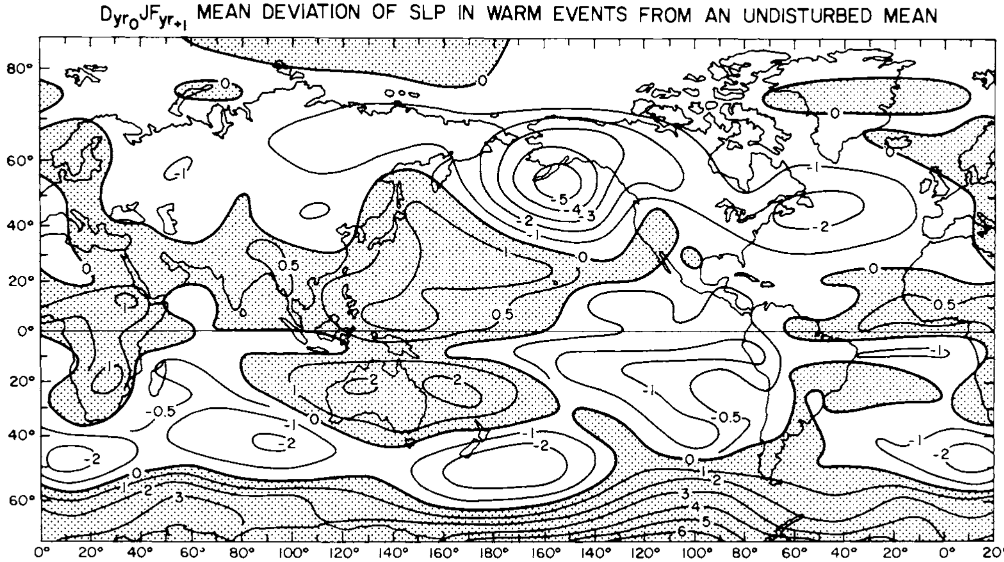

Figure 12. *As Fig. 5, but for December$_0$-January$_{+1}$-February$_{+1}$ of a Warm Event.*

Southern Hemisphere reverse sign from Year$_{-1}$ to Year$_0$ in the southern winter, and the anomalous winds over our area of interest, about 10°S to 45°S west of 135°W, switch from northerly in Year$_{-1}$ to southerly in Year$_0$. The water surface and the air above it are thus colder in Year$_0$ than in Year$_{-1}$ over this area (Figs. 8 and 9), the convection in the South Pacific Convergence Zone during the following spring and summer is weaker (Fig. 10), and one should therefore expect pressures above normal. This expectation is borne out by Fig. 12 in which an area of above normal pressure extends southeastward from Queensland into middle latitudes in the central Pacific, near the normal position of the South Pacific Convergence Zone. Whereas the anomalous winds associated with the pressure anomalies in Fig. 12 now are westerly over most of the equatorial belt, they are already easterly over most of the tropics and subtropics of the South Pacific. It is thus possible that the Southern Oscillation ordinarily has a means of ending a Warm Event through the positive pressure anomalies and easterly wind anomalies associated with the weaker South Pacific Convergence Zone during the southern spring and summer of Year$_0$ that in turn are related to the negative temperature anomalies at the surface which developed in the preceding winter between 10°and 45°S, west of 135°W.

As in the southern summer a year earlier (Fig. 11) the pressure anomalies over the tropical North Pacific Ocean in Fig. 12 are of the same sign as those in the same longitudes on the Southern Hemisphere. The northeast trades in the northern winter at the end of a Warm Event therefore pick up at the same time as their counterparts on the Southern Hemisphere, at a time when the anomalous winds on the equator are still westerly. In Fig. 12 as in the three seasons shown in Figs. 5, 6, and 11, the tropical anomalies are larger on the Southern than on the Northern Hemisphere. Considering, in addition, the plausible physical relationships outlined in the development of the pressure anomalies over our area of concern, I should like to plead that the Southern Oscillation has its origin in this area, which the South Pacific Convergence Zone traverses. The tropics of the North Pacific act in symmetry with the South Pacific (Figs. 11 and 12), but the anomalies are smaller there and are not associated

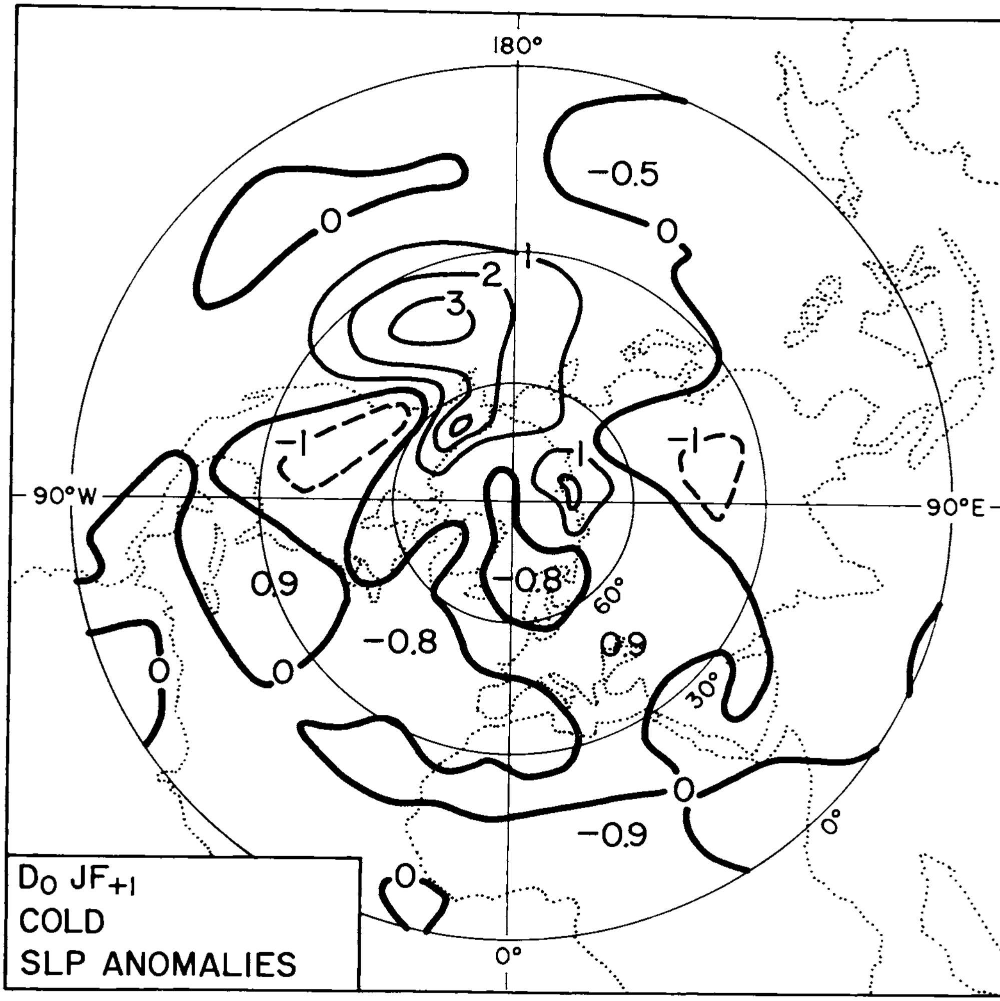

Figure 13. *Mean anomalies of sea level pressure for December$_0$-January$_{+1}$-February$_{+1}$ of Cold Events. See text for the data used to compile the map.*

with such a physically logical chain of events as that taking place around the South Pacific Convergence Zone. I should also like to argue that the marked anomalies in the southern fall and winter of the year before a Warm Event (Fig. 5), which at the time have no counterpart on the Northern Hemisphere, and their complete reversal a year later (Fig. 6), are cause for believing that the air-sea interaction over our area of concern plays a major role in the forcing of the Southern Oscillation.

The North Pacific Wave Train in Winter

The mean anomalies at the end of a Cold Event are shown in Fig. 13, with a Student t-test of the statistical significance in Fig. 14, where the areas above the 95% confidence level are shaded. Sixteen Cold Events are used for the mean anomalies north of 20°N, and seven south of 20°N. The mean against which the anomalies are measured contains 50 years north of 20°N and 23 years to the south. The signal in the Pacific, across North America, and into the western Atlantic is clearly a wave train in the opposite phase of that of Warm Events at the same stage (Fig. 12), and

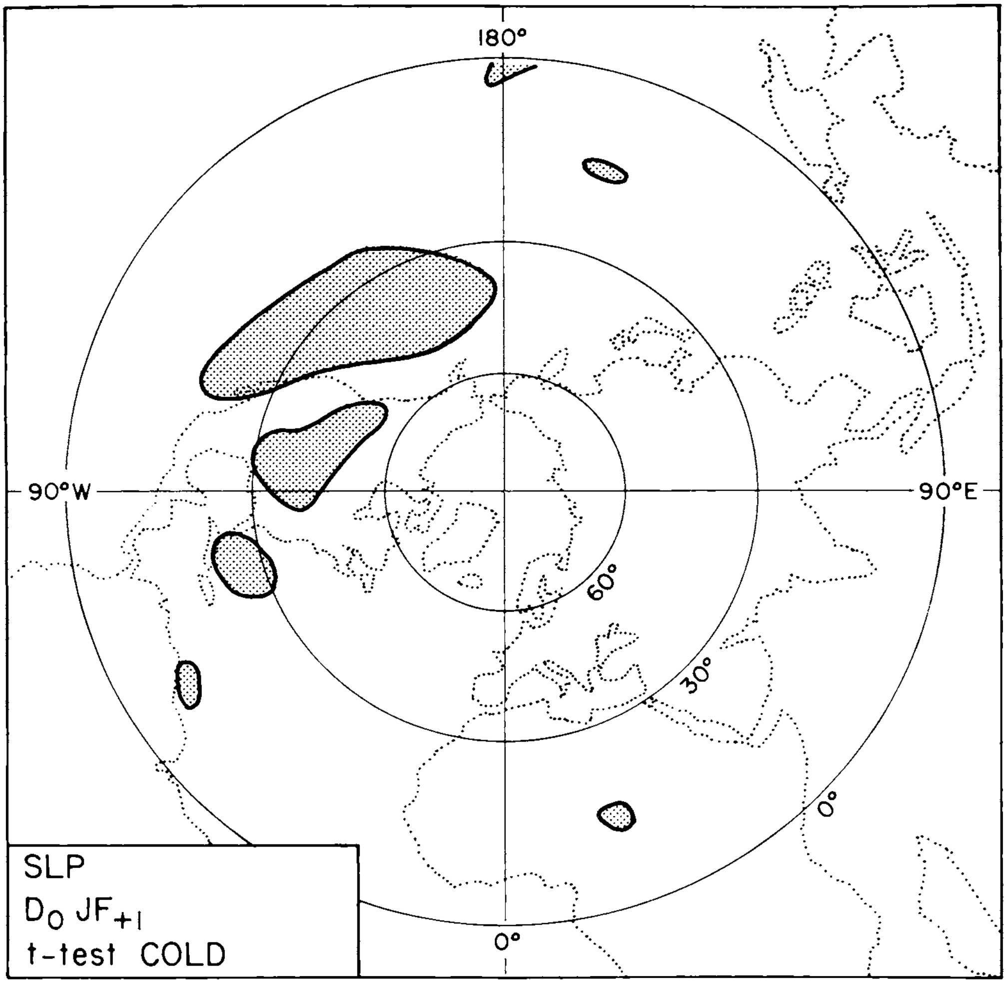

Figure 14. *A Student t-test of the anomalies in Fig. 13. Areas above the 95% confidence level are stippled.*

the anomalies of opposing sign in the waves are all statistically significant at the 95% confidence level.

In his paper on the interactions between hemispheres, Namias (1963) pointed out that "strongly opposing circulations for two extreme winter months over the Northern Hemisphere are associated with latitudinal bands of opposing temperature and precipitation anomalies extending through North, Central, and South America." He noted in particular the opposition in phase of the wave train over North America and the adjacent oceans. The two months he picked for illustration, January 1950 and 1958, happen to be the Januaries of Year$_{+1}$ of a Cold Event (1950) and a Warm Event (1958), both of which showed an uncommonly clear signal of their respective event.

Going back to Figs. 11 and 12, one should note that the anomalies in $D_{-1}J_0F_0$ in the Trans-North-America wave train are the opposite of those 12 months later; or said otherwise, the wave train changes phase from the northern winter at the beginning of the year of a Warm Event to the winter at the end of the event. Although the anomalies are not so large in Fig. 11 as in Fig. 12, they are nevertheless significant at

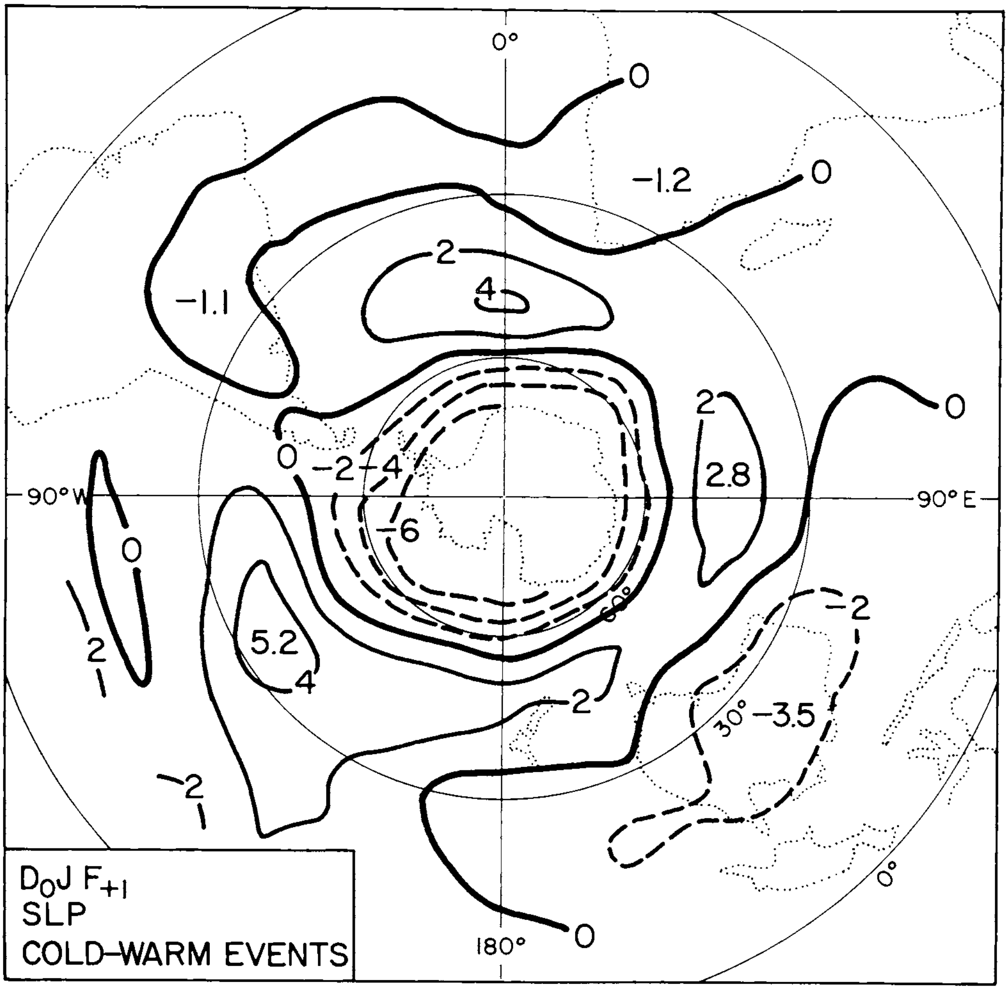

Figure 15. *The mean pressure difference between Cold and Warm Events in December$_0$-January$_{+1}$-February$_{+1}$. The map contains six Warm and three Cold Events.*

the 95% confidence level. It is also noteworthy that the phase of the wave train across North America is the same at the beginning of Year$_0$ of a Warm Event as it is at the end of Year$_0$ of a Cold Event, which is remarkable as only five of the Years$_0$ of the 16 Cold Events were Years$_{-1}$ of Warm Events (Table 1 in van Loon and Shea, 1985). The pressure difference over the Pacific between the Warm and Cold Events in Figs. 12 and 13 corresponds well to Namias' difference (Fig. 1) based on the extremes of sea surface temperature at Puerto Chicama.

Finally, Fig. 15 shows the difference Cold minus Warm Events in D_0, J_+, F_{+1} for the Southern Hemisphere south of 10°S. The picture is provisional in its details as there are only three Cold Events available to produce it. None of the other such differences computed are worth showing as already in the winter of Year$_0$ only two Cold Events are available. There are six Warm Events in Fig. 15. The overall features of the figure deserve comment: The map has the sign of the Cold Events and indicates that at the end of these events pressure over the antarctic is lower than in the Warm Events. As expected, the pressure over Australia is lower and over the Pacific higher in the Cold

Events, and the pattern in middle latitudes tends towards zonal symmetry, as evident in Figs. 11 and 12 too, but contrasting the marked waves in winter (Figs. 5 and 6).

Conclusion

This paper has attempted to expand our knowledge of the association between the Southern Oscillation and the circulation at sea level on the Southern Hemisphere, and in this respect extends work begun by Trenberth (1975, 1976) for the South Pacific area, and continued and expanded by van Loon and Madden (1981), van Loon and Rogers (1981), van Loon (1984) and van Loon and Shea (1985). As for the Northern Hemisphere, the paper has followed in Namias' footsteps and confirmed and amplified his work (Namias, 1976) on the signal of the oscillation's extremes in the North Pacific Ocean.

Let me end this talk be acknowledging my debt to Jerry Namias in this and other aspects of my work. There is no better way of doing so than by applying to Namias his own words about Jacob Bjerknes, "In his golden years [he] is no less active, no less productive and most important, no less imaginative and creative than in his early years... We lesser synopticians owe him a tremendous debt of gratitude for what he has done and is doing, and for giving us a model to try to emulate" (Namias, 1975).

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Expressing Uncertainty in Long Range Forecasts

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Climate Analysis Center

The probabilities that are subjectively assigned to the NWS Monthly and Seasonal Outlooks depend on the verifications of a long record of previous forecasts. The advent of dynamical prediction and other new objective tools in long range forecasting will raise anew the question of how measures of predictive uncertainty may be generated.

The other speakers at this Symposium have pretty well boxed the compass of Jerry Namias' many research interests, so I would like now to turn to the subject that was perhaps his first love, the making of forecasts. I suppose that he has made - and criticized - more forecasts than any other person in this hall, more even than all of us together. Is it just coincidence that he has stationed himself in the front row before me in exactly the same place that he used to take at five-day forecast discussions in the 50's and 60's in Building 4, Suitland - eye to eye with the briefing forecaster?

My talk will have to do with attaching probabilities to monthly and seasonal forecasts: how we in the Climate Analysis Center do it now, what lessons we can draw from our first three years' experience at doing it, and how the entry of extended dynamical model runs into monthly prediction may change and strengthen the basis for doing it.

Generating Forecast Probabilities

I will not take time today to argue the need for presenting the users of long-range - or other forecasts - with valid, unbiased estimates of the uncertainty of each aspect of those forecasts. Suffice it to say that decisions influenced by forecasts that are applied without counting their uncertainty will not only be poorer than they ought to be; they may in fact be worse or more costly than decisions taken with no forecast at all. The uncertainty is best expressed, I think, in terms of numerical probabilities.

Probabilities need events to refer to. In some kinds of weather forecasts the events stand forth clearly; rain or not, frost or none, hurricane force winds or less. In monthly and seasonal forecasts, dealing with such elements as mean temperature or total precipitation, we must - in the present state of the art - make somewhat arbitrary distinctions, dividing up continuous frequency distributions into a limited number of categories or classes. The current CAC Outlooks offer only three: Warm, Near Normal, or Cold for temperature; Light, Moderate, or Heavy for precipitation.

Numerical bounds are chosen to make the climatic probabilities of the classes 30, 40 and 30%. Verification records have shown us that this simple division presents the limited skill of the forecasts in the most useful way, since essentially all of it occurs outside the central class. That class may be given a fixed rating of 40%, leaving only the two outer class probabilities to vary with the forecast. When one of them goes up, the other goes down by the same amount. A forecast map therefore need give only one probability at each location.

Figure 1, the temperature outlook for Winter 1982-83, shows our format, in which probability contours are drawn for the preferred class - in this case COLD west of the Mississippi and Warm in the East. Along the heavy 30 line, there is no preferred class, the probabilities being a perfectly climatological 30-40-30. In the shaded areas, the preferred class's probability exceeds 40 and it becomes also the most likely class. Elsewhere Near Normal remains most likely.

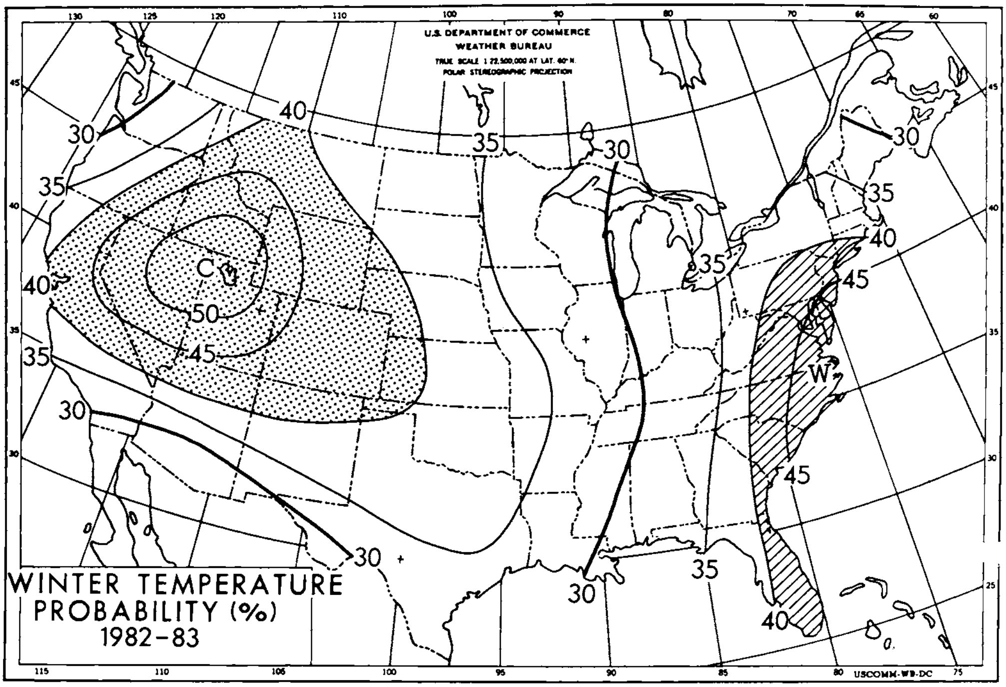

Figure 1. *Winter outlook, 1982-83, NOAA Climate Analysis Center. C stands for COLD, W for WARM, and NEAR NORMAL is a constant 40% and does not appear. The 30% line separates the WARM and COLD areas.*

Some subscribers to the *Outlook*, conscious of their own special or critical thresholds of temperature, will want to redefine the class boundaries to fit their decision problems. They can do so (approximately) with the help of a supplementary table, based on our verifications, that converts the bell-shaped Gaussian climatological probability distribution to a forecast-conditioned Gaussian distribution, slightly narrowed and shifted a little toward the preferred class by amounts that depend on the strength of the original forecast probability. Precipitation, alas, does not follow the classic Gaussian distribution and will need much more elaborate treatment.

The preceeding description will seem unfamiliar and disorienting to those in the audience who have seen only the press or NOAA facsimile circuit maps and not the published leaflet sent to subscribers. The press/facsimile version deals in two classes only–Above or Below Normal temperature and Above or Below Median precipitation. The probability contour lines of the maps look just like those on the subscription maps, but their numerical labels are 20 points higher as a result of dividing the 40 points of

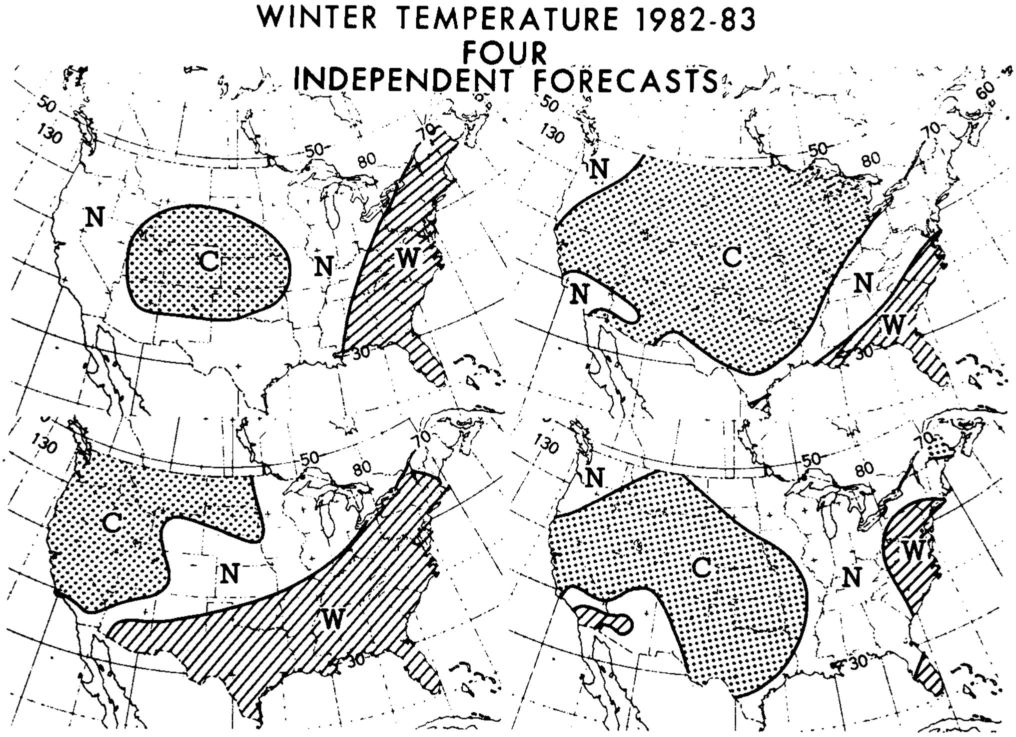

Figure 2. *Input to the CAC Winter Outlook in categorical form. Areas designated as in Figure 1.*

Near Normal equally between the Above and Below events. Shading is used to blot out all areas within 5 points of Normal, effectively removing them from the forecast.

Our forecast methods differ somewhat between months and seasons, but in both cases amount to a mixture of statistical calculations, synoptic climatology, study of selected past cases, forecasters' judgement, and–for months–synoptic extrapolation and 5- and 10-day dynamical model runs, or–for seasons–consideration of tropical Pacific and North Pacific sea surface temperatures. In narrating our forecast procedure, I will continue to use as an example the Winter of 1982-83 Outlook of Figure 1.

In November 1982, four forecasters, working independently, sketched key height anomalies on 700 mb charts and then inferred from them the surface temperature patterns of Figure 2. The agreement among the patterns was perhaps a little better than usual. All four patterns were combined objectively by vote at our 100 U.S. verification stations to give the "consensus" picture of Figure 3. In deference to the statistics of El Niño winters, of which this was an ongoing case, the Deep South was cooled a little for the final outlook in Figure 3. The 1972-73 El Niño winter loomed large in the discussions, seeming to offer the closest approach to an analog for the current oceanic warming.

To transform the simple temperature category map to a contour chart expressing the probabilities of the categories, we turned first to the tables of Figure 4. The columns of the first table show verifications in percent frequency of occurrence, given the forecasts on the column head, as compiled at 100 stations for the previous 24 winter

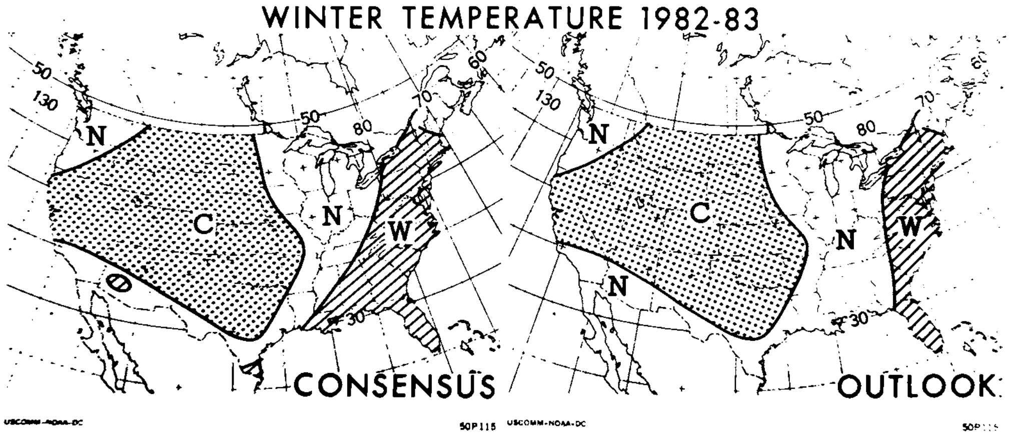

Figure 3. *Second and third stages of the CAC Winter Outlook, still in categorical form. Areas designated as in Figure 1.*

outlooks. The unlabeled fourth column gives the actual frequency of occurrence of the categories without regard to forecast. The class limits in use over 1959-82 had been designed to divide the events into equal thirds, but the fourth column shows that cold conditions had tended to prevail most often. Note how completely the skill of the forecasts - that is, the departure of the conditional frequencies of the labeled columns from the unconditional frequencies of the fourth column - is concentrated under Cold and Warm. Near Normal shows less than none.

PREDICTED(1959-82)

OCCURRED	C	N	W	
C	55	40	27	42
N	33	29	31	31
W	12	31	42	27

PREDICTED

PROBABILITY	C	N	W	
C	43	28	17	30
N	40	40	40	40
W	17	32	43	30

Figure 4. *Verification table (left) and a probability table (right) derived from it for use in forecasts. Expressed in percent. Right-hand columns give unconditional frequencies and probabilities, respectively.*

The first table summarizes the past. A second is needed to apply to forecasts of the future, for which we were using new 30-40-30 class limits and for which no prevailing temperature bias could be known in advance. The verification table's numbers were accordingly adjusted to bear about the same relation to 30-40-30 as they previously had to 42-31-27, with all Near Normal probabilities further constrained to equal exactly 40. That process gave us the second table: estimates of conditional probabilities.

The probability table applies to the United States as a whole. Can its numbers be modulated geographically? Figure 5 shows a schematic derived from the map of

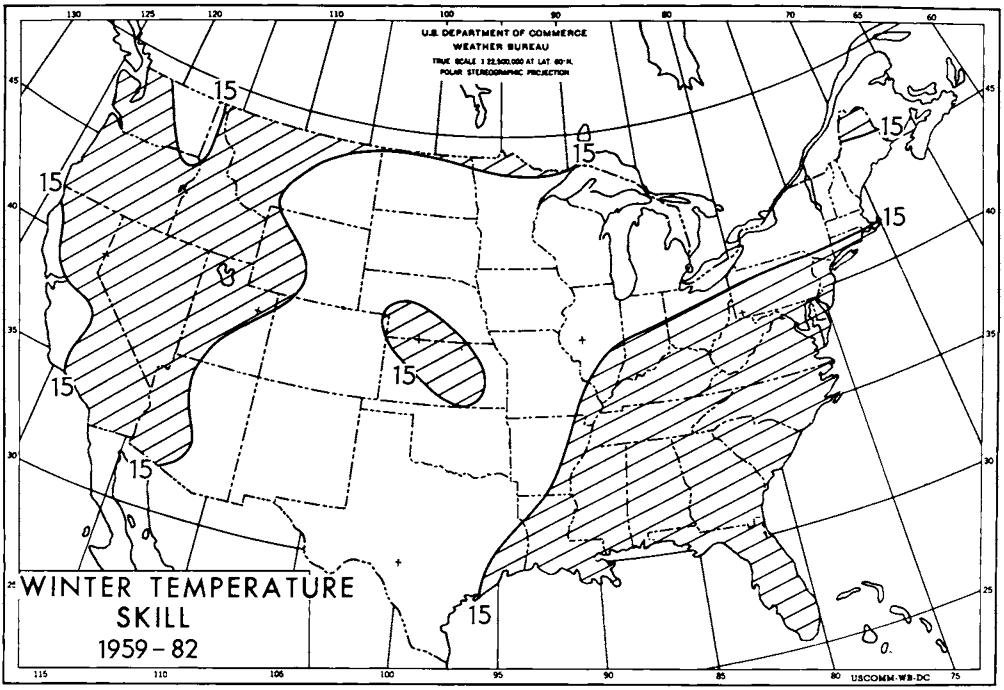

Figure 5. *Verification map for CAC Winter Outlooks. Shaded area encompasses stations where skill score exceeds 15, on a scale of 0 for randomly generated forecasts (expected value) to 100 for perfect in the 3-category non-probabilistic system.*

100 local winter skill scores. Plotted in raw form, they make a very noisy pattern, with random variations (about zero) of as much as ±15. Values in the shaded areas of Figure 5 exceed 15 and would appear to be significantly positive. Elsewhere the skill remains dubious. Because sampling uncertainty on a larger spatial scale can also exist, one may still mistrust the reliability of such a pattern, and for many years I had. Roland Madden's estimates - from long data records - of potential predictability of seasonal temperature help to anchor the pattern. Figure 6 shows his estimate for Winter, which resembles the empirical skill pattern pretty well except from the Great Lakes to New England. That is an area of considerable year-to-year variation of snow cover, certainly influencing local temperature, which, because of its dependence on particular storms, we cannot expect to predict.

In addition to allowing for the skill pattern, we noted the degree of unanimity among independent forecasts at each location. We judged the comparative level of difficulty of the whole forecast pattern on this particular occasion and discussed some of the most likely alternate outcomes. And, finally, we increased the tabular probabilities somewhat in the middle of predicted Warm or Cold areas and decreased them along the margins in the process of smooth contouring that completed the integration of all these judgements and brought us to the final form of Figure 1.

Seasonal precipitation outlooks are treated in the same way as those for temperature, but all monthly outlooks, because of tight deadlines, rest finally on one forecaster's work aided by a short discussion with others, so that the "consensus" character of the seasonal outlooks is not attained. The value of "consensus" in maximizing skill in

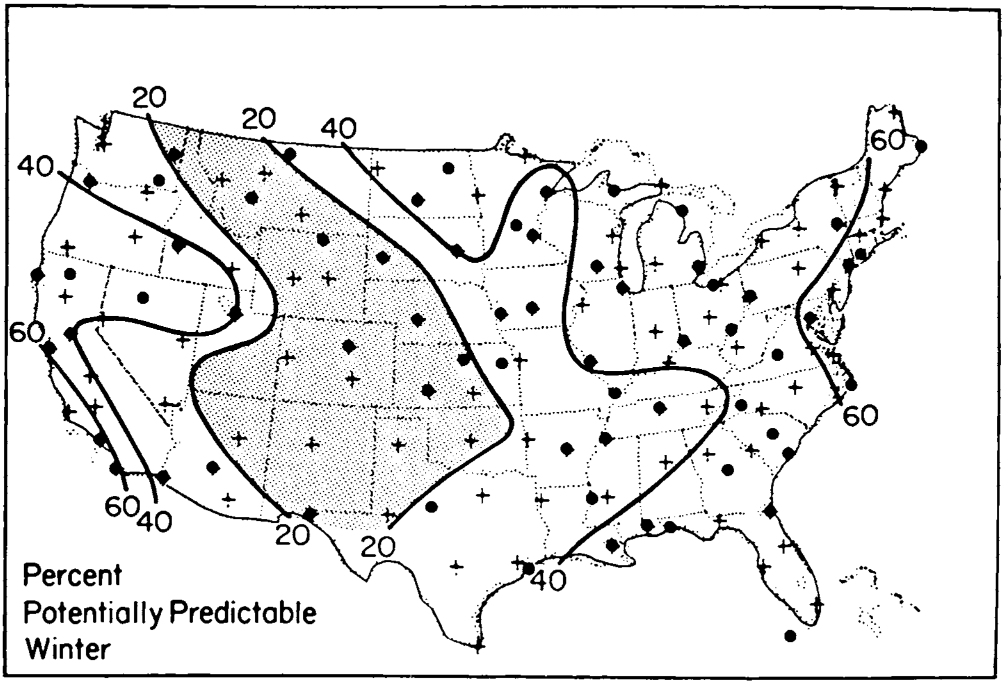

Figure 6. *Predictability map for winter average temperature, in terms of potential percent of variance. From Madden (1981).*

subjective aspects of short range forecasting is well known; the absence of "consensus" therefore probably harms monthly forecasting to some degree.

Assessing the Forecast Probabilities

We first began to issue outlooks in the probabilistic format of Figure 1 in mid-summer, 1982. Let's now examine some verification results for the first three years of the new operation, comprising 72 monthly and 36 seasonal forecasts. Bias diagrams, rather than numerical scores, will tell the most interesting story. On these diagrams forecast probability, grouped at multiples of 5 percent, follows the x-axis; observed frequency, also in multiples of 5 percent, follows the y-axis. The ideal unbiased result would lie on the heavy 45-degree line: i.e., quoted probabilities of 20 percent, taken at all times and stations together, would be verified by the predicted event in just 20 percent of the cases; 30 percent, by 30 percent, and so on.

The monthly temperature outlooks, as seen in Figure 7, performed well. Between 15 and 45 points, where the great bulk of cases lie, cold forecasts show no important bias and Warm forecasts are just a bit conservative, understating the warmth that tended to dominate these three years. The highest probability assignments for both Warm and Cold, few in number, were actually too conservative, as were the lowest probability assignments for Warm.

The monthly precipitation outlooks, in Figure 8, stayed closer to climatology – always within 10 points – and needed to. In a rather wet three years, Heavy was systematically understated and Light overstated, but the general discrimination (slope) within both the Heavy and the Light was about right.

That's the good news. The bad news follows in Figure 9, where not only does the

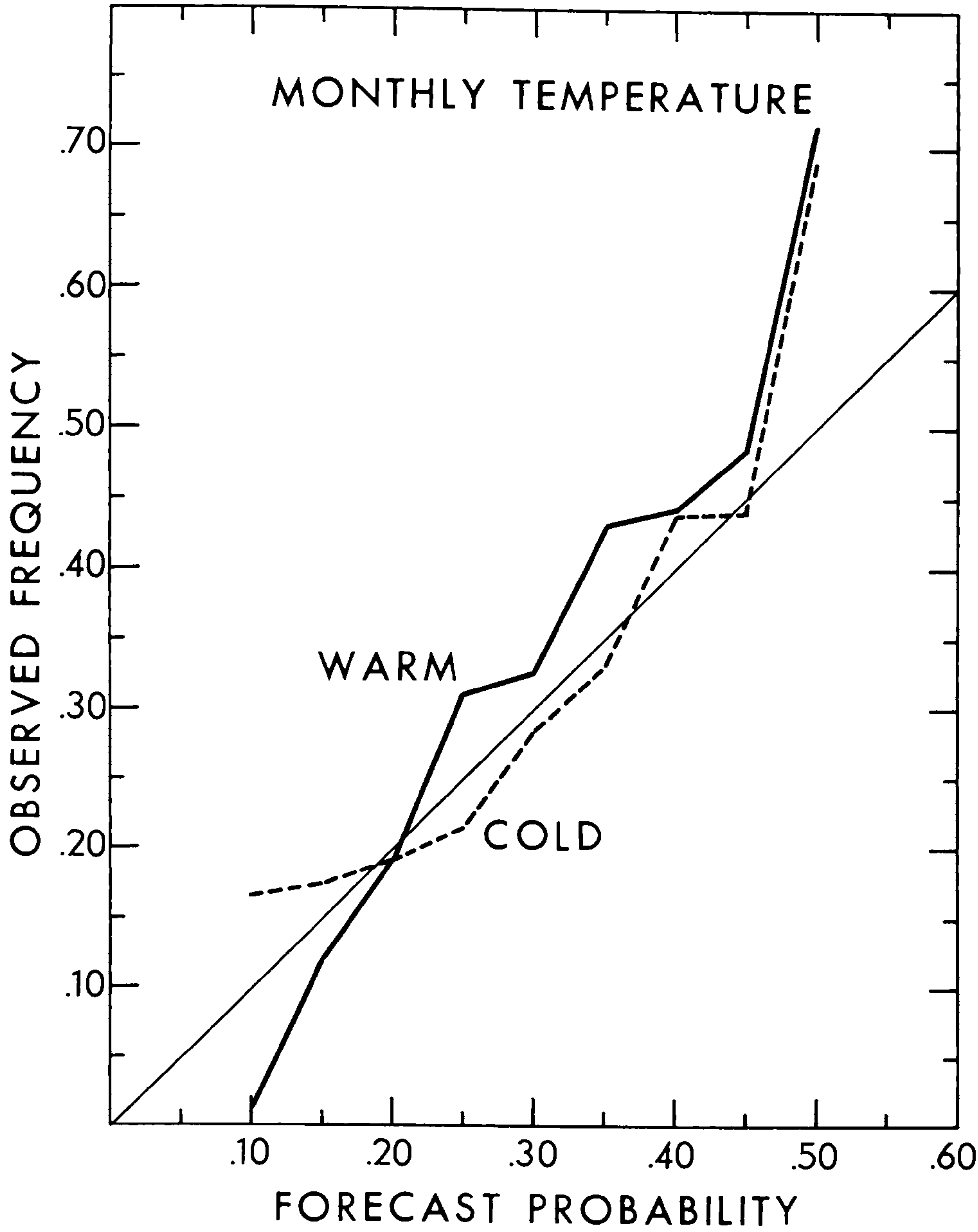

Figure 7. *Verification results for 72 probability forecasts of monthly temperature, 100 stations each forecast. Solid line for probabilities attached to WARM category; dashed, for COLD.*

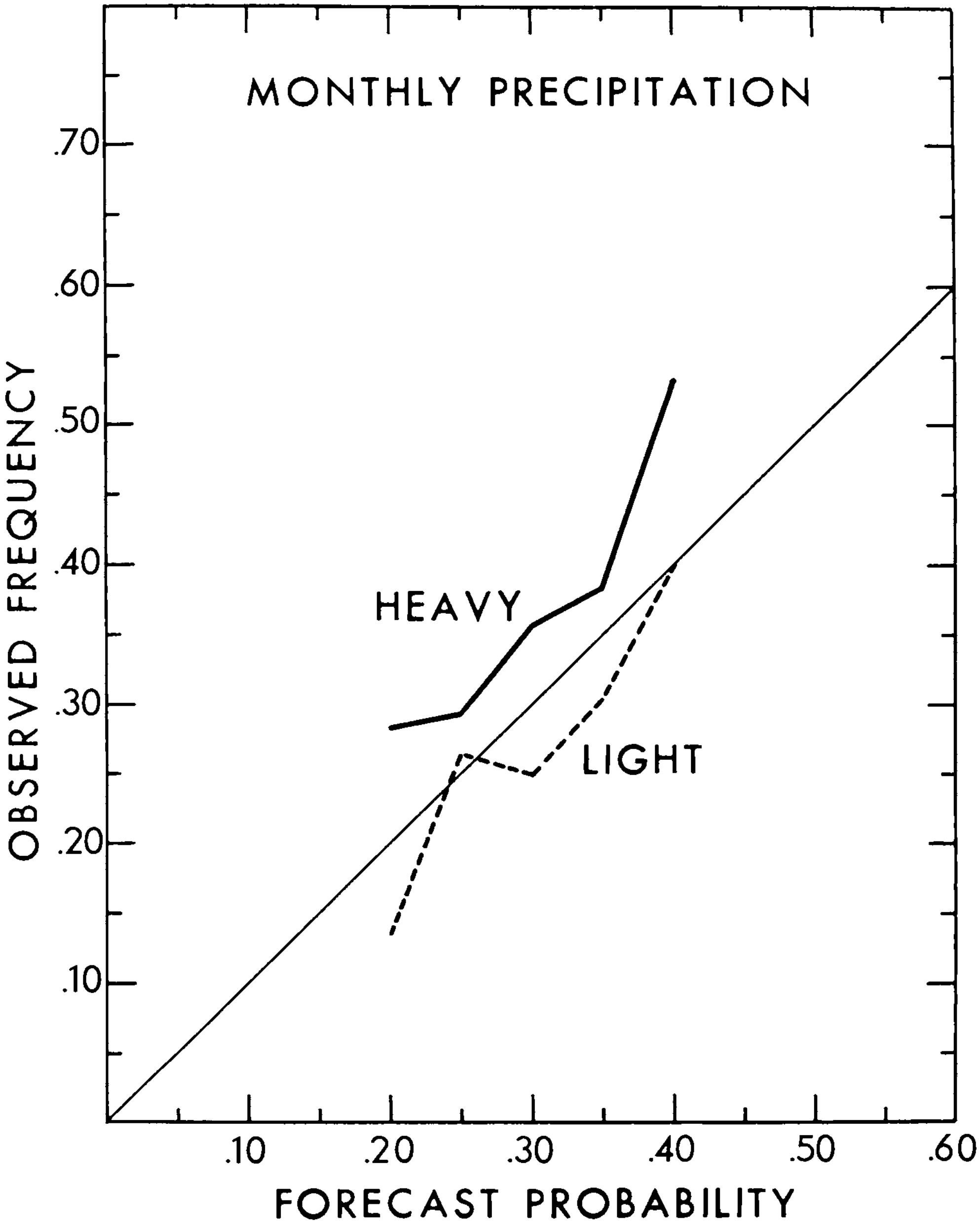

Figure 8. *Same as Figure 7, but for precipitation.*

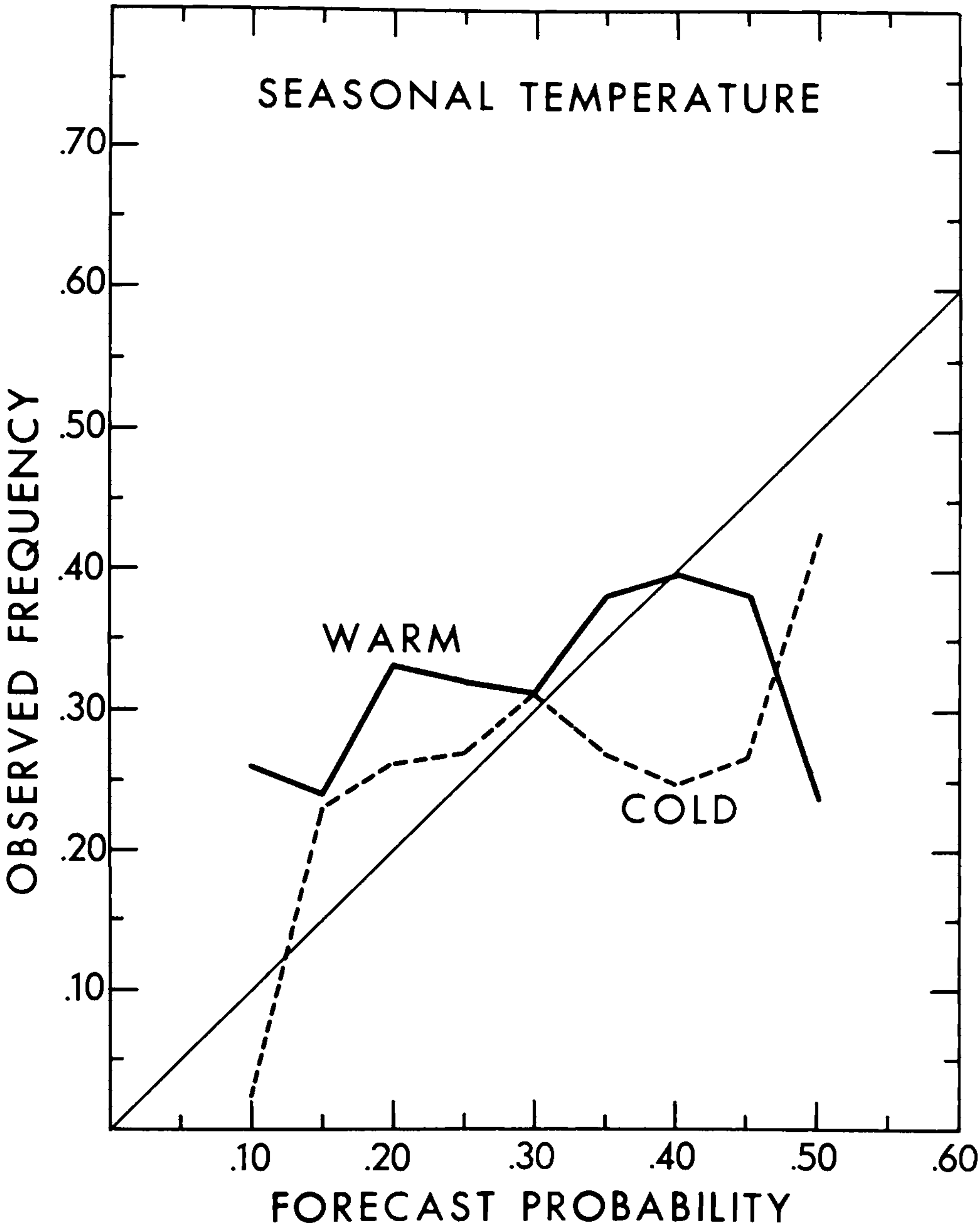

Figure 9. *Same as Figure 7, but for 36 seasonal temperature forecasts.*

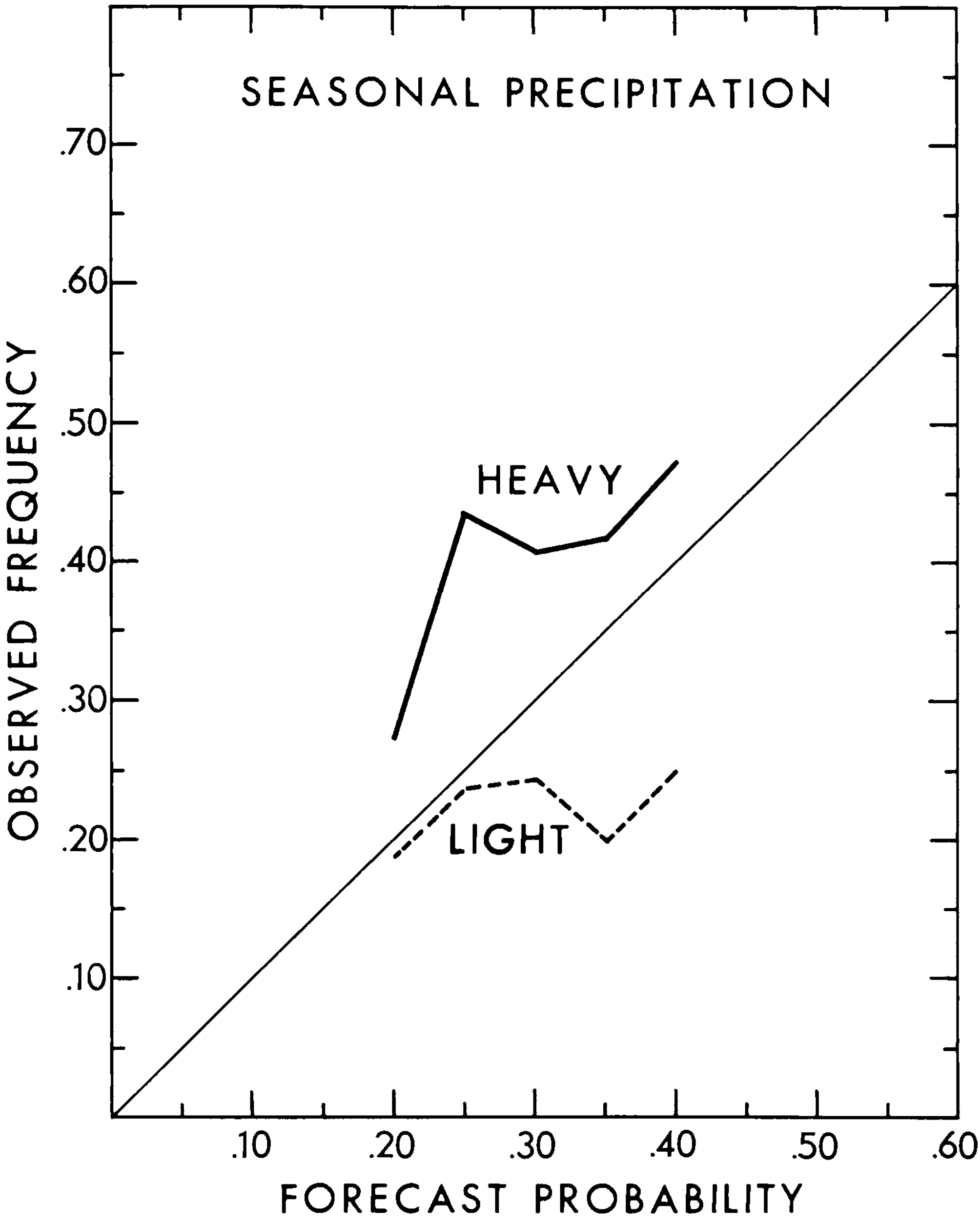

Figure 10. *Same as Figure 9, but for precipitation.*

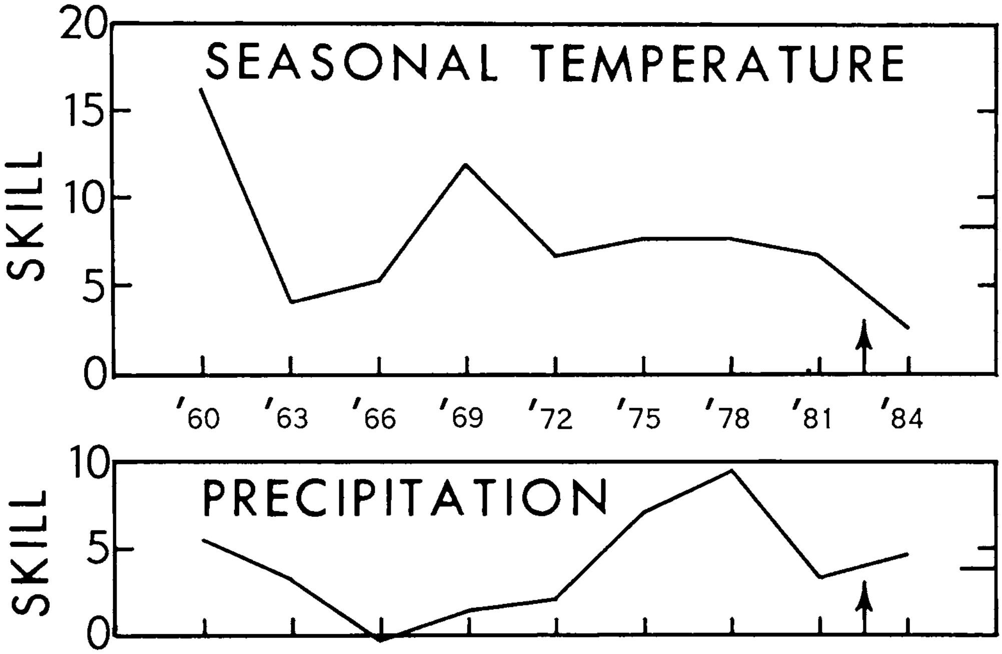

Figure 11. *Verification series, by non-overlapping 3-year averages, of CAC seasonal outlooks. Skill defined as in Figure 5, 100 U.S. stations. Vertical arrow shows beginning of probabilistic format, longer horizontal tick is full-record average.*

general 3-year dominance of Warm over Cold weather go undetected by the seasonal forecasts - just as for monthly forecasts - but the slopes between 15 and 45 flatten toward zero, the slope that implies no skill in discriminating between higher and lower probabilities. The outcome is particularly poor for the Cold forecast points, though the few extreme probabilities at each end perform much better. The extreme Warm probabilities, on the contrary, fail miserably.

Figure 10, seasonal precipitation, does not offer much consolation. As with the monthly outlooks, the general tendency to wetness goes undetected. The slope of the lines near the climatological 30 is slightly negative for both Heavy and Light – no skill there. The end point extremes are better, especially for Heavy.

Some insight on these poor seasonal results can be gained by setting them in historical context in Figure 11. There the standard skill scores for 3-category forecasts are grouped for each three years since the regular preparation of forecasts began in 1959, with points plotted for the middle year of three. The vertical arrow shows the beginning of the probabilistic issuance. The temperature outlooks, after a steady 12-year run of about average performance, dropped during the recent three years to their lowest levels since the work began. The probabilities assigned to them, based on earlier performance, were too strong. The forecasters did not recognize this as a particularly difficult set of cases. The precipitation patterns, however, were drawn with about typical skill. The forecasters – they include the author – simply did not apply the probability table, which is extremely conservative, with adequate rigor.

Some adjustments in the way we assign probabilities are needed. The monthly temperature outlooks, whose standard skill scores have risen a little in recent years with improvements in the 5- and 10-day dynamical model output, can be made bolder in their highest probabilities. Updating the probability tables to use only the most recent eight years should give the proper guidance. More than updating tables will be required for the seasonal outlooks. On the precipitation outlooks, the area encompassed by the 35 percent probability contours should be systematically shrunk, whether or not 40 percent contours are also being applied. On the temperature outlooks, the shrinkage should also include the 40 line. If, as I believe, the recent drop in the temperature pattern skill is an aberration – simple persistence also shows large variations from one 3-year period to another – then these adjustments should suffice. The net effect of such adjustments will manifest itself as a greater variation between sharpness and blandness from one forecast to the next and perhaps across single forecast maps.

DERF and Probabilities

Bureaucrats love acronyms. DERF stands for Dynamical Extended Range Forecasting, and the extension in this case begins where current medium-range forecasting leaves off, at about ten days. The DERF program at the National Meteorological Center has scarcely entered its developmental stage, but because its principal object is improving the usefulness of monthly forecasts, not simply increasing their skill, I want to suggest in this talk how such an improvement might be brought to happen.

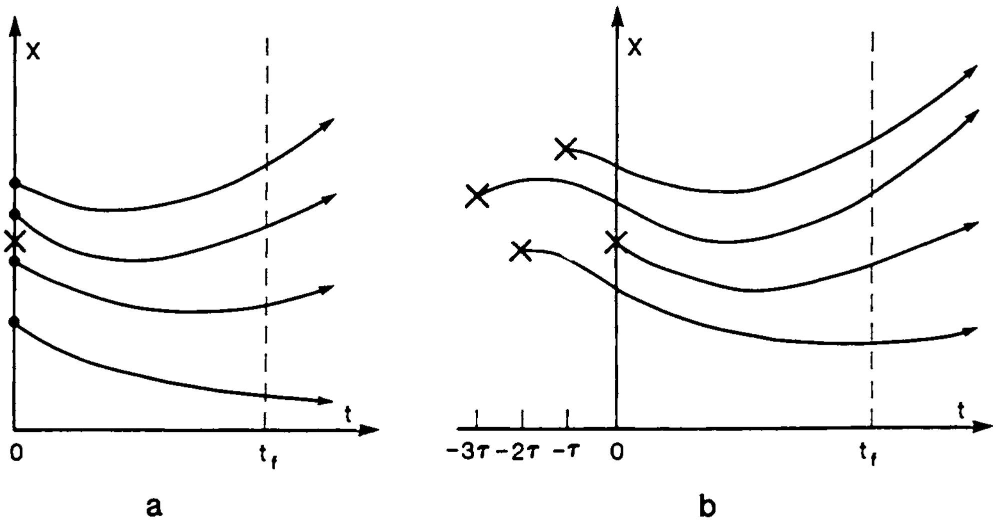

Figure 12. *Schematic time evolutions of the Monte Carlo (a) and Lagged-Average (b) forecast systems, for one variable x over time t, with preceding starts at intervals τ. From Hoffman and Kalnay (1983).*

There are at least five reasons why pushing dynamical models beyond their present cutoff at 10 days is beginning to make more sense than it would have just a few years ago: better models, faster computers, more extensive data, encouraging predictability tests, and practical demonstration cases. Atmospheric general circulation models have evolved to a level of physical completeness, computational precision, and climatological realism that seems appropriate to the monthly prediction problem, and their builders have gained the experience and insight about GCM behavior needed to apply them sensibly. Long model runs on the best computers no longer break the bank. Improvements in the global assimilation and initializing of both conventional and remotely sensed data have taken some of the sting out of that very tough problem, at least for

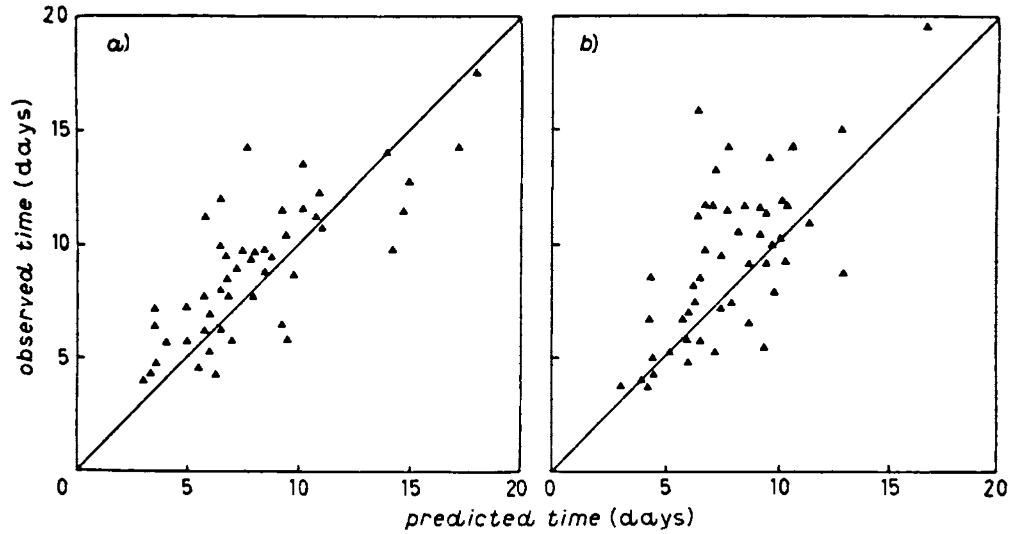

Figure 13. *Predictions by Lagged-Average (a) and Monte Carlo (b) systems of time for "error" growth to half of final rms value. From Kalnay and Livezey (1985).*

atmospheric variables. Model-based predictability trials at NASA's Goddard Laboratories suggest that some mean values or other statistics may remain partly predictable even after practical daily predictability has faded away for certain times of year, regions, and initial conditions. Theoretical and modeling studies of blocking and other dynamical metastabilities suggest a few likely sources of this predictability. Diagnostic studies of large-scale atmospheric oscillations of 30 to 60-day periods in the Tropics and their possible influence in the temperate zones offer another – but more problematic – source. And finally, some pioneers have been at work blazing experimental trails in actual forecast tests. Results from a number of runs of a major GCM at NOAA's Geophysical Fluid Dynamics Laboratory, from a long series of single and multiple runs with a simpler GCM at the U.K. Meteorological Office, and most recently from a few high-resolution GCM runs at the European Center for Medium Range Forecasting all suggest (in rather dissimilar ways) the idea that dynamical extended range forecasting makes sense, that it is not merely tilting at windmills.

Success at applying dynamical prediction on the monthly time-scale might open a number of doors to greater usefulness of the forecast product. Some information about the trends or variability within the month, about the chance of extreme events within the month, or about extreme abnormality of the month as a whole would be greatly welcomed by users of the forecasts and would undoubtedly attract many new users. Opportunities to offer such information will surely be sought as the DERF program evolves. These questions deserve more discussion than I can give them on this occasion. Instead, I want to return to the subject of estimating forecast probabilities.

How can forecasters go beyond simply trying to use dynamical model output to formulate more accurate outlooks to get, in addition, help in assigning probability fields that vary with location, season, and occasion? Can they themselves assess the uncertainties of the model's performance in general or on forecast day? The approach taken by short-range forecasters has been to accumulate working experience of a model's strengths and weaknesses over many actual cases and to calculate an objective version of such experience called Model Output Statistics (MOS). Two forecasts a month instead of two a day make this too long and winding a road to travel. We wouldn't live to arrive.

Could one hope to identify in advance, by theoretically-based or other physical insight, large-scale states or regimes that might offer more or less than usual predictability of some kind? This is a deep and tantalizing research question on which a few ideas exist now, and it will be explored during the DERF program, but we should not rely on useful results. The inductive approach to this question, employing empirical study of forecasts and of verifying observations without any preconceived hypotheses, would do worse than fail – it would almost surely lead us down blind alleys.

What about direct calculation of the growth of initial and boundary-value uncertainty in the model runs by elaboration of the model's own equations, the method called "stochastic dynamic prediction"? That approach appears still to generate too much computation for the biggest available machines to handle economically. The competing idea of a suite of parallel runs from slightly and randomly differing versions of the initial conditions, called Monte Carlo Forecasting (MCF), offers greater efficiency. A variation of Monte Carlo, Lagged-Average Forecasting (LAF), may offer even greater savings, because it draws on single runs made on preceding days in place of small random perturbations applied many times on forecast day. Figure 12, diagrammatically compares the MCF and LAF ideas.

If the dynamical model must be initialized each day for use in medium-range forecasting, as is expected to be the case at NMC, the lagged-average approach becomes still more attractive. A critical question must still be resolved. How many parallel runs are needed to calculate an adequate estimate of the growth of uncertainty (or variance) in the model output? It is easy to imagine that a combined LAF/MCF system may be needed. One may also ask if uncertainties in the specification of the most important underlying boundary conditions should be added to the scheme of computation.

The virtues of these multiple-computation forecast schemes remain to be established on real cases. One low-order model-based predictability experiment, however, gives us some grounds for optimism. Figure 13 shows that both a Lagged-Average scheme (on the left) and a Monte Carlo scheme (on the right) repeatedly produced skillful, though of course imperfect predictions of the rate of growth of uncertainty.

The use of one or both of these dynamical prediction schemes or systems promises, I think, a double reward to monthly forecasters. Even without themselves setting final probabilities, the uncertainty-growth calculations will show forecasters where and when their problem is most difficult and where and when they may expect stability in existing circulation patterns. The same calculations can also signal when the model output should be reduced in weight, turned off, or withdrawn from use in arriving at a "best" prognostic chart of the mean global circulation. This double reward exactly matches the forecasters' double challenge: to make a product that is not only more accurate, but also more useful.

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Kalnay, E. and R. Livezey, 1985: Weather predictability beyond a week: an introductory review. *Turbulence and Predictability in Geophysical Fluid Dynamics and Climate Dynamics*, Soc. Italiana di Fisica, Bologna.

Madden, R.A., 1981: A quantitative approach to long-range prediction. *J. Geophys. Rev.*, **86**, 9817-9825.

The Index Cycle is Alive and Well

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Massachusetts Institute of Technology

Some thirty-five years ago Namias presented a theory of the major variations of the zonal index. Interest in the index cycle declined during the ensuing years of increasing enthusiasm over numerical weather prediction, and some studies appeared to indicate that the variations of the index were random. The index cycle is now recognizable as a manifestation of chaos rather than randomness.

Laboratory and numerical models have produced index cycles with varying degrees of irregularity. Interest in the index cycle and related phenomena such as blocking has recovered as numerical weather prediction has come of age. In one numerical model the index cycle plays an essential supporting role in producing very-long-period climatic variations.

One of the prominent features of the circulation of the atmosphere is the presence of a broad belt of westerly winds in middle latitudes of either hemisphere. As a long-time observer of worldwide weather patterns and their day-to-day, week-to-week, and month-to-month variations, Jerry Namias always took a keen interest in the behavior of the westerlies.

Nearly fifty years ago, in what has become one of the best known meteorological papers ever published, Rossby and collaborators (1939)–Namias was one of the key collaborators–introduced the average wind in the belt between 35°N and 55°N, measured geostrophically as the pressure difference between 35° and 55°, as a conveniently computed index of the strength of the zonal westerlies. Originally defined at sea level, the "zonal index", as it came to be called, was soon afterward evaluated at upper levels also. The same paper introduced Rossby's famous formula, expressing a direct relationship between the strength of the westerlies, i.e., the zonal index, and the speed of progression of the secondary systems.

As recounted by Willett (1948)–another key collaborator–it soon became apparent that the zonal index as defined was sensitive to the latitude as well as the strength of the westerly current, and that, quite aside from displacement rates, the weather structures typically found during periods of high zonal index differed systematically from those characteristic of a low index. It therefore appeared that a good forecast of the zonal index ought to be a useful first step in the preparation of an extended-range forecast.

Although Rossby and his collaborators had used the term "index cycle" to describe the changes which took place as the zonal index fell from a high to a low value and then returned to a high value, it was Namias (1950) who first systematically examined the index cycle with an attempt to determine a physical explanation for its principal features. For the winter season, he found such an explanation in the accumulation of a reservoir of cold air at high latitudes, which would ultimately be released in the form of cold outbreaks. Such outbreaks could be of extended duration, and they would inhibit

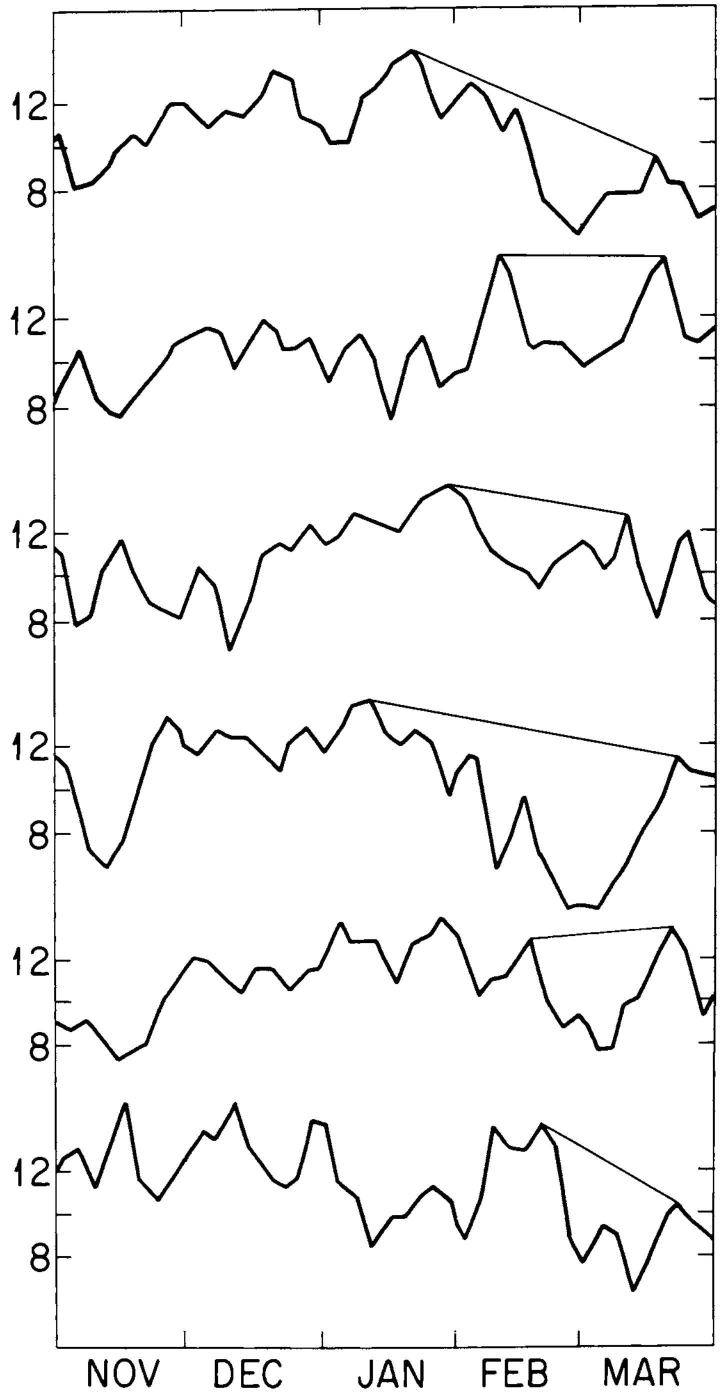

Figure 1. *The 5-day averaged 700-mb zonal index, in m s^{-1}, from November through March during the six successive years 1943-44 to 1948-49. The thin line segments connect the beginning and end of the principal index cycle in each year, as identified by Namias. The figure is based on Fig. 1 of Namias (1950).*

a strong eastward flow, thus producing a low value of the index. A striking finding was the tendency for the index cycle to occur at the same time in different years; Fig. 1, which is condensed from one of the figures which Namias presented, shows the variations of the 700-millibar zonal index during six successive winter periods, and reveals a tendency for the low point in the most pronounced cycle to occur in late February or early March– a time when the supply of cold air is abundant.

During the ensuing years I saw Jerry on frequent occasions, and our conversation would often turn to the zonal westerly winds and the distinctive features of their fluctuations. Thus, when I was recently invited to speak at this symposium on some topic related to Jerry's interests, I more or less automatically turned to the index cycle.

My own interest in the zonal westerlies as a possibly predictable phenomenon began while I was working with Victor Starr, who was at that time investigating the angular-momentum balance of the atmosphere. Starr (1948) had noted that a dominating process in changing the relative angular momentum, proportional at any latitude to $[u]$, was the convergence of the poleward transport of angular momentum, proportional to $-\partial[uv]/\partial y$, and he had hypothesized that the larger-scale eddies might account for a major fraction of the transport. Here u and v are the eastward and northward wind components, y is distance northward, and brackets denote an average about a latitude circle. If such was the case, $[uv]$ could be estimated fairly well from pressure-height observations, using the geostrophic approximation, and measurements of the virtually unmeasureable departures from the geostrophic wind would be unnecessary. When sufficient data became available, I found significant lag correlations between $[uv]$ and $[u]$ at various latitudes (Lorenz, 1952). At U.C.L.A., similar ideas were being pursued by Jacob Bjerknes, and Mintz was conducting studies parallel to mine (Mintz and Kao, 1952).

As a further step in establishing a procedure for predicting $[u]$, I sought to learn what accounted for the changes in $[uv]$. Here any expressions that I could derive involved departures from the geostrophic wind, and no simple physical interpretation appeared to be at hand. I soon found myself concentrating on other matters.

It was just at this time that numerical weather prediction was beginning to reveal its potentialities. In the same year that Namias published his study of the index cycle, Charney, Fjörtoft, and von Neumann (1950) described a moderately successful numerical integration of the barotropic vorticity equation, and the meteorological world became aware that numerical weather prediction was here to stay. The popularity of the new method soon led to a perhaps predictable side effect, however. In making a numerical forecast, one takes a set of numbers representing the initial wind, pressure, and temperature fields, and, regardless of what synoptic structures may be present in these fields, plugs the number into the same program, obtaining another set of numbers representing the forecast. Inevitably the attitude arose that fields rather than structures or phenomena, such as cyclones and fronts or cyclogenesis and frontogenesis, were the essence of the atmospheric state. Whether or not many meteorologists actually changed their views, the views of the meteorological world as a whole did change, because that world was being rapidly augmented by younger scientists who were trained with the new methods and exposed to the new attitudes. In particular, the perceived importance of the zonal index and the index cycle declined. Since Jule Charney, probably more than anyone else, was responsible for making numerical weather prediction a reality, I feel compelled to add that he did not share the attitude of some of his contemporaries toward structures; in fact, his introduction of the geostrophic filtering approximation was prompted by the realization that synoptic forecasters made reasonably good forecasts, based upon the location of structures, without any indication of the departures from the geostrophic wind. Later on he worried because the sets of numbers representing the weather patterns could not resolve fronts, which he regarded as essential elements.

As it became more and more evident that the equations of numerical weather prediction gave fair approximations to reality, I suddenly realized that I had an answer to my problem; to find an expression for the time derivative of $[uv]$ which did not involve geostrophic departures, it was sufficient to use the equations of numerical weather prediction to find the derivatives of u and v, and hence of uv, at each longitude, and then integrate over longitude. Almost simultaneously, however, I realized that my success was somewhat empty; if the zonal index was to be predicted as an aid to predicting the weather pattern, and if one first had to predict the weather pattern in order to predict the zonal index, why bother to predict the zonal index?

The status of the zonal index and the index cycle was probably not elevated by a subsequent study by Enger (1957), who examined 25 years of daily sea-level zonal-index values, and found that they very closely fit a first-order Markov process, with a one-day lag correlation of about 5/6. Such a process was suggestive of randomness. In attempting to assess Enger's results a few years later (Lorenz, 1964), I found that an artificially constructed first-order Markov process with a similar one-lag correlation "looked like" a zonal-index time series; it was hard to tell by inspection which series was which.

In the light of more recent work, it is now apparent that in looking at the index cycle we were not looking at randomnes, in the classical sense. What we were seeing was chaos, as recently defined. The term "chaos", formerly used with a number of connotations, is now used extensively to refer to any deterministically evolving process which may appear to be evolving randomly, particularly if it is observed at rather infrequent intervals. A rapidly increasing body of knowledge now exists, and many examples have been described (cf. Guckenheimer and Holmes, 1983). Quite a few examples come from meteorology. Close inspection of a chaotic time series often reveals certain regularities, even though the series is not periodic. Tests for distinguishing between chaos and pure randomness have been developed; these do not include classical spectral analysis.

Evidence that regularity in the index cycle constituted a physically reasonable assumption was actually accruing well before the apparent randomness received attention. Fultz (1953) had already found that water in a suitably heated rotating cylindrical container in the laboratory would acquire a circulation rather similar to that of the atmosphere, and apparently resulting from similar causes, but the "vacillation" discovered by Hide (1953) in his experiments immediately suggested an index cycle. Vacillation is a phenomenon where the flow pattern, instead of exhibiting the seeming irregularity found in the atmosphere, develops a chain of identical waves superposed on the zonal westerlies, whereupon these waves, in addition to progressing eastward, undergo regular periodic changes in their shape or intensity, completing a cycle in a few simulated days or weeks. The transport of angular momentum effected by these waves necessarily varies with a similar period, and produces periodic variations of the zonal index.

Vacillation did not invariably occur in Hide's apparatus, and different rates of rotation or different amounts of heating would lead to different regimes with various degrees of regularity or irregularity. If the real atmosphere was obviously in a less regular regime than vacillation, there was still no *a priori* reason why it had to be completely irregular.

A few years later I succeeded in producing chaotic "index cycles" with a simple numerical model (Lorenz, 1962). The model was derived by severely truncating the equations of the familiar two-layer geostrophic model so popular in numerical forecasting at that time, and it consisted of 12 coupled ordinary differential equations, whose 12 variables–six representing the 500-milibar wind field and six representing the temperature field–were supposed to capture the gross features of the general circulation. The external heating driving the model circulation varied with latitude and longitude but not with time.

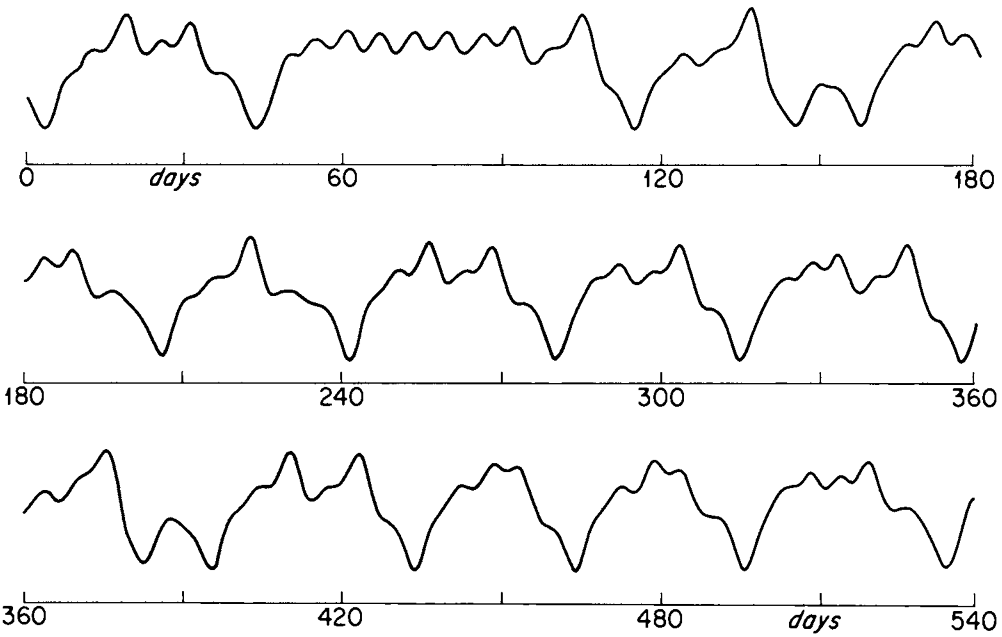

Figure 2. *Variations of the "zonal index" in the 12-variable model of Lorenz (1962) during 18 consecutive months.*

Fig. 2 shows the variations during an 18-month period of one variable, which represents the contrast between the speeds of the westerlies in high and low latitudes; a positive value indicates that the westerlies are displaced northward. Since the real atmosphere zonal index depends upon the latitude as well as the intensity of the westerlies, it seems permissible to call this variable a zonal index. We see some obvious regularities despite the absence of periodicity; this is typical of the simpler examples of chaos. There are episodes of high index, lasting a month or longer, separating briefer periods of low index. The separate episodes share many features, including a rapid initial rise from low index, superposed shorter-period oscillations while the index is high, and a rapid final fall to low index. An unanticipated feature, which hints that "synoptic" methods of forecasting the model's behavior could have been developed, is the occurrence of a second minimum, about two weeks after a first minimum, whenever there has been a very smooth descent from a high maximum to the first minimum; this feature appears twice in Fig. 2.

Numerous "global circulation" models have appeared in more recent years. Some are even simpler than the 12-variable model which we have described, while others, including the largest models used in operational weather forecasting, possess several hundred thousand variables. These models produce their own index cycles. Not surprisingly, the cycles in some instances are as regular as vacillation, and in others are seemingly as irregular, and perhaps actually as irregular, as the real atmospheric index cycle. They are nevertheless manifestations of chaos rather than pure randomness, and regularities may be present even when they are not obvious. The real index cycle appears to exhibit some regularities, including the seasonal preference noted by Namias, and it is not unreasonable to conjecture that additional regularities might be revealed by suitable analysis schemes.

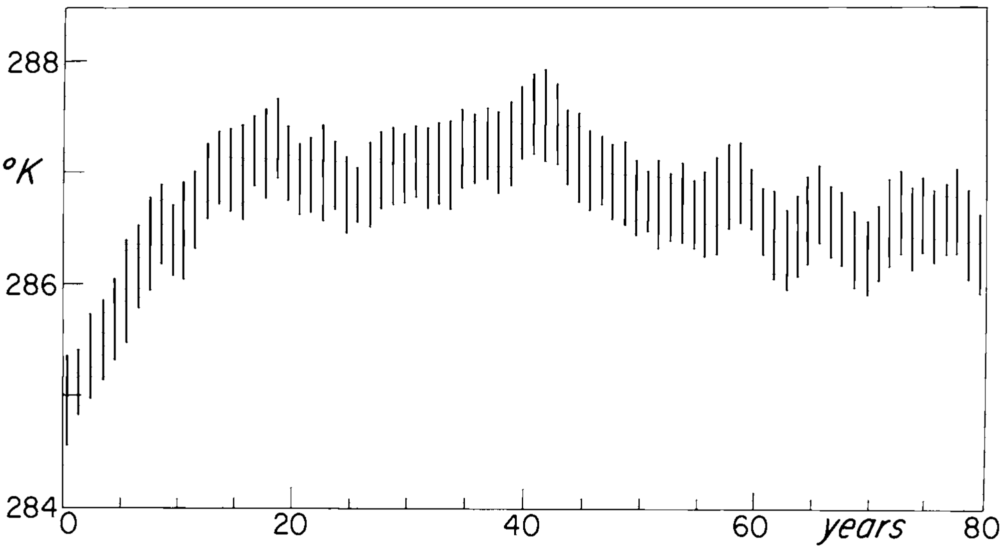

Figure 3. *Ranges of the globally averaged temperature in the 27- variable model of Lorenz (1984) during 80 consecutive years, starting from sub- equilibrium conditions. The numerical values of the constants are those used in Tables 2-4 of Lorenz (1984), except that the lapse rate is 7/10 of the dry- adiabatic, the heat capacity of the oceanic layer is 10 times that of the atmosphere, and* $T^* = 273.0 + 10.0\phi_3$.

Along with the coming of age of numerical weather prediction has come the realization that further improvements may depend more and more on skillful engineering and less and less on new scientific breakthroughs. Scientific studies which have not avoided numerical methods altogether have therefore tended to take already-developed models and apply them to specific problems. Inevitably many of the recent studies have dealt with structures and phenomena.

One phenomenon which has received much attention in the past few years is blocking. This has traditionally been considered a low-index phenomenon, and its occurrence is presumably linked in some manner to the index cycle. One may argue as to whether the low index is primarily a cause or an effect. Nevertheless, as a phenomenon which may be observed in the atmosphere, and produced and controlled in the laboratory and the computer, the index cycle appears to be quite healthy.

Let me conclude by describing a numerically produced phenomenon in which the index cycle seems to play a crucial supporting role. This is the long-period variability of the globally averaged temperature as observed in a 27-variable model. The model, described elsewhere in detail (Lorenz, 1984), is an extension of the 12-variable model which produced Fig. 2, and, in addition to a thirteenth variable T_0 representing the globally-averaged sea-level temperature, contains seven variables representing the moisture field and seven representing the sea-surface temperature. A key feature is the use of the total dew point–the value which the dew point would assume if all the water were converted to vapor–as the moisture variable; an auxiliary equation determines the apportionment of the water between vapor and liquid, and guarantees that supersaturation will not occur. The model includes viscous and thermal damping, evaporation and precipitation, and radiative heating and cooling. The cloud cover is a preassigned function of the relative humidity, and the albedo depends in turn upon the cloud cover.

A feature which was not intentionally introduced and not anticipated is a cloud-albedo feedback process; a positive temperature disturbance produces a drying of the atmosphere, which reduces the cloud cover and the accompanying albedo and allows the sun to produce further heating. As a consequence the approach to equilibrium from an unbalanced state is an extremely slow process, often requiring several years. The rather high heat capacity of the upper oceanic layer contributes to the slowness.

In Fig. 3 the vertical bars show the ranges of T_0 during 80 consecutive years, in a solution beginning with a sub-equilibrium temperature. Baroclinic activity of variable intensity occurs throughout the period, and is carried explicitly in the model, which is integrated in 90-minute time steps. Although the ranges in successive years show considerable overlap, there is a gradual progression toward apparent equilibrium, lasting about 20 years. After year 40 a gradual downward progression develops, suggesting that equilibrium may not have been reached after all.

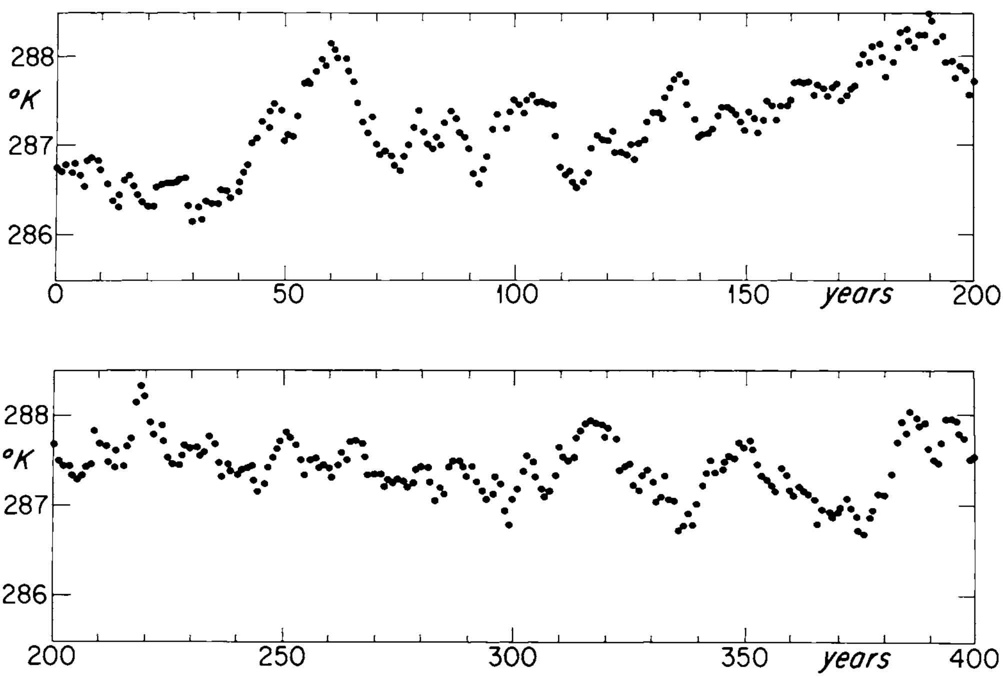

Figure 4. *Annual mean values of the globally averaged temperature during 400 consecutive years in a continuation of Fig. 3. The initial state follows the initial state of Fig. 3 by 50 years.*

Fig. 4 shows annual mean values of T_0 for a 400-year period; the first 30 years are the final 30 years of Fig. 3. Evidently the downward trend does not continue, and is followed by a rise of more than two degrees to an ultimate peak 160 years later. Thereafter fluctuations continue, and no unequivocal equilibrium value of T_0 can be identified.

A one- or two-degree local temperature change is not a spectacular event. The significance of Fig. 4 is that the globally and annually averaged temperature is changing by this amount, and corresponding changes in the real atmosphere seem to be of comparable magnitude. In all likelihood an overall warming or cooling of the real atmosphere resembling what appears in Fig. 4, say from year 30 to year 60, year 115 to year 135, or year 350 to year 375, would, once detected, be interpreted as a climatic change by many observers, and attempts would be made to determine the cause.

In Fig. 4 the changes simply represent the model's natural variability; there are no variations in external conditions. However, the nonlinearity associated with the moist processes leads to weak interactions between the mean temperature and the cross-latitude temperature gradient. If these interactions were suppressed, T_0 would in due time approach equilibrium. It appears, then, that the variability of the temperature gradient and its associated westerly wind current, i.e., the index cycle, is acting as a weak quasi-random forcing upon the mean temperature, producing the "climatic" variations.

What does this result tell us about the real atmosphere? Certainly it does not say that real climatic fluctuations are produced by a cloud-albedo feedback process; we do not even know whether such a process actually takes place. It does tell us that the very-long-period fluctuations may depend upon features which seem so secondary that they are often omitted in theoretical investigations. Instead of clouds, the key feature might be ice, or perhaps vegetation. It might even be some feature which we have not yet dreamed of examining.

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Certificate of Achievement

Alan D. Hecht
National Climate Program Office

If you could look up "Namias" in the dictionary you would find the following definition: "A man who gives good reasons for any long range forecast, and even better reasons for why it fails." This is the character of the man who is an infinite source of good ideas, and who thinks fast on his feet.

One of the consequences of the political and human events of the 1970s, which Bill Nierenberg described this morning, was the establishment of the National Climate Program by Congress in 1978. This act has a far reaching provision, namely the establishment of experimental forecast centers. In 1978 few people, other than Jerome Namias, were optimistic that much progress could be made in this area. Events since then have generated a new sense of enthusiasm that climate prediction under certain circumstances is possible. The traditional bounds of prediction that never held Jerome back are now being pushed back by a growing effort in the field.

While the National Climate Program Act had this far reaching provision, the idea was not new. The first experimental forecast center was established at MIT under Rossby in 1936. The New York Times reported "The Weather Bureau has enlisted aid of experts from several universities in starting a study of long range forecasting." Namias was a young member of this team and began in a short time to pioneer the experimental 5 day forecast. It is fitting that nearly 45 years later the first experimental climate forecast center to be established under the NCP was established at Scripps under Namias and his colleagues. In 50 years we have come full circle. Today we are, of course, in a better position to know how difficult this task is, but success seems more possible now than ever before. Other experimental centers are now operating and the number of people addressing the problem is increasing. I remember when Namias was one of the few who was optimistic about such studies.

In the geological science we had a giant, like Namias, who pioneered quarternary studies. The late Richard Foster Flint made many contributions to climate history. In one of his late essays, *Three Theories in Time*, he wrote that he had seen in his lifetime the development of two major paradigms which had revolutionized the biological and earth sciences. They were the theories of evolution and plate tectonics. Flint suggested that the next great evolution would be in atmospheric and ocean sciences by a theory of climate which would explain the complex interactions of the atmosphere and ocean. We are closer now that ever before to such an understanding. The national and world climate research programs are now concentrating on many of the ideas of global air-sea and ice interactions that Namias proposed in his papers between 1936 and 1963.

Progress in climate research has followed a gradual slope upward, marked now and then by points which change the rate of this progress. Namias in his long career has contributed many times to increasing the slope of this increase in knowledge.

Finally on a personal note, and based on my years in the trenches of grant support at NSF, I learned that Namias is always a scholar, but more importantly is always a gentleman.

I have the pleasure to present to you this certification of achievement, signed by both the new Administrator of NOAA, Dr. Anthony Calio and the Secretary of Commerce, Mr. Malcolm Baldrige, for your pioneering efforts in climate forecasting and for your long service to NOAA and the DOC.

Happy Birthday, Jerome Namias.







Jerome Namias

Bob Born, Carolyn Baxter, Margaret Robinson, Rick Salmon

Chester Newton

Joe Smagorinsky, James Coakley, Larry Gates

James O'Brien, Tim Barnett, Bob Cherwin

Ben Volcani, Andy Benson

Mike Ghil, Jim Coakley, Rick Anthes, Larry Gates

Joe Smagorinsky, Jerome Namias

Harriet Newton, Arnt Eliassen

Henry Diaz, Harry van Loon, Don Gilman

(front) Arnt Eliassen, Chester Newton
(standing) Morry Neiburger
(rear, to right) Jerome Namias, Joe Smagorinsky
(rear, to left) Don Gilman

Morry Neiburger, Akio Arakawa

Don Gilman

Larry Gates, Sekharipur Venkateswaran







Jerome Namias Bob Haney

Walter Munk Rick Anthes Arnt Eliassen

Mike Ghil Bill Sprigg

Bill Huston, Dave Fultz, Bill Sprigg   Duane Stevens

Bob Knox   Sargon Tont