History of the Atmospheric Sciences

Editor's Note. As a contribution to the celebration of the Bicentennial of the United States in 1976, a session of the program of the 56th Annual Meeting of the AMS, 19–22 January 1976, Philadelphia, Pa., was devoted to the theme, "History of the Atmospheric Sciences." Under the Chairmanship of Prof. Bernhard Haurwitz, the session included five addresses: "The History of Meteorology from Aristotle to the 19th Century—Some Highlights," by H. Howard Frisinger; "A History of Prevailing Ideas about the General Circulation of the Atmosphere," by Edward N. Lorenz; "The Genesis of the Thermal Theory of Cyclones," by Gisela Kutzbach; "The History of the Polar Front and Air Mass Concepts in the United States—An Eyewitness Account," by Jerome Namias; and "A History of Numerical Weather Prediction in the United States," by Philip D. Thompson. A subsequent attempt to publish these addresses, along with those from a historical session at the Annual Meeting of the American Association for the Advancement of Science, 18–24 February 1976, Boston, Mass., as an AMS Historical Monograph did not succeed. In the meantime, the larger works on which two of the addresses were based have been published separately as AMS Historical Monographs:

H. Howard Frisinger, *The History of Meteorology: to 1800*, American Meteorological Society and Science History Publications, New York, 1977, 148 pp. (Second printing, AMS, Boston, Mass., 1983)

Gisela Kutzbach, *The Thermal Theory of Cyclones, A History of Meteorological Thought in the Nineteenth Century*, American Meteorological Society, Boston, Mass., 1979, 255 pp.

The remaining three addresses given by Lorenz, Namias, and Thompson in the AMS session on "History of the Atmospheric Sciences" in Philadelphia in 1976 are published on these pages, where they become a permanent part of AMS historical archives.

Except for minor corrections by one of the authors and some minor editing by AMS, the addresses are published as given in the authors' 1976 manuscripts. They should be read from that time perspective, even though no major changes in what the authors had to say in 1976 seem to be necessary when read from the 1983 perspective.

A History of Prevailing Ideas about the General Circulation of the Atmosphere

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Abstract

During the past three centuries, the prevailing ideas about the general circulation of the earth's atmosphere have evolved in a stepwise manner. Early in each step, a new theoretical idea is formulated. Late in each step, the idea gains general acceptance, but, more or less concurrently, new observations show that the idea is wrong. An account of three steps and part of a fourth is presented.

The general circulation of the earth's atmosphere has been the subject of many excellent studies during the last three centuries. Throughout much of this period, the problem of the general circulation has been looked upon as one not yet solved, but offering a readily understandable qualitative solution. This situation has undoubtedly contributed to its popularity as a research problem. The continual appearance of new ideas has been interspersed with histories of these ideas; one could almost write a history of histories of the general circulation. Some of these accounts have appeared as introductions to presentations of new results (e.g., Hildebrandsson and Teisserenc de Bort, 1900). Others are found in textbooks or survey articles (e.g., Hann, 1901). Perhaps the most readable history of all is contained in the Bakerian Lecture of Thomson (1892).

The present summary is based upon a rather detailed historical account prepared a decade ago (Lorenz, 1967; see pp.

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1-4, 59–78). The reader is referred to that account for details not found in this summary.

The prevailing ideas have evolved in a manner that appears to be far from random. Indeed, to a present-day dynamic meteorologist, an account of the development of these ideas is suggestive of a giant stepwise numerical integration, with time steps of half a century or longer. At the beginning of each step, certain ideas appear more or less as established facts in the standard texts, but are questioned by the avant-garde. Within each step there occurs a formulation of new theoretical ideas, an interval in which these ideas are rejected or simply ignored, an interval of fairly general acceptance, a more or less concurrent discovery of observational facts that contradict the new theory, an interval in which these observations are ignored or questioned, and a final acceptance of the new observations and a rejection of the theory by the new avant-garde. To many readers, our time steps will be more suggestive of innings.

The initial time in our summary is the early 18th century. The generally accepted theory of the trade winds had been formulated by the astronomer Edmund Halley (1686), who is well remembered today for the comet that bears his name. Halley had carefully noted the presence of similar wind systems in three separate oceans, and had sought a common explanation. He identified solar heating as the driving force; this he believed would cause the air to rise in low latitudes and sink in high latitudes, whence the equatorward drift in the trade winds, and a poleward drift aloft, would follow from mass continuity. He maintained that the westward drift in the trades would likewise result from the tendency of the air to follow the sun.

Here he seems to have made an error in logic, which is as common in qualitative reasoning today as it was then; he failed to distinguish fully between a quantity and its time derivative. Thus "following the sun" appears to mean moving toward the sun when applied to the north–south motion, but it means moving in the direction in which the sun is moving when applied to the east–west motion. Halley did not discuss the middle latitude westerlies, and his work cannot be equated to a theory of the global circulation.

The opening event in the first step was the famous paper of George Hadley (1735). This account has been retold so many times that another repetition appears superfluous, but a few points should be mentioned. First of all, it introduces a new physical concept—the tendency of air to retain its present absolute angular momentum as it moves over a portion of the earth's surface having greater or less angular momentum. This tendency is precisely what we now call the east–west component of the Coriolis force.

Hadley had accepted Halley's ideas regarding the north-south motion. He therefore deduced that the equatorward-moving air at low levels would be deflected westward, while the air returning poleward at higher levels would be deflected eastward. He invoked friction to explain why the easterly and westerly winds would not be much stronger than observed, and then noted that the presence of low-latitude easterlies, with their westward frictional drag on the earth, demanded the existence of westerlies at other latitudes, with an opposing drag. His account thus embraces the concept of a global circulation, whose various branches cannot be explained independently of one another. Figure 1a shows this circulation schematically; a single thermally direct cell occupies each hemisphere.

For a number of years, Hadley's paper remained virtually unknown—so much so, in fact, that the idea was rediscovered first by Immanuel Kant (1756), also without attracting attention, and later by John Dalton (1793). Perhaps it was partly because Dalton subsequently learned, and acknowledged, that he had been completely anticipated by Hadley that Hadley's paper finally gained notice. By this time, however, new observations were becoming more plentiful. At about the time that Hadley's theory became the generally accepted one, the observations revealed that the theory was wrong; the surface westerly winds in middle latitudes possessed a poleward drift, rather than an equatorward drift as the theory demanded.

The second step began with various attempts to reconcile Hadley's physical reasoning with the new observations. The works with the most lasting influence were the rather similar ones of Thomson (1857) and Ferrel (1859). Like Hadley's work, they were founded upon a new physical concept—the presence of a greater, or smaller, centrifugal force acting upon air that rotates more rapidly, or less rapidly, than the underlying earth. This tendency is, of course, what we now call the north–south component of the Coriolis force.

Thomson and Ferrel accepted Hadley's ideas regarding the lower latitudes and regarding higher levels in the remaining latitudes. They likewise invoked friction, and noted that this would cause the westerlies to decrease very rapidly with decreasing height through a shallow layer near the surface, while, in view of hydrostatic considerations, the northward pressure gradient would decrease only slowly. There would therefore be a substantial unbalance of forces near the surface, causing the lowest layers of air to proceed poleward, in agreement with observations.

Figure 1b shows Thomson's circulation schematically. Ferrel differed with Thomson principally in not extending his low-level thermally indirect cells into the polar regions.

Thanks largely to Ferrel's continued writings, the new ideas gained attention much more quickly than had Hadley's a century earlier. But new observations were also accumulating more rapidly. At the end of the 19th century, just as the new view of the circulation was becoming generally accepted, the International Meteorological Organization was completing a study of upper-level winds, deduced from the motions of high clouds (see Hildebrandsson and Teisserenc de Bort, 1900). This study revealed that there was no high-level poleward current from tropical to temperate latitudes, as Thomson's and Ferrel's theories, and also Hadley's, would have demanded.

Early in the third step, the ideas assumed divergent paths. A feature of the generally accepted theories had been a complete symmetry of the circulation pattern with respect to the earth's axis. This does not mean that the proponents of these theories had been unaware of the prevalence of intense storms and other departures from axial symmetry. Ferrel even wrote about the general circulation and storms in separate paragraphs of the same paper. But he never suggested that the general circulation and the storms might somehow influence one another.

Along one path, some investigators added more and more cells to Thomson's picture of the circulation so as to establish

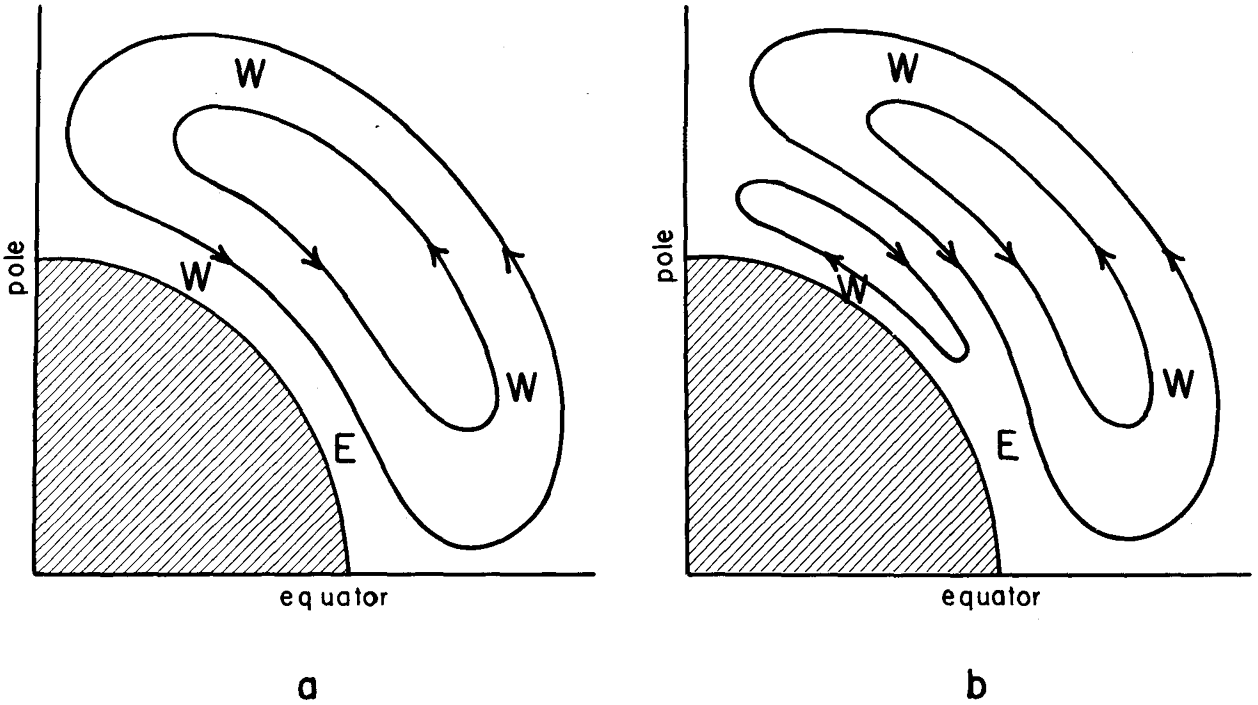

FIG. 1.a) Equator-to-pole cross section of the earth and the atmosphere, showing the symmetric circulation pictured by Hadley (1735). Streamlines indicate north-south and vertical motion. Letter E or W indicates motion from the east or west. b) The same, for the symmetric circulation pictured by Thomson (1857).

compatibility with the new observations without destroying axial symmetry. One after another, those patterns that were not obviously physically impossible were found to disagree with still newer observations.

Along the other path, the idea began to emerge that the general circulation, which by now had come to mean the axially symmetric portion of the circulation, could not be explained independently of the storms that were superposed upon it. The idea was stressed by Bigelow (1902), who pictured an asymmetric circulation in higher latitudes, with cold and warm equatorial and poleward currents flowing side by side, and with storms developing as these currents interacted. It had been realized that the excess energy received from the sun in low latitudes had to be transported within the atmosphere to higher latitudes before being discharged, and the uniform upper-level poleward current had supposedly formed the means for this transport. When this current was found not to exist, an alternative transport mechanism had to be found. Bigelow maintained that the cold and warm adjacent currents provided the mechanism.

At this point, we must turn back a full step to the ideas of the eminent meteorologist Dove (1837). He accepted Hadley's ideas regarding the lower latitudes, but described the middle-latitude circulation as consisting of alternate longitudes of north winds and south winds. Dove's "winds" appear to be the same as what we now call polar and tropical air masses. He regarded the migratory storms as originating from a conflict of these winds. His description resembles the one that we have attributed to Bigelow.

The reader may well inquire why we waited until this point to introduce Dove's advanced ideas. It is true that we could write a tidier story by pretending that Dove's work never existed, but this is not sufficient reason for doing so. Many historical accounts appearing in the later 19th century did, in fact, ignore Dove altogether. We may guess why they did so after examining the original edition of the excellent treatise of Hann (1901), who made no mention of Dove in his chapter on the general circulation, but described his work in detail in the following chapter on storms. Evidently the phenomena that Dove had so carefully observed were not considered by 19th-century meteorologists to be the general circulation. As a consequence, his work failed to influence subsequent general circulation studies. Dove had not proposed, as Bigelow later did, that the north and south winds formed the principal mechanism for the heat transport. This is understandable; there was no reason then to suspect that the upper-level poleward current was absent.

The idea that asymmetries were essential to the general circulation received only minor support until Defant (1921) proposed that the motions in middle latitudes were simply a manifestation of turbulence on a very large scale. Defant went beyond Bigelow by applying the results of turbulence theory to estimate the amount of heat that would be transported poleward by turbulent eddies with diameters of thousands of kilometers. He found that this agreed well with the required transport and concluded that his ideas were confirmed.

Asymmetries, whether or not they are regarded as turbulence, require an origin, and a suitable explanation was provided by V. Bjerknes (1937). He sought the circulation pattern that would develop if it were forced to remain symmetric, and concluded that it would look much like the patterns favored by Thomson and Ferrel. He maintained, however, that these patterns would be unstable with respect to asymmetric disturbances of small amplitude. Fully developed asymmetries would therefore characterize the actual circulation.

Defant's description of the cyclones and anticyclones as turbulence, with its connotation of randomness, seems to have encountered some resistance. It should be remembered that the meteorologists who studied the general circulation and those who studied cyclones were not disjoint groups; to a considerable extent they were the same persons. Having dealt with cyclones in detail, and having identified certain regularities in their structures, they may have been reluctant to look upon them now as mere random eddies. Nevertheless, the idea that cyclones, like random eddies, should diffuse heat, and thus act to smooth out the symmetric portion of the temperature field, met with considerable favor.

This idea naturally extended itself to the motion field. It was pursued most vigorously by Rossby (1941, 1947). By postulating a diffusion of momentum, and later a diffusion of vorticity, Rossby was able to deduce flow patterns that agreed fairly well with reality. For a time his ideas were the ones quoted in the standard texts.

The refutation had its origin in the work of Jeffreys (1926). It had been realized that angular momentum as well as heat had to be transported poleward within the atmosphere, and the absence of a uniform upper-level poleward current, which had been thought to provide the mechanism, was posing further problems. Jeffreys proposed that this transport, like that of heat, was accomplished by the asymmetric eddies.

His ideas were not received enthusiastically. The transports that were deduced by applying turbulence theory were quite unlike those needed to fulfill the global balance requirements.

Following World War II, J. Bjerknes (1948), Priestley (1949), and Starr (1948) independently proposed that upper-level observations had now become plentiful enough for the direct evaluation of transports of angular momentum on a day-by-day basis. The ensuing computations confirmed what Jeffreys had maintained; throughout much of the atmosphere angular momentum was actually transported from latitudes of low to latitudes of high angular velocity, in opposition to what was demanded by turbulence theory. The third step had been completed.

It is more difficult to view the fourth step, which is currently in progress, from a historical point of view. A prevailing idea, clearly stated by Eady (1950), appears to be that cyclones and other asymmetries should conform to baroclinic-stability theory. Charney (1959) was able to deduce a fairly realistic circulation by postulating that the asymmetric disturbances, although of finite size, would assume the same shapes as the disturbances that, while of infinitesimal size, would amplify most rapidly. Work along these lines continues.

If our own most recent view of the general circulation (Lorenz, 1969) is accurate, we may be nearing the end of the fourth step. We have pictured a circulation that, if not easily explainable in simple sentences (except by calling it a baroclinic-instability phenomenon), can at least be duplicated in its main features by numerical solutions of fairly realistic approximations to the governing dynamic equations. The statistics that have been evaluated from these solutions compare fairly well with those determined from real atmospheric data. There is a comfortable feeling that the problem is nearly solved.

We may therefore pause and ask whether this step will be completed in the manner of the last three. Will the next decades see new observational data that will disprove our present ideas? It would be difficult to show that this cannot happen.

Our current knowledge of the role of the various phases of water in the atmosphere is somewhat incomplete; eventually it must encompass both thermodynamic and radiational effects. We do not fully understand the interconnections between the tropics, which contain the bulk of the water, and the remaining latitudes. Satellite observations have revealed various features, such as a frequent continuum of clouds extending northeastward from the tropical Pacific into the central United States, which were not previously recognized. Perhaps near the end of the 20th century we shall suddenly discover that we are beginning the fifth step.

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The History of Polar Front and Air Mass Concepts in the United States—An Eyewitness Account

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This report differs from the scholarly paper preceding it in this session. Mine is an "eyewitness" account, and I would like to list my credentials—because an eyewitness can be quite biased. Part of the period that I have observed is not to the credit of American meteorology. The period had some villains, some heroes, and, later on, some great developments.

Some of the younger people in this audience may not be aware that my interest in meteorology began in high school in the late 1920s. At this time I read all the books on meteorology I could find in the public library; these included the works of Redfield, Espy, Milham, and many others. Even though it was the depth of the Depression, I landed a job with the Smithsonian Institution in Washington in 1930–31 to gather data for World Weather Records. My place of work was at the U.S. Weather Bureau where, not being a Bureau employee, I didn't have to follow the "party line." I met many people and frequently found evidence of scientific backwardness, noticing particularly the neglect of many new works that I discovered in the excellent Weather Bureau Library. It was in this library that I first found the epoch-making works of the Norwegian (or Bergen) School of Meteorology. In 1932, proceeding in a more conventional fashion, I studied at M.I.T., but visited Washington from time to time, and at M.I.T. I saw a new chapter of meteorological history being inaugurated in the mid-thirties.

At the start I must say something about the Norwegian birthplace of polar front and air mass concepts. This development had continuity over a long period of time, but it took great people of the stature of Vilhelm Bjerknes and his son, Jacob (Jack) Bjerknes, and also a number of others, including some Americans, to bring it to fruition. I'll focus chiefly upon the history of polar front and air mass concepts in the United States.

Figure 1 is the model that set in motion a revolution in synoptic meteorology. It was developed in the years 1918–19 at the end of World War I when Norway was deprived of far-flung observations, but still had to make forecasts for her great fishing fleet. Figure 1 is the model developed at the Bergen School by Bjerknes, Sølberg, and colleagues (Bjerknes, 1919; Bjerknes and Sølberg, 1923). Incidentally, this figure appears on the front cover of a just-published volume of selected papers of Jacob Bjerknes (1975). It is a classic volume about two inches thick. I recommend it to all who are interested in meterological history and particularly in Jacob Bjerknes' tremendous contributions. His untimely death was costly to world meteorology, since he was productive to the last at the age of 77. Bjerknes's "model cyclone" consists of a warm sector, a cold front and a warm front, and associated vertical cross sections. It appears in just about every meteorological text book. Of course, it has been found that nature is not so simple. Among the first group to learn this were airline meteorologists, who found that the structure of the cloud and rain systems are much more complex. Now radar and satellite meteorologists have shown that this figure is often an oversimplification. Nevertheless, the core of this model still retains its integrity and is used as a primary tool by weather forecasters throughout the world. It is amazing to reflect that something developed as early as 1918 and 1919 should stand the test of time, so that in every weather forecasting office frontal analysis is still practiced. It is a development unlikely to pass from the scene, in spite of the fact that numerical predictions have taken over many aspects of forecasting.

A diagram much like Fig. 1 first appeared in *Geofysiske Publikasjoner* in 1919 in a remarkable eight-page paper written by Jacob Bjerknes, "On the Structure of Moving Cyclones." I discovered this paper relatively untouched in the library of the Weather Bureau in 1930 along with further papers by Sølberg and Bjerknes on the structure of moving cyclones (Bjerknes, 1919), on the cyclone family (Bjerknes and Sølberg, 1923), and on the formation of rain (Bjerknes and Sølberg, 1921). These papers opened my eyes, for I had been trying to learn about weather forecasting by studying a large tome, "Weather Forecasting in the United States," by a number of eminent Weather Bureau forecasters: A. J. Henry, H. J. Cox, H. C. Frankenfield, and E. H. Bowie. Probably there is no other person in this audience who is aware of this book, though it was published in 1916. I'll later explain why the book died almost as soon as it was published—an indication of the sad state of the art of forecasting in the United

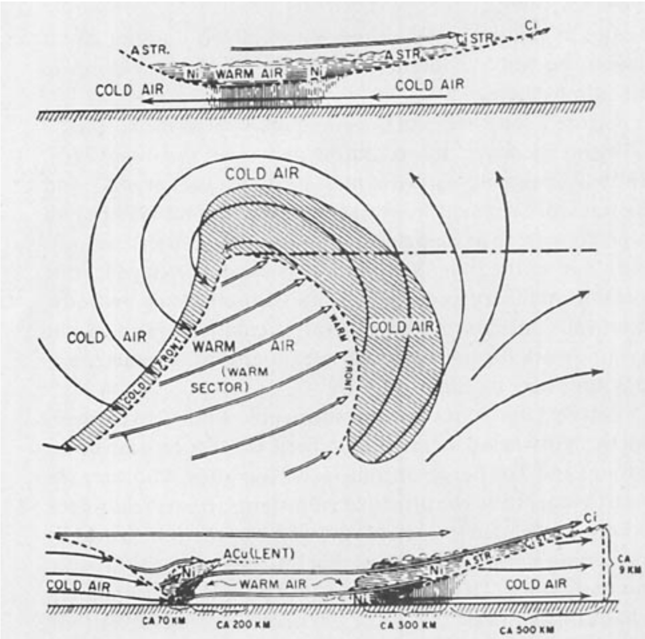


FIG. 1. Classical cyclone model (Bjerknes, 1919).

FIG. 2. V. Bjerknes (Bergeron, 1959).

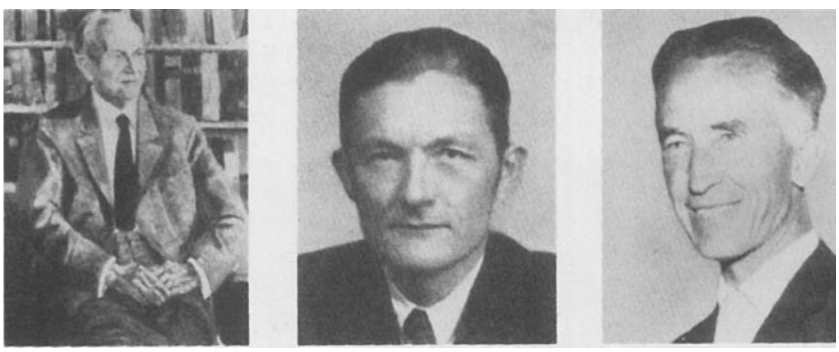


FIG. 3. Left to right: H. Sølberg, T. Bergeron, and J. Bjerknes (Fjørtoft, 1966).

FIG. 4. Bergen Weather Service, 14 November 1919: Bergeron, Rossby, S. Rosseland, and Technical Staff (Fjørtoft, 1966).

FIG. 5. Leroy Meisinger (AMS, 1924).

States at that time. Of course, it contained nothing about fronts, nor did it contain much about physical processes in the atmosphere.

Figure 2 is a picture of the grand old man of meteorology, Vilhelm Bjerknes. I don't know just when this was taken. In 1949 I had the pleasure of meeting him at the age of 87, and he was still extremely keen and inspiring. Before 1920 he had a professorship at Leipzig, and his son, Jacob Bjerknes, was with him at the time. Vilhelm Bjerknes set the stage for the polar front theory by developing the theoretical and hydrodynamical concepts of the general circulation. He laid the groundwork for both physical oceanography and meteorology for years to come.

Figure 3 illustrates three of the people who were pioneers on the Norwegian scene—H. Sølberg, the theoretician of the group, and Tor Bergeron and Jacob Bjerknes, who were the practitioners and the principal empiricists. It was really Jack who spearheaded this endeavor, which excited the world by providing a practical method that the forecaster could use in his daily work. The concepts made order out of the apparent chaos of weather—they were *physical* concepts rather than simply isobaric geometry. Bergeron, a genius at analysis, developed the occluded cyclone model, defined the structure of air masses, and explained cloud and rain formation and many other phenomena. Bergeron was an enviable combination of scientist and artist. His artistic ability and intuition enabled him to construct models with rare insight. Sølberg, the theoretician, unfortunately left the field of dynamic meteor-

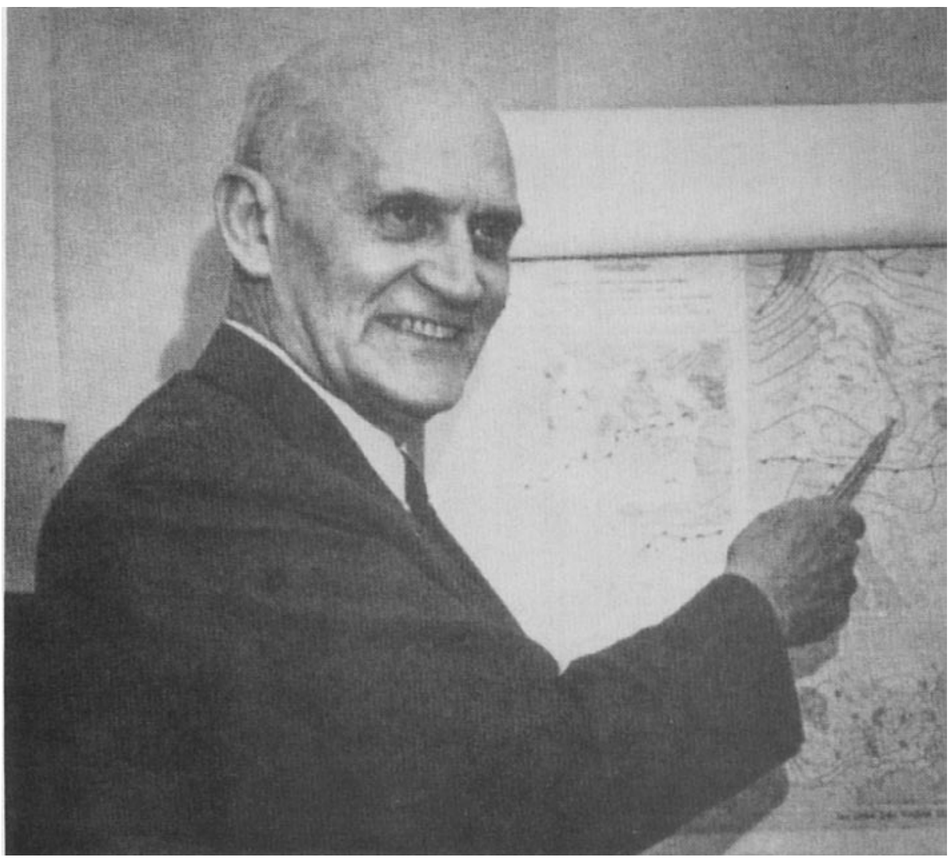


FIG. 6. F. W. Reichelderfer (AMS, 1963).

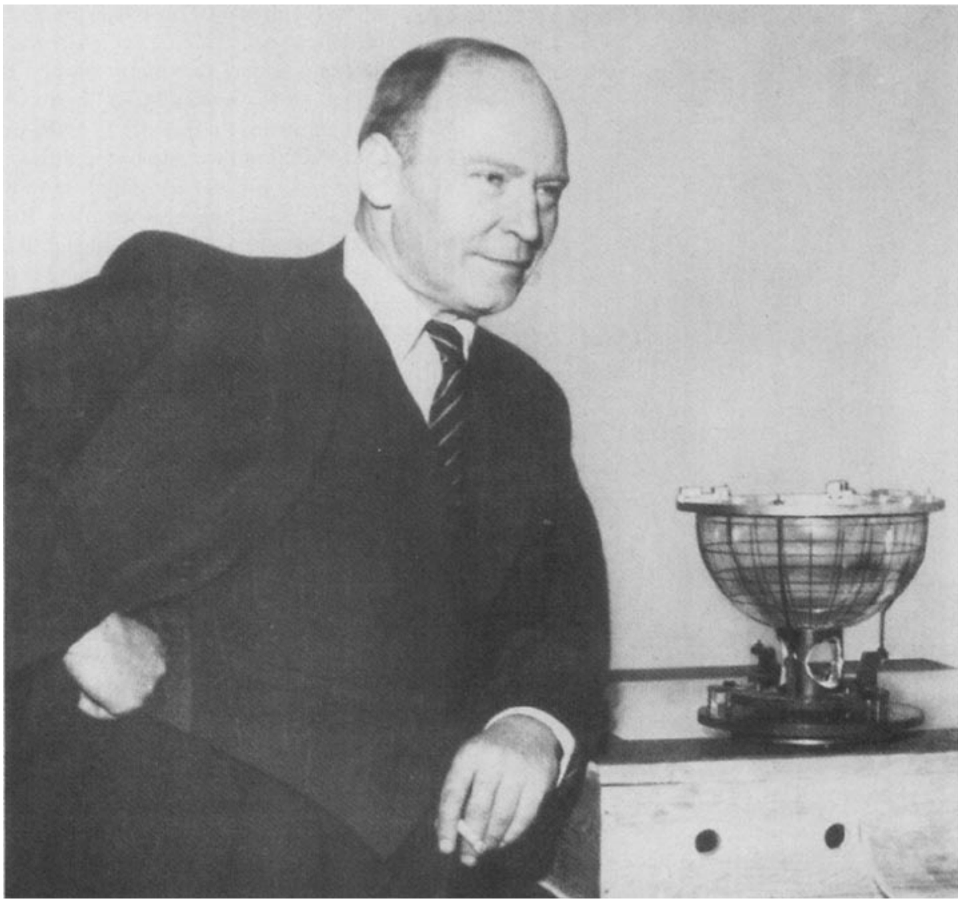


Fig. 7. C.-G. Rossby (Bolin, 1959).

Fig. 8. H. C. Willett.

Fig. 9. Horace R. Byers (AMS, 1962).

FIG. 10. H. Wexler.

ology after a few years. He became more immersed in pure mathematics and administration. The other two kept on working on air mass and frontal problems. Bergeron remained active until his death in 1977 at age 86.

Figure 4 is a remarkable picture of the Bergen Weather Service taken on November 14, 1919. Here we have T. Bergeron sitting in front (left), C.-G. Rossby next, and then S. Rosseland. Standing near the barograph is Jack Bjerknes. The other people are not identified; they were the technicians—the plotters of the weather maps. Rossby's presence in this picture deserves further comment. Rossby was in the Bergen Weather Service, but I understand from many conversations that he didn't fit into the scene quite as firmly as he would have liked. I think that this situation had a great deal to do with his subsequent work and drive, because when he came to the United States in 1926 he wanted to show that he could do more than what he suspected some of his former colleagues at the Bergen School thought him capable. Bergeron hints at this in his piece on Rossby in the Rossby Memorial Volume (Bergeron, 1959). At any rate, Rossby was a member of the Bergen School and both learned and contributed a great deal there, although his major contributions were to come later in the U.S.A.

Figure 5 sets the background for the American scene, which, as already indicated, was bleak in the 1920s. But there was a brilliant American, Dr. LeRoy Meisinger (after whom the AMS Meisinger Award was named), who was an intense student of meteorology. He was a free balloonist and used ballooning to explore the structure of the atmosphere. In 1924, he began a series of 10 ascents to try to learn more about the free air. Before the 10th and last ascent, when he was only 29 years old, he was advised not to make the flight because of predicted severe thunderstorms, but Meisinger

FIG. 11. Namias, Reichelderfer, and Rossby in the mid-1950s.

FIG. 12. S. Petterssen (Fjørtoft, 1966).

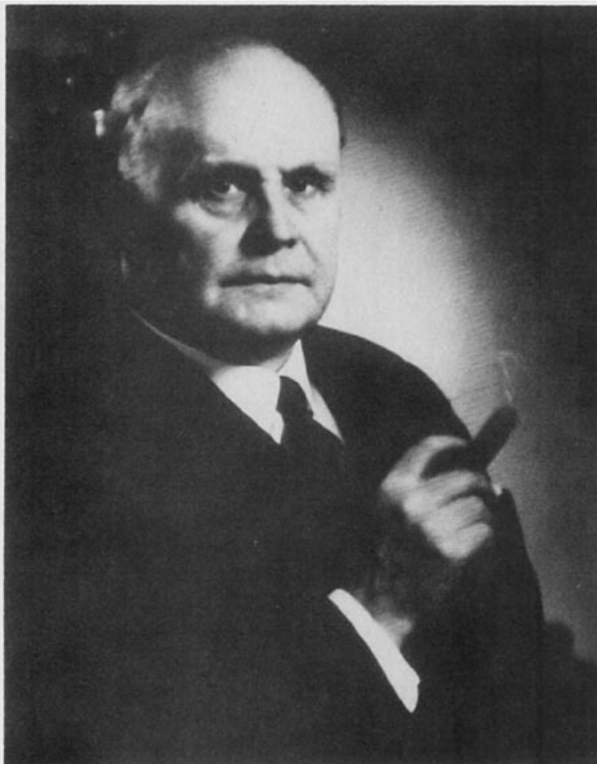


FIG. 13. Eric Palmén (Vuorela and Väisälä, 1958).

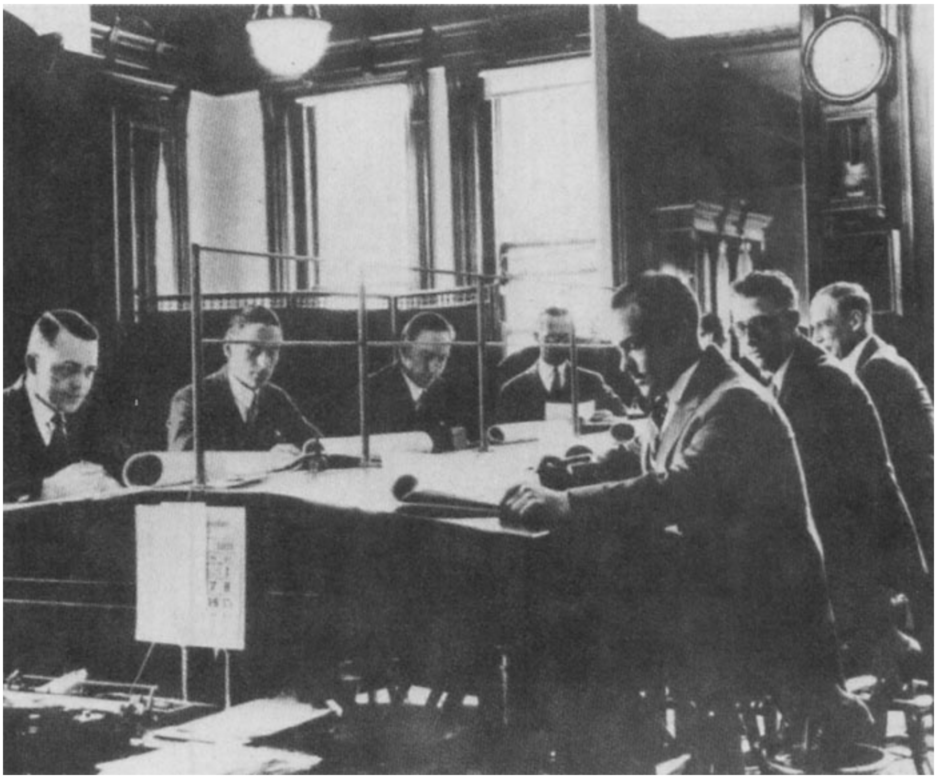


FIG. 14. Weather Bureau Forecast Office, Washington, D.C., 1926 (Hughes, 1970).

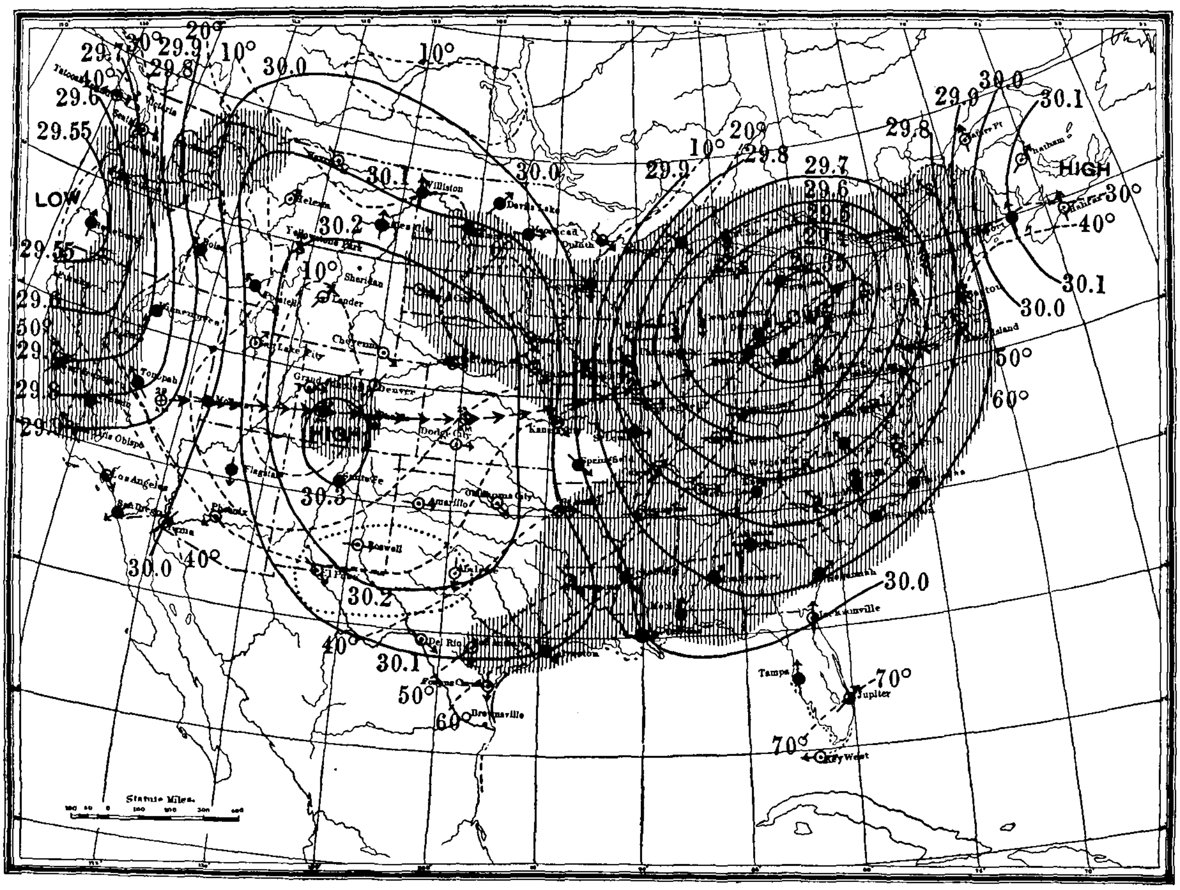


FIG. 15. Typical extratropical low, U.S. Weather Bureau map, 30 December 1907 (Milham, 1912).

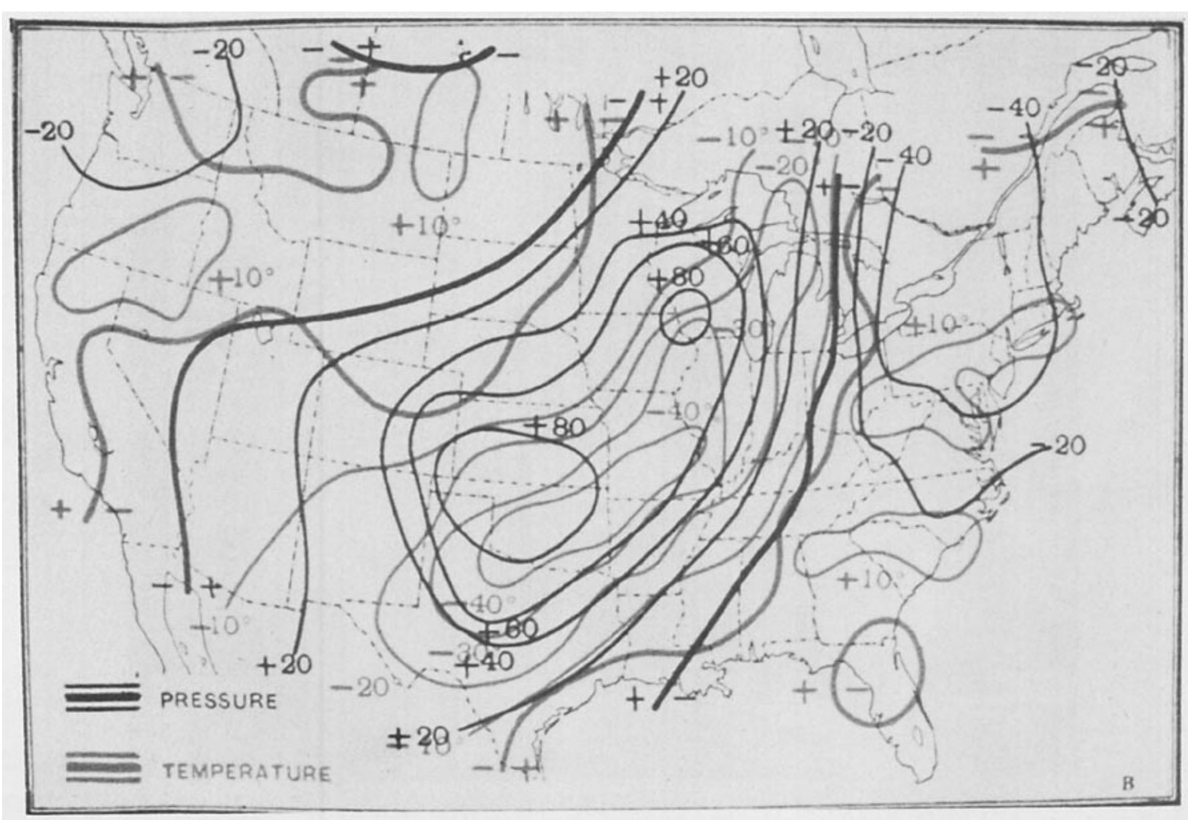
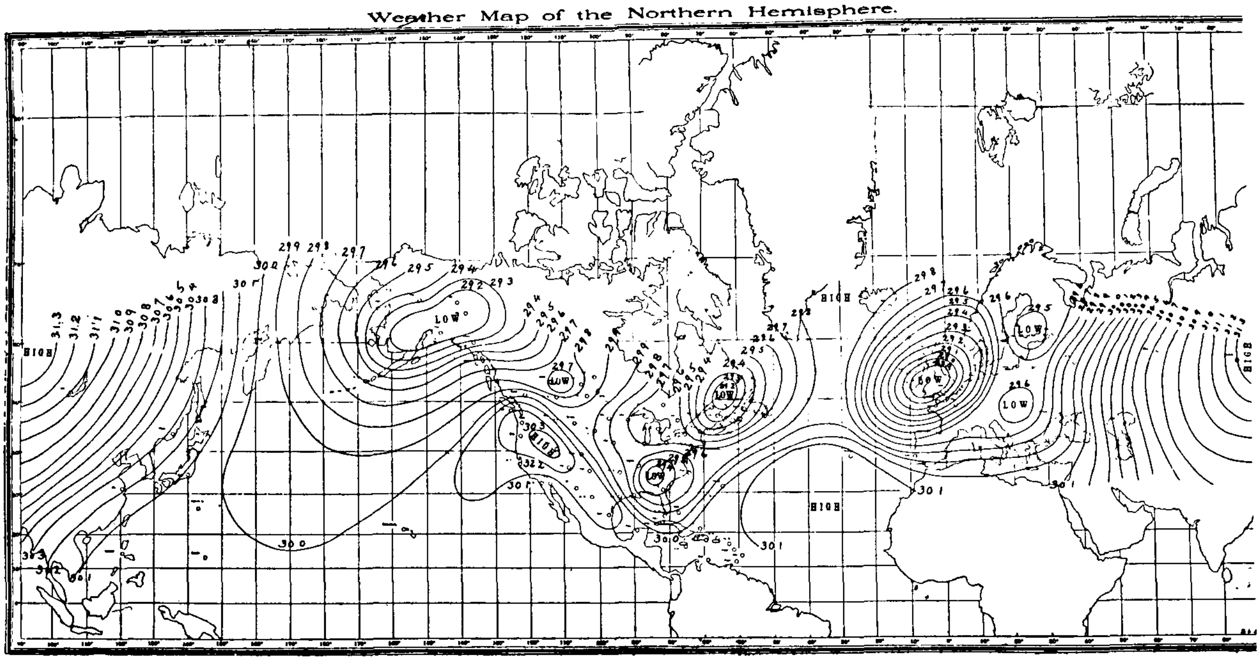


FIG. 16. Twelve-hour pressure changes and 24-hour temperature changes, 3 March 1904 (Milham, 1912).



Weather Map of the Northern Hemisphere for January 28, 1910. (U. S. Weather Bureau.)

FIG. 17. Northern Hemisphere isobars, U.S. Weather Bureau map, 28 January 1910 (Milham, 1912).

wanted to complete the series and ascended with Lt. Neeley of the U.S. Signal Corps. A static electricity discharge ignited the hydrogen balloon and the two lost their lives. Had Meisinger lived, he would have been one of this country's most prominent figures in American meteorology.

He was one of the first to construct upper-air maps and to devise a method of constructing these from surface data alone—a method later to be known as differential analysis. He was one of our early heroes in American synoptic meteorology—and one familiar with the polar front theory. But, and this was perhaps unfortunate for weather forecasting, he took his Doctor's Degree under Prof. W. J. Humphreys. Now, Humphreys held the exalted post of Meteorological Physicist of the Weather Bureau, but he had a pure physicist's view of the atmosphere rather than the broad view of the dynamical meteorologists we know today. Humphreys did much fine work in atmospheric optics and many other topics, but not in synoptic meteorology. You'll note in his book, *Physics of the Air* (first published in 1920) that only one page is devoted to the air mass and polar front theory. Humphreys was critical of some of these new ideas involving the polar front theory, as I know from many conversations with him. In spite of Meisinger's interest in frontal and air mass concepts, it wasn't the "in thing" at the Weather Bureau and he did not contribute directly to this subject.

Figure 6 is a picture of our great friend Reich (as we all know him), who is in the audience today.[1] It's a pleasure to talk about Dr. Reichelderfer, who was an American pioneer in air mass and frontal analysis. Many of you may not be aware of the fact that he became interested as early as 1920, when he was a Naval officer and studied the papers I referred to earlier. He began analyzing weather maps for the Navy at Hampton Roads, Va. These were probably the first frontally analyzed maps in the United States. He kept pushing for the adoption of air mass and frontal analysis methods, and it was he who was instrumental in starting the U.S. Navy on this course. Reichelderfer was sent by the Navy to Europe and visited a number of capitals in the early 1920s. In some very interesting letters he wrote about this trip, he told of an encounter with the German meteorologist, Dr. Noth, at Berlin's Templehof Airport. Dr. Noth tried to discourage him in his pursuit of Norwegian methods by saying, "How could anything important scientifically come out of tiny conservative Norway?" But that didn't dissuade Reich. He returned to the United States, wrote reports, prepared a series of analyzed maps that were furnished to various centers in the country, and had an important impact on the acceptance of polar front methods. Later he came to know Carl Rossby, who came to the United States in 1926 on a Guggenheim Scandinavian-American Fellowship.

In the United States Weather Bureau, Rossby found a hot bed of resistance to Bergen School ideas. However, Rossby began analyzing maps, performing experiments, and encouraging a more scientific approach to forecasting. Dr. Reichelderfer came to know Rossby well, and was extremely impressed with him. Later Reichelderfer had a lot to do with Rossby's stay in the United States, with the development of the first full-fledged school of meteorology at M.I.T., and with a host of other things, including the Guggenheim aeronautical network on the West Coast, a network that Rossby was instrumental in establishing. So Reich will enjoy a secure part in the history of polar front and air mass analysis. Later, in the 1940s, he played an important part in the improvement

[1] Dr. Reichelderfer died 26 January 1983 at the age of 87. Two articles on Dr. Reichelderfer appeared in *Weatherwise*, 1981, Vol. 34, Nos. 2 and 4.

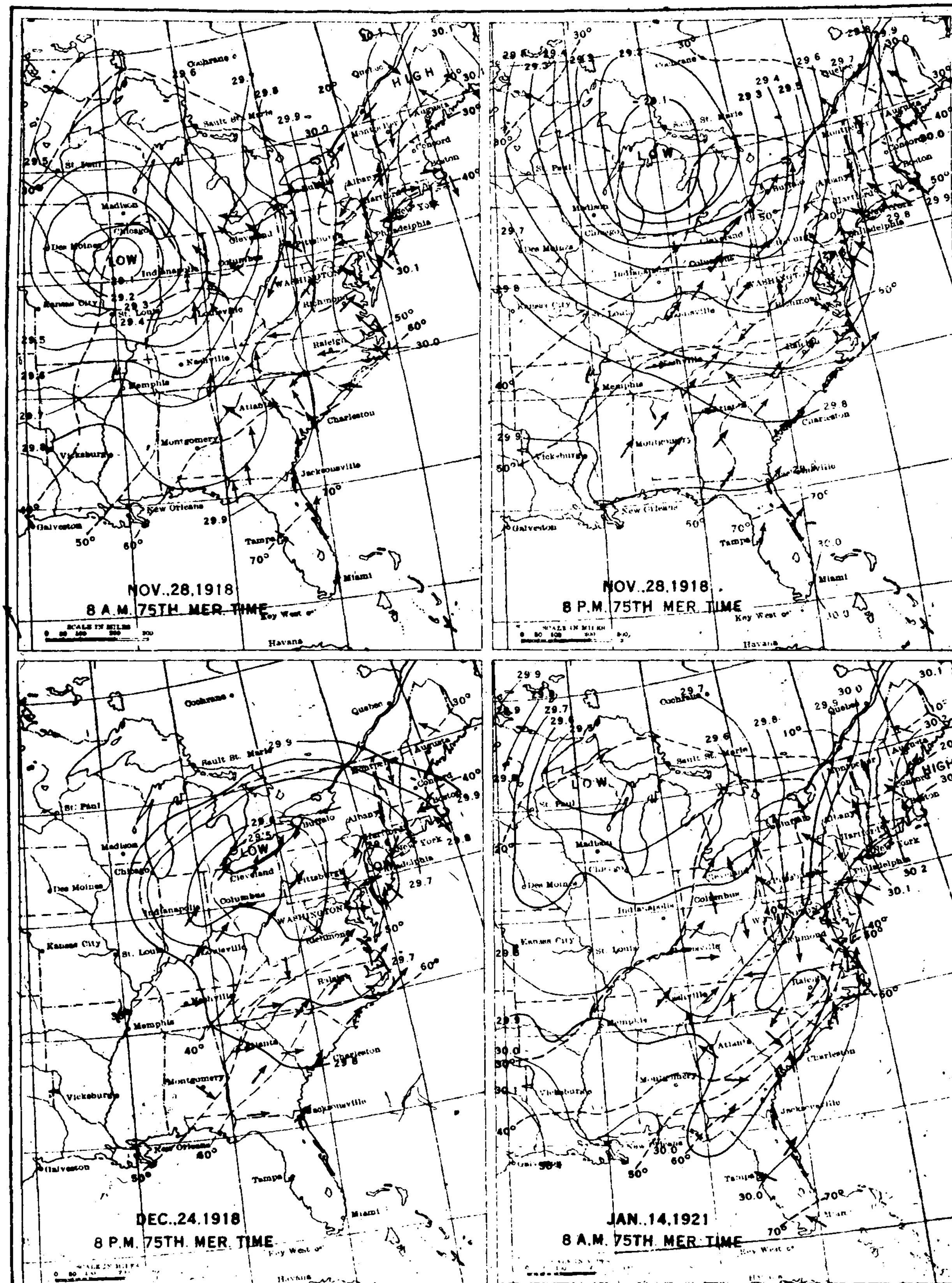


FIG. 18. Secondary cyclones, 28 November 1918, 24 December 1918, and 14 January 1921 (Brooks, 1921).

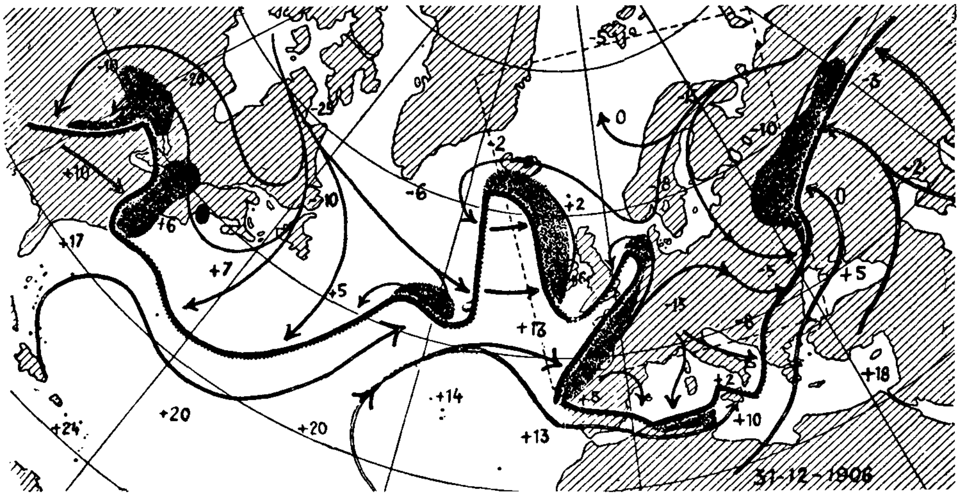


FIG. 19. Analyzed frontal map for North America, Atlantic, and Western Europe, 31 December 1906 (V. Bjerknes, 1921).

of forecasting by encouraging the use of modern electronic computers and by expediting the development of the meteorological satellite program. Reich also instigated the first full-fledged analysis center—a forerunner of today's National Meteorological Center. Since he is in the audience, we should give him a round of applause.

Figure 7 shows Prof. C.-G. Rossby—one of the outstanding meteorologists of all time. He played a tremendous role in the instigation of air mass and frontal analysis in America. His work with tank (dishpan) experiments beginning in 1926 started A. F. Spilhaus, Dave Fultz, and many others in related experimental work. In 1928, he established the first U.S. Meteorological Department at M.I.T., where frontal and air mass analysis was applied and expanded—studies that led to *direct* aerology through the use of thermodynamics and finally isentropic analysis.

While at the Weather Bureau in 1925–26, Rossby discovered a young man who seemed to have a lot of potential—H. C. Willett—Fig. 8. Willett had studied mathematics and physics at Princeton and was interested in the forecasting problem, even though his post at the Weather Bureau was much subordinate to that of forecaster. Rossby helped arrange for Willett to go to Bergen on a Scandinavian-American Fellowship in order to study air mass and frontal analysis. Willett spent the year 1929 at Bergen and returned to the U.S. Weather Bureau afterwards. Dr. Reichelderfer went to Bergen in 1933. Meanwhile, Rossby had established the department at M.I.T. and invited Willett to join him. But Prof. C. F. Marvin, the Chief of the Weather Bureau, didn't like the idea and said to Willett, "you know if you leave the Weather Bureau, we can't take you back." He tried to discourage Willett from going to M.I.T., but did not succeed; so Willett left for M.I.T. to accept an Assistant Professorship even with a reduction in salary.

In 1934, President Roosevelt set up a Meteorological Advisory Committee consisting of Karl Compton of M.I.T., Isaiah Bowman of John Hopkins, and Carl A. Millikan of C.I.T. to examine the Weather Bureau. When asked why the Bureau had not adopted Norwegian Methods, Chief Marvin replied, "We didn't pursue it because the man we sent to study in Norway left us." That was the excuse for the Weather Bureau not taking up air mass and frontal methods. Later I'll mention some of the forecasting methods that were being used.

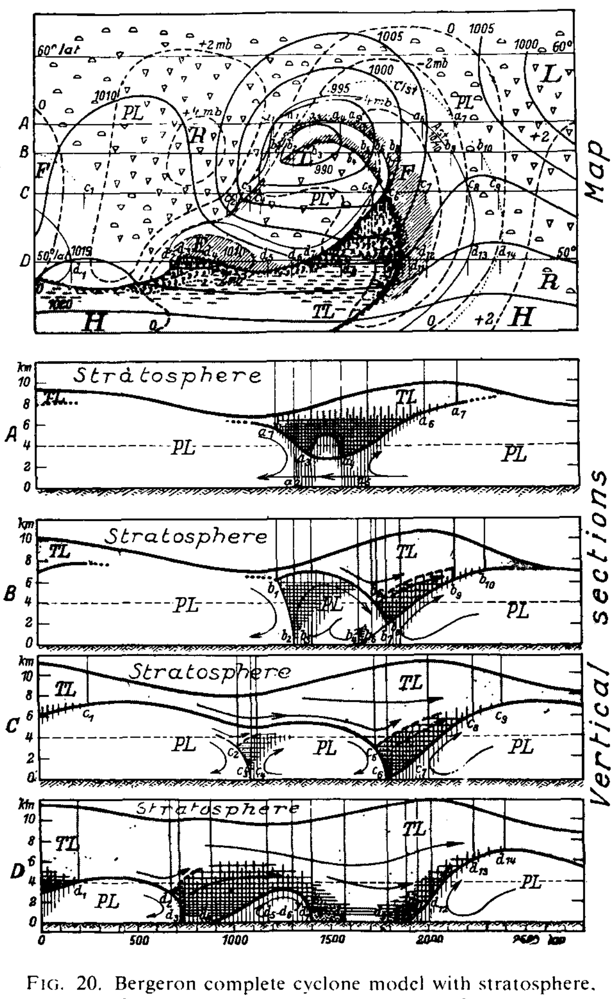


FIG. 20. Bergeron complete cyclone model with stratosphere, fronts, and hydrometeors (Namias, 1940).

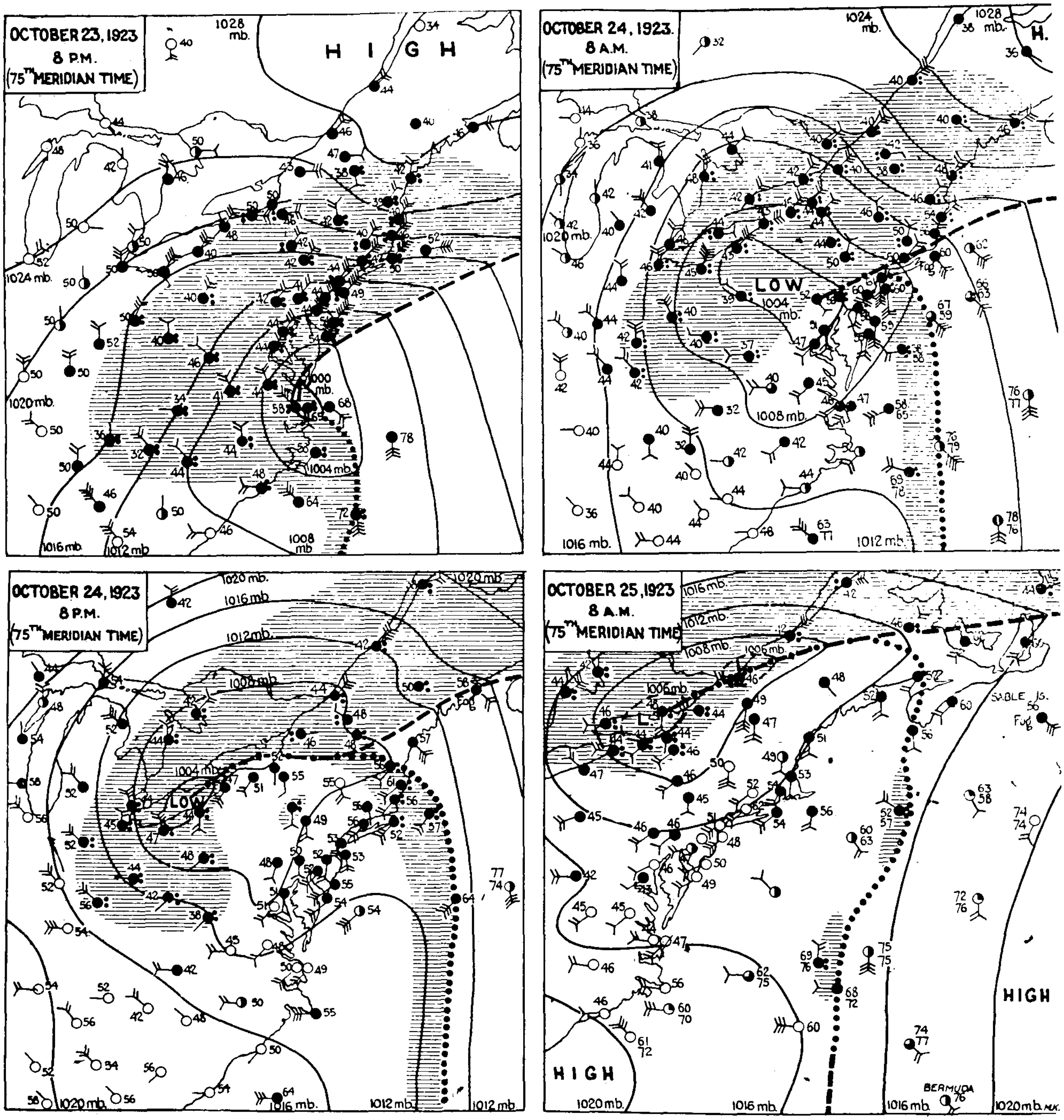


FIG. 21. Analysis of a retrograde depression in eastern United States, 23–25 October 1923 (Bjerknes and Giblett, 1924).

Figure 9 is a picture of Horace Byers. Horace worked in the West Coast Guggenheim Aeronautical Network under Rossby, and later, after Byers had graduated in Geography at Berkeley, Rossby persuaded him to carry on graduate work at M.I.T. After M.I.T., Byers worked for TWA and introduced air mass and frontal analysis methods to the airline before establishing the first air mass analysis group at the U.S. Weather Bureau—an action that was partly the result of advice from President Roosevelt's Scientific Advisory Committee.

Byers was beset with many difficulties in introducing air mass analysis to the Weather Bureau. His group was placed in a corner room of the Weather Bureau—a safe distance from the forecasters. The forecasters were doing "the real thing" and couldn't be contaminated by the young upstarts analyzing maps so strangely in the other room. The air mass group held daily map discussions and some of the old-timers came "loaded for bear." But among the upstarts were not only the resilient Byers, but also Harry Wexler, who loved scientific argument. Harry (Fig. 10) took an active role in the development of air mass and frontal analysis. He did classical work on the role of radiation in development of polar

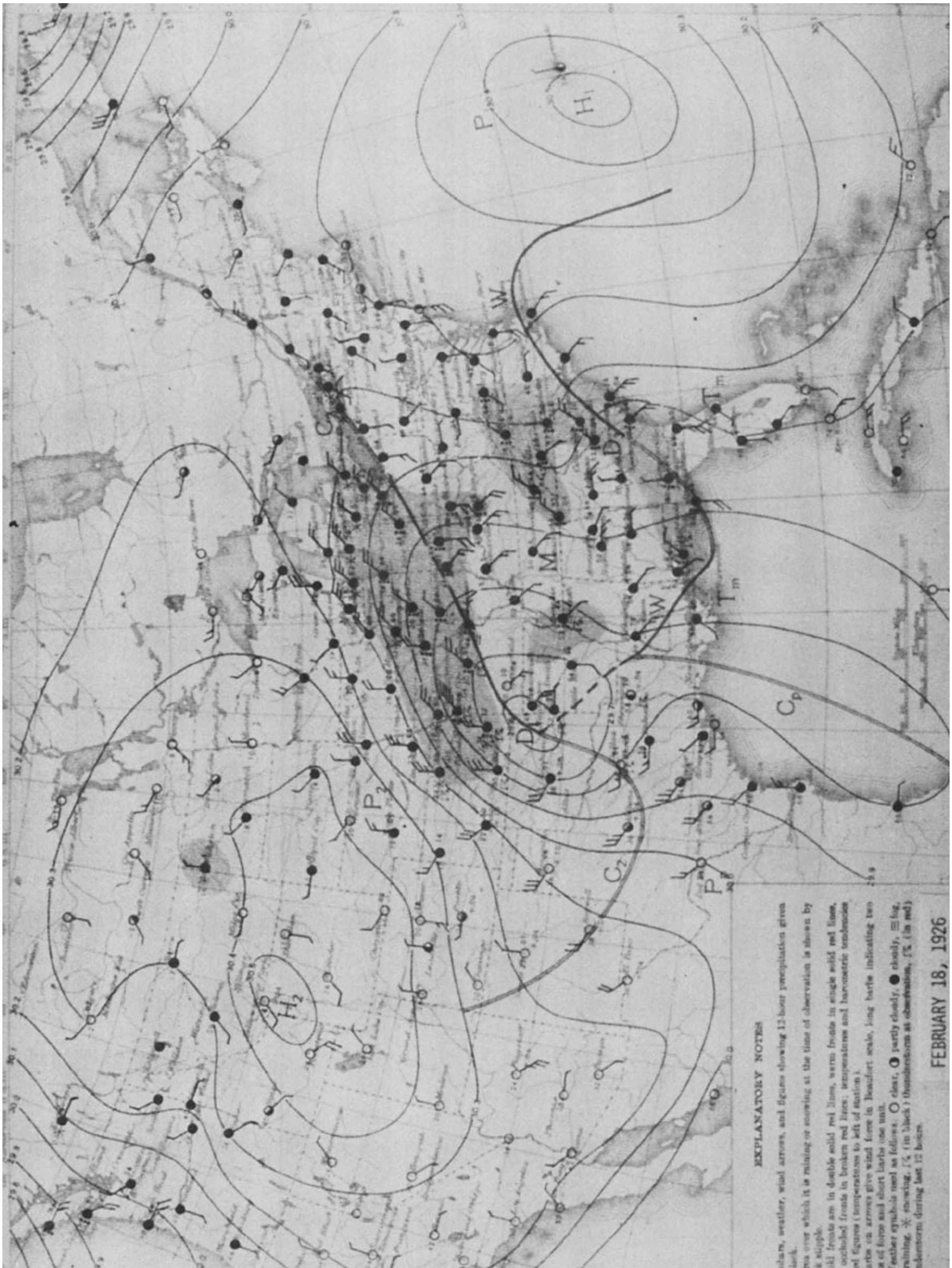


FIG. 22. Frontally analyzed weather map (Rossby and Weightman, 1926).

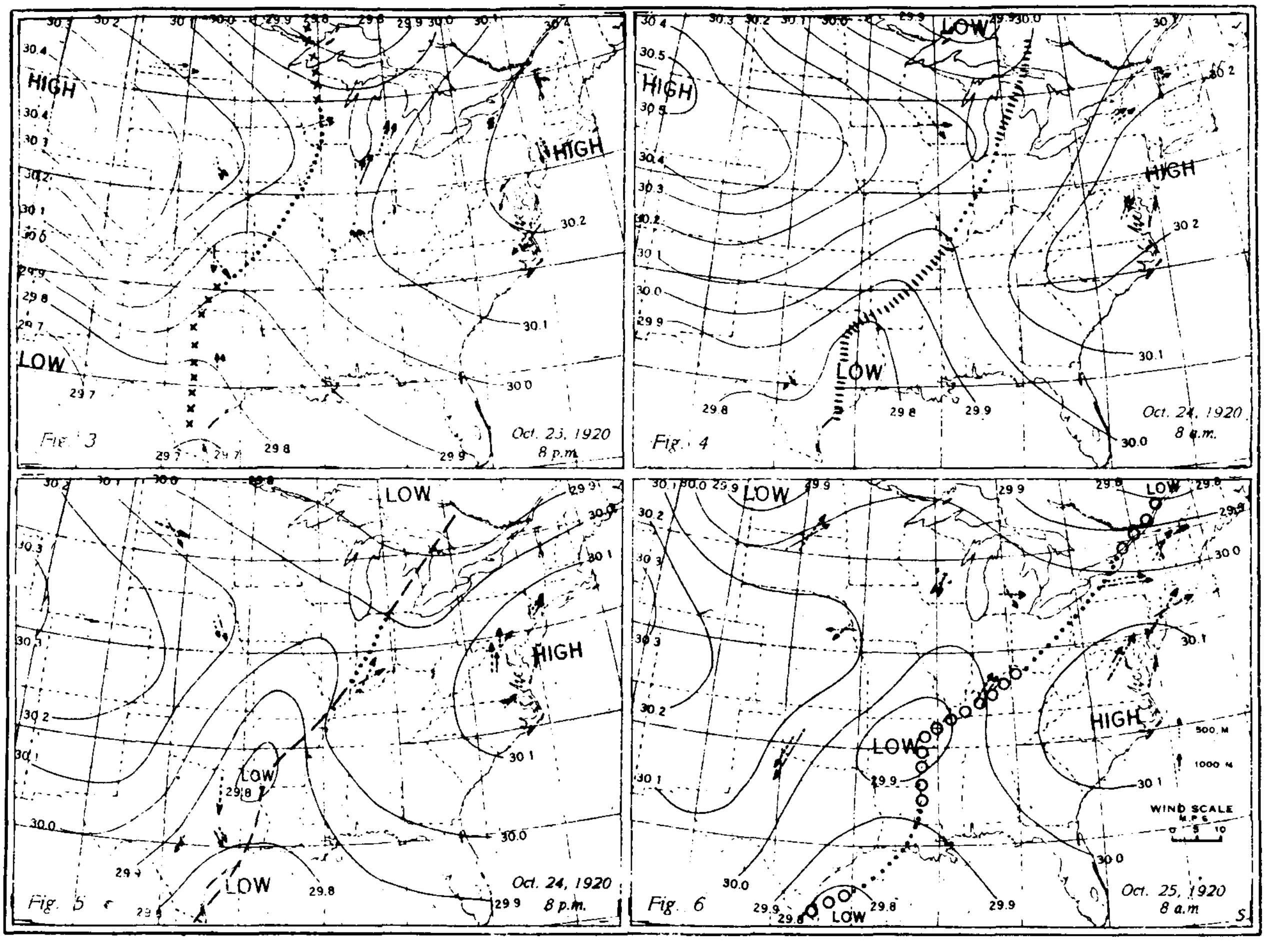


FIG. 23. Application of Bjerknes lines to the development of secondary lows during International Balloon Race, 23-25 October 1920 (Andrus, 1921).

continental air, made studies of fronts, and participated in many things with a hyperactive drive that ultimately led to his untimely death.

Figure 11 (taken in the 1950s) shows three of us together—Rossby, Reichelderfer, and myself. My own role in this business, especially in the 1930s, was partly as a salesman for the Bergen School. Jokingly, over in Norway on one visit, Bergeron and some Norwegians bestowed on me a sort of honorary membership in the Bergen School. One of the things I did was to publish a series of articles written during the mid-thirties, which were made into a booklet called "Air Mass and Isentropic Analysis" (Namias, 1940). A number of other people also contributed, including our chairman, Dr. Haurwitz, who wrote an excellent article on Norwegian Wave Theory. Bob Stone put together an extensive bibliography, and Hurd Willett contributed material on the structure of American air masses. The book was popular because the world was hungry for a physical rationale on which to base weather forecasts. Over 50 000 copies were sold by the AMS (the publishers). In addition, it was translated into two different Spanish editions, and was used in hundreds of weather offices over the world. In my later travels around the country as a U.S. Weather Bureau official, I found that this book was the thing that I was best known for, although I had hoped to be better known for my scientific papers. On one occasion a forecaster came up to me and said, "So you're Namias—that SOB who got me into this damn field with that book."

Figure 12 shows Sverre Petterssen, who passed away in 1974. He was also one who helped spread frontal and air mass concepts to America. As you all know from his technical papers and books (Petterssen, 1956), he was a wonderful writer and teacher. Reich got the Navy to invite him to the United States in 1935, and Rossby brought him to M.I.T. to take over the M.I.T. department in 1939 after he (Rossby) left for a two-year assignment as Assistant Chief of the Weather Bureau.

Figure 13 is a picture of Eric Palmén, who came to the University of Chicago in the 1940s. With the assistance of Chester Newton, he did some remarkable work on the analysis of atmospheric cross sections, the structure of polar air, the dynamics of cold lows, and many other synoptic phenomena. Palmén brought new life to the field of 3-dimensional synoptic meteorology—by means of real observations and not the *indirect* aerology from which the Norwegians had made so many inferences. The Norwegians had developed an air mass classification system that was supposed to give an idea of the structure of the upper air. As it turned out, in America we were fortunate to be able to capitalize on a rapidly developing aerological network, first by kites, then by airplanes, and then radiosondes. We were routinely analyz-

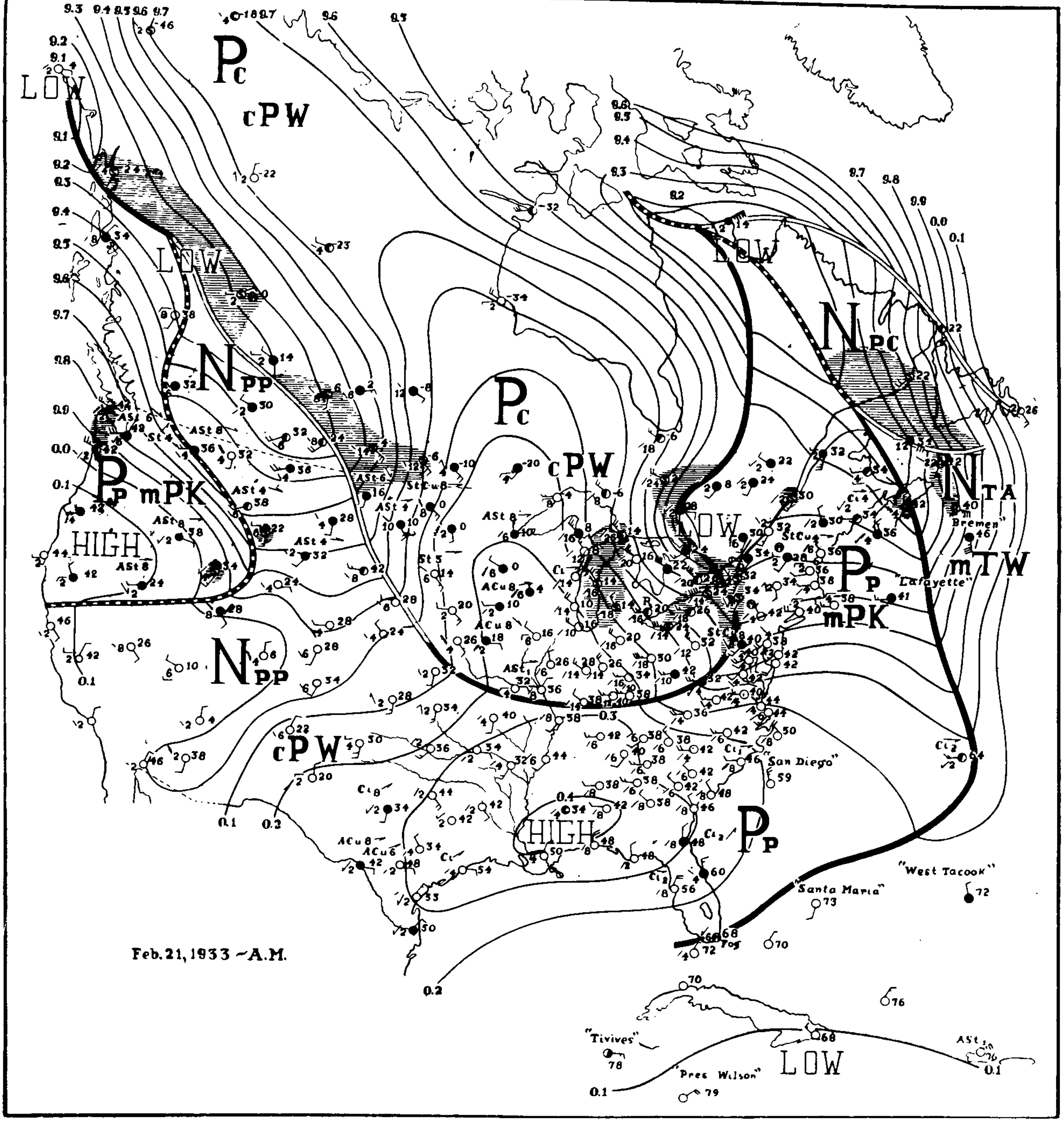


FIG. 24. Typical example of M.I.T. North American map analysis in 1933 (Namias, 1934).

ing the real structure of the atmosphere when some of the invited Norwegians to the U.S. were still talking "indirect aerology." The demise of "indirect aerology" came in the late 1930s and early '40s.

Figure 14 brings us to a scene at the Weather Bureau in 1926—the "map room" as it was called (Hughes, 1970). W. P. Day is in front on the left, R. H. Weightman at the back of this row, Ballard on the front right, and Tom Brooks (back right). I can't recognize the others. Someone took down messages sent in code by telegraph from the various stations; these were then translated verbally to the group. Each man plotted a separate map. One plotted a pressure change map, another a cloud map, another a precipitation chart, etc. A whole series of maps had to be studied by the chief forecaster. There was no central map, or "Map A," which the Bergen School developed, showing all weather elements on one chart. This was the status in the U.S. Weather Bureau in 1926, eight years after the development of the Bergen School. In the corner of this room (not shown) was Reich, a Naval Liaison Officer, analyzing weather maps according to Norwegian Methods. Although H. C. Willett was also here at the time, he wrote me saying that no one at that time (1926) in

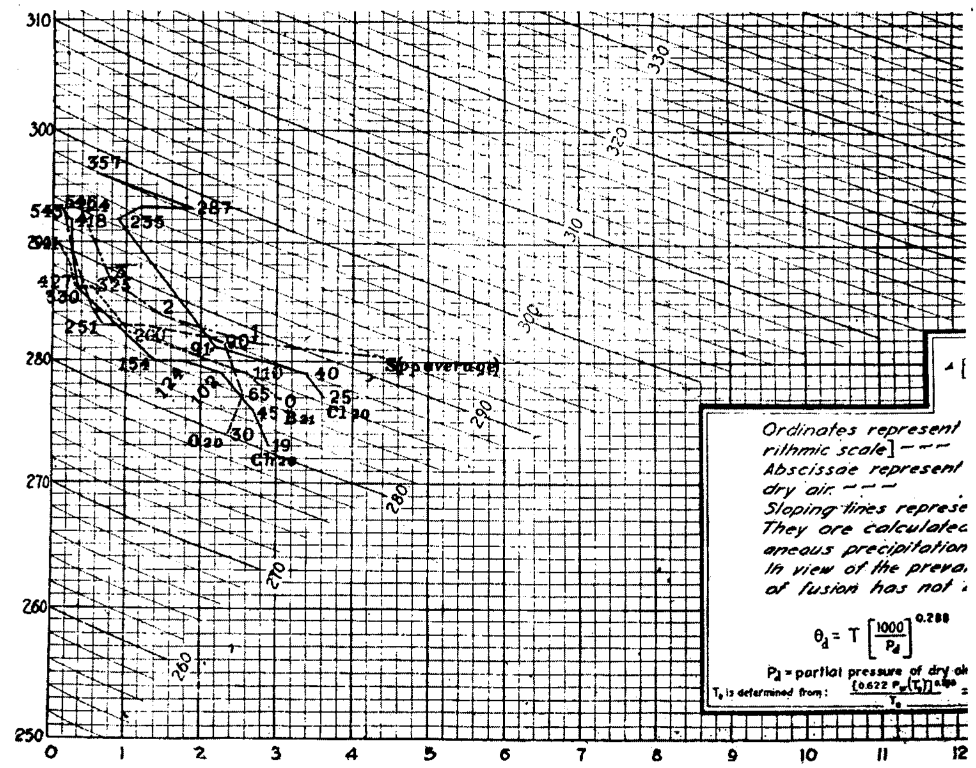


FIG. 25. Rossby diagrams of the early 1930s (Namias, 1934).

this organization practiced polar front analysis or, as a matter of fact, had even heard of it.

Figure 15 illustrates what a printed map for the public looked like then. It is dated in 1907, but the form stayed the same for decades. No fronts or air masses were indicated—in spite of the fact that strong fronts were present—but there were isotherms and storm tracks. The methods of forecasting stressed extrapolation, used classifications of storm tracks, and, of course, moved the precipitation areas along with the "Lows." In the large forecasting volume I mentioned earlier, there are hundreds of rules that I tried to memorize in order to learn how to forecast. There were confusing and seemingly contradictory rules involving pressure changes, etc., and I soon had to give up.

Figure 16 shows one of the popular maps in use—pressure and temperature change maps showing 12-hour pressure changes in black and 24-hour temperature changes in red. Much emphasis was given to these maps.

Figure 17 illustrates a trend toward the development of hemispheric pressure charts. These were actually drawn in the Weather Bureau in 1910 and carried on for some time by W. P. Day. There were no fronts, and the data on which the map was based were naturally limited. The map was, however, a giant step in the right direction.

There were studies like those by Charles F. Brooks on the development of secondary storms that formed on the hanging tongue of low pressure (Fig. 18). These were generally studies in isobaric geometry in which little physical thinking was introduced (Brooks, 1921).

Figure 19 shows a map that appeared in an article by Vilhelm Bjerknes in 1921, although it's a 1906 chart (Bjerknes, 1921). Vilhelm Bjerknes had the idea that the polar front circumscribed the whole hemisphere. He made no provision in this map for the transfer of polar air into the subtropics and into the tropics, but it was a pioneering attempt to analyze on a large scale.

Figure 20 shows Bergeron's cyclone model (Namias, 1940), which includes the stratosphere, tropopause, rain areas, air mass showers, etc. A whole series of lectures could be given on this one diagram, for it gave much physical insight into weather.

In addition to those whose pictures I showed, I want to mention the names of some others who also contributed. Among them were I. P. Krick, a contemporary of mine. Irving Krick married Horace Byers' sister-in-law and Horace encouraged him to enter meteorology. Dr. Krick was an excellent analyst and short-range weather forecaster. After a job with Western Air Lines, he proceeded to build up a meteorological department at Cal Tech. He felt that the meteorology department at M.I.T. was entirely too theoretical and someone had to teach students how to do things in a "practical" way—i.e., forecast. He turned out a number of excellent students, among them the late Ben Holzman and Joe George, and many U.S. Air Force officers. Krick had visions of extending detailed forecasts not only for a day, a week, and a month, but for years. He claimed to be able to make predictions for presidential inaugurals years in advance. These claims brought him into confrontation with the meteorological community and especially with scholars of predictability, who claimed that *detailed* predictability is limited to two or three weeks at most.

However, Krick's role in practical and commercial appli-

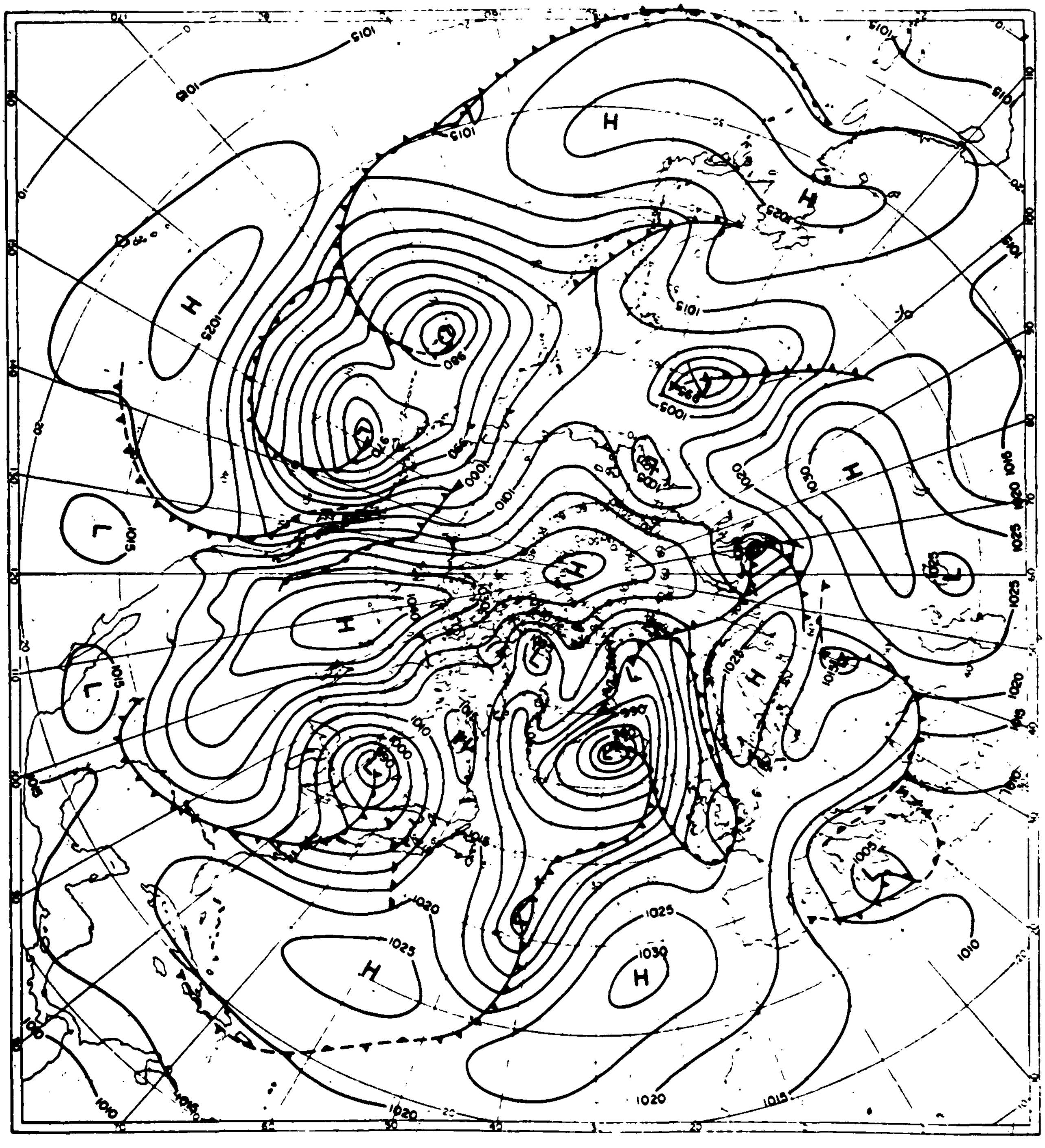


FIG. 26. Early Northern Hemisphere weather map analysis as practiced at M.I.T. beginning 1936 and currently (Saucier, 1955).

cations of air mass and frontal analysis is well recognized. Also, together with Bob Elliott, he developed a series of weather types that were of pedagogical value.

Joe George was a leader in airline meteorology (also a Brigadier General in the Air Force Reserve). Ben Holzman headed the first U.S. Weather Bureau Analysis Center and made forecasts for trans-Atlantic flights before entering the U.S. Air Force (retiring as Brigadier General).

Jörgen Holmboe was a principal collaborator with Bjerknes in his pioneering studies of long waves and their relation to cyclones. Bernhard Haurwitz extended Rossby's ideas of long waves into the domain of a spherical earth. Haurwitz also made studies at Blue Hill Observatory on the vertical structure of air masses through soundings by exploring correlations between levels. Victor Starr wrote a book on weather forecasting that was 20 years ahead of its time, but

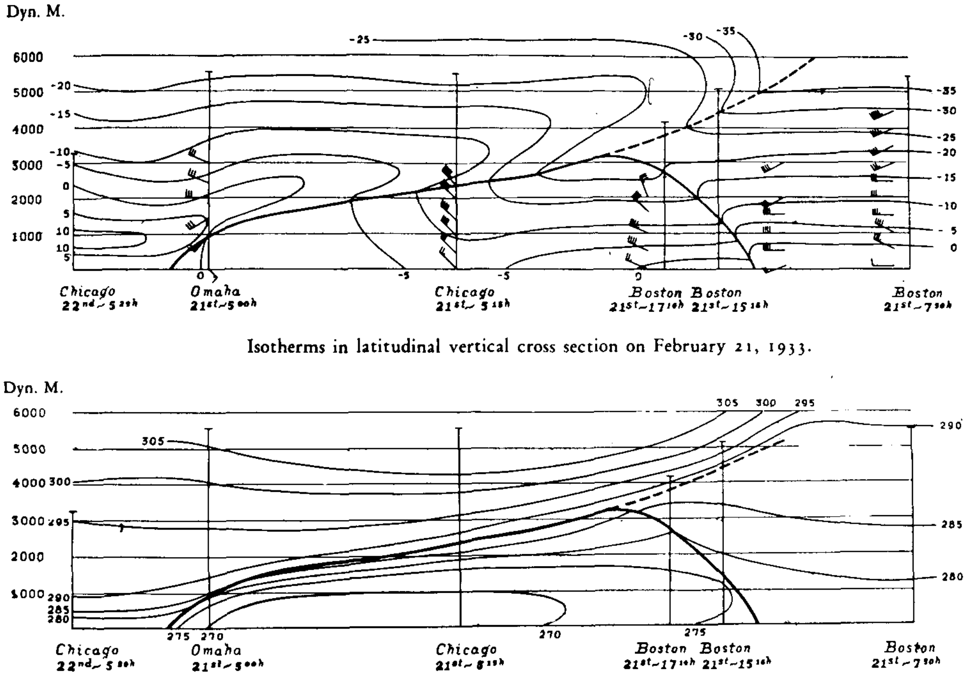


FIG. 27. North American wedge of P_c air, February 1933 (Namias, 1934).

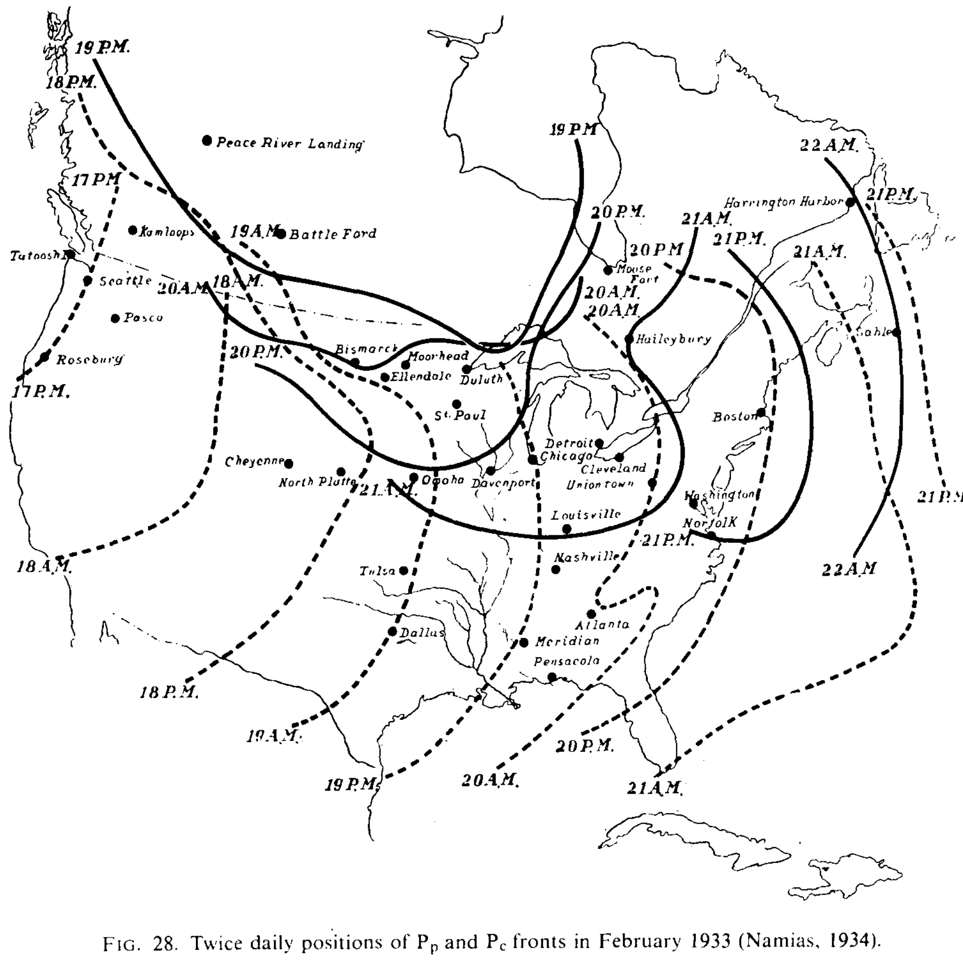
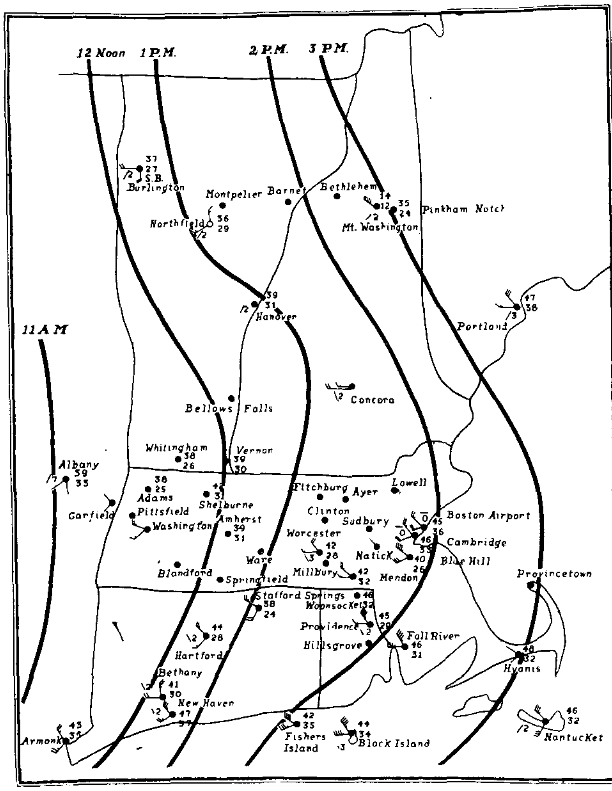


FIG. 28. Twice daily positions of P_p and P_c fronts in February 1933 (Namias, 1934).



Cold front isochrones over New England (February 21, 1933).

FIG. 29. Hourly isochrones of P_c front, 21 February 1933 (Namias, 1934).

he was criticized by some forecasters who wondered how a dynamicist (not a forecaster) could have the nerve to write a book on forecasting. Herbert Riehl was first very active in mid-latitude meteorology and then studied the tropics, adding much to our knowledge of low latitudes. George Cressman developed methods of extending prognostic charts to two days in advance and later developed objective systems for analysis. George Taylor wrote a book summarizing much of the air mass and frontal knowledge of the times. Bob Fletcher worked for the airlines, the U.S. Weather Bureau, and later was scientific consultant to the U.S. Air Force. Henry Harrison, Ed. Minser, and Arthur Merewether were pioneers in airline meteorology. Even I spent part of my early career as an airline meteorologist. It was the thing to do to get realistic exposure to the weather. The airline meteorologists contributed a great deal to the application of the polar front theory because they knew first-hand about cloud layers and could adapt these realities to the simple polar front model.

At the outbreak of World War II, a vast and important project was undertaken by the Weather Bureau to construct a series of daily Northern Hemisphere analyzed charts dating back to 1889. This work was carried on first at N.Y.U. and later at C.I.T., and at the National Weather Service Climate Center in Asheville, N.C.

We will now look at some figures that give an idea of research and practice in the late 1930s. Figure 21 is from a paper that appeared in a *Monthly Weather Review* (Bjerknes and Giblett, 1924), but it received little attention in spite of the fact that it was a careful analysis of a retrograde depression in terms of fronts and air masses. Figure 22 gives a remarkable picture of some frontal and air mass analyses (Rossby and Weightman, 1926) in the *Monthly Weather Review*. In this excellent paper, Rossby and Weightman described upper-air conditions with modern methods and observations. They introduced the concept of convective instability, gave physical reasons for the rainfall, cloud systems, and other phenomena. This paper also received little attention in the Weather Bureau. I recommend it for those who wish to see a thorough pioneering synoptic analysis. C. G. Andrus studied meteor-

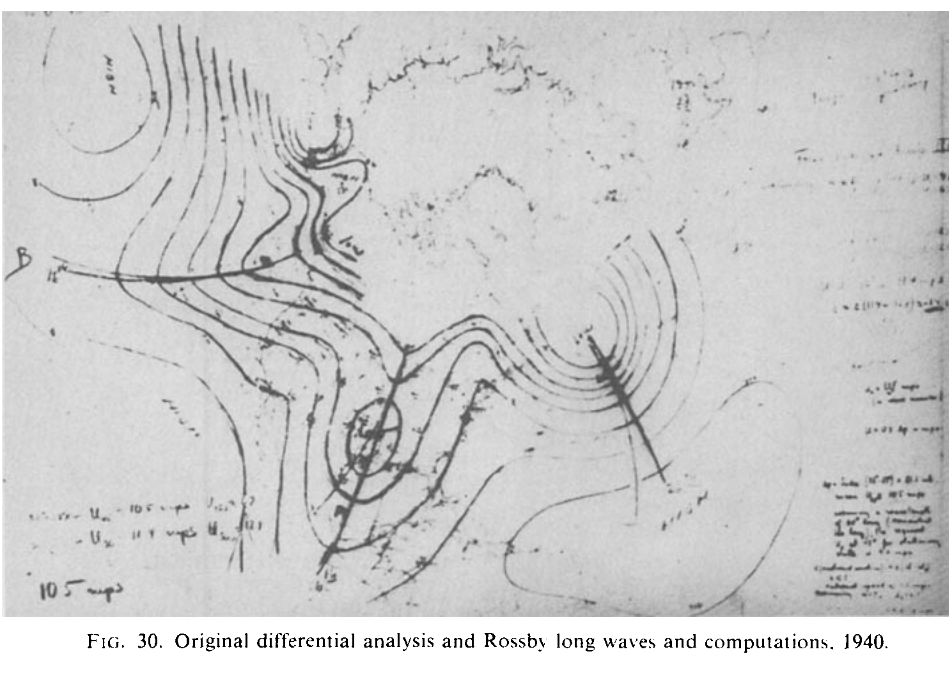


FIG. 30. Original differential analysis and Rossby long waves and computations, 1940.

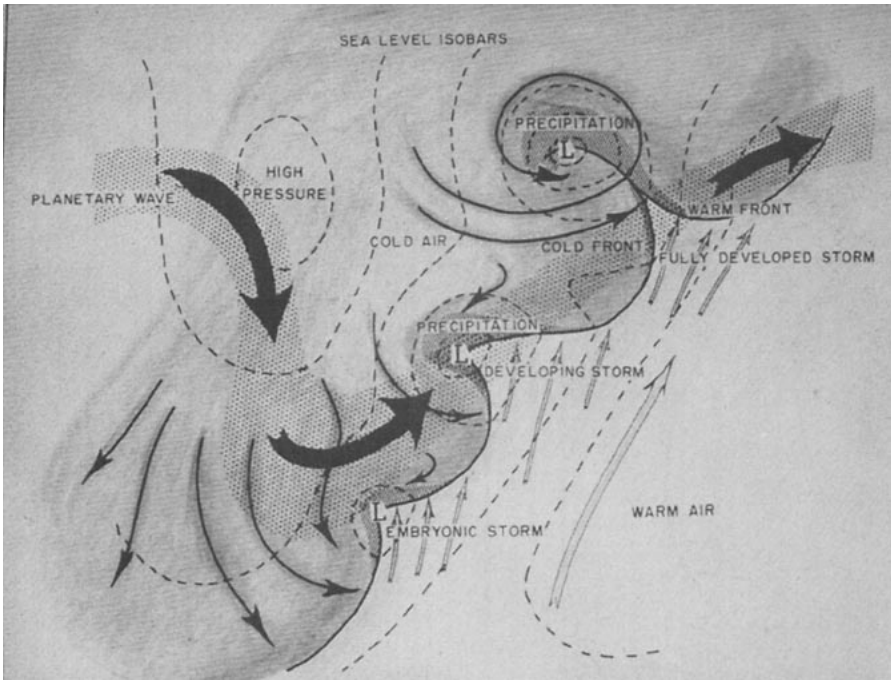


FIG. 31. Schematic cyclone family—Jet stream and air masses.

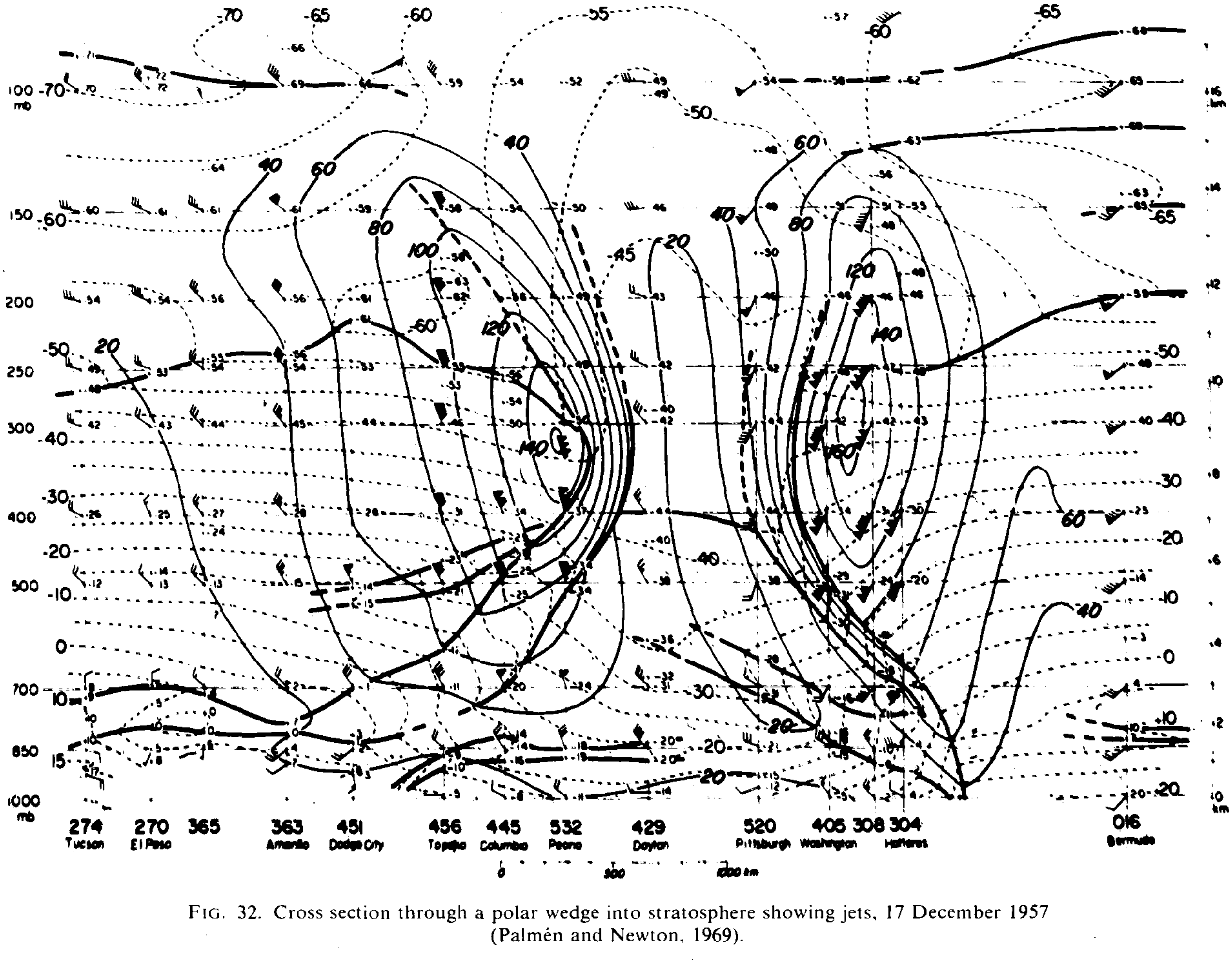


FIG. 32. Cross section through a polar wedge into stratosphere showing jets, 17 December 1957 (Palmén and Newton, 1969).

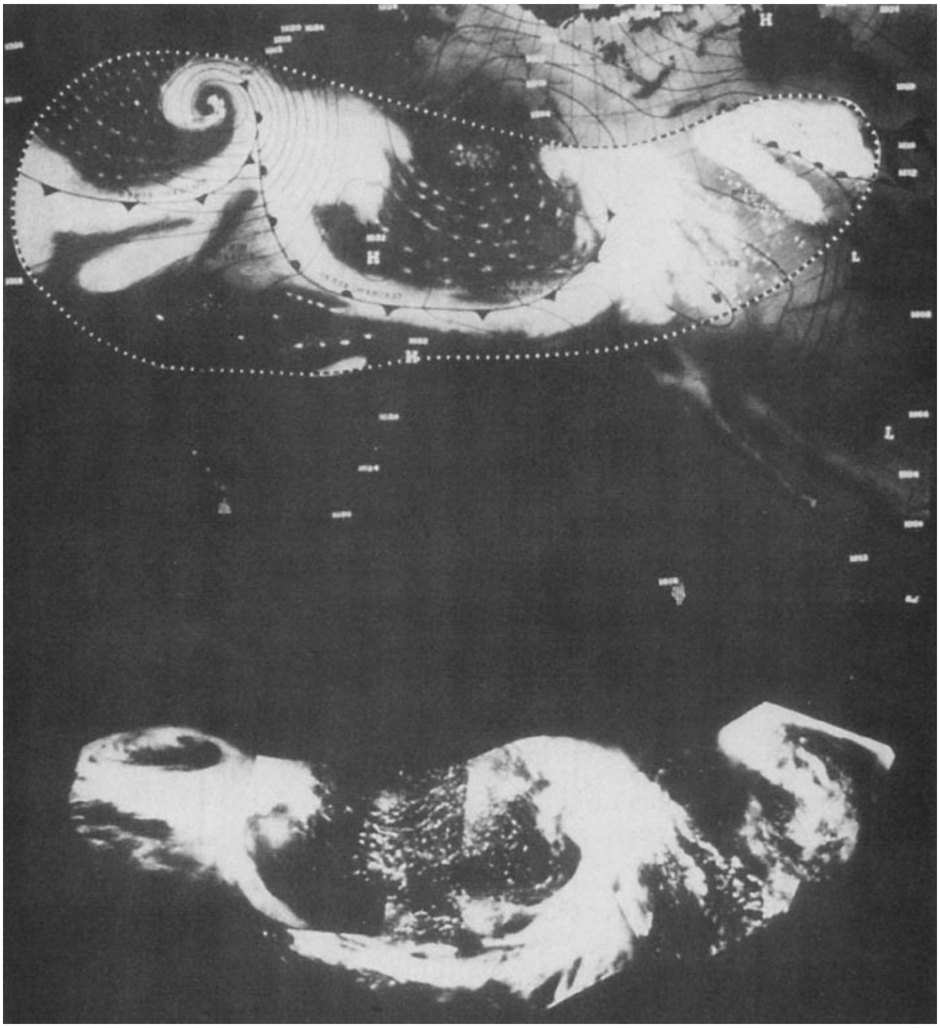


FIG. 33. Satellite mosaic showing a Pacific Storm. (Courtesy of V. J. Oliver, National Satellite Center.)

ology for balloon races and had identified the line that we now call a "front" (Fig. 23). Figure 24 shows the type of analysis that was first done at M.I.T. after Rossby arrived. We had only the North American observations. The air mass designations were both local (by Willett), and international (by Bergeron). Figure 25 shows the much-used Rossby thermodynamic diagram in which potential temperature was plotted against mixing ratio, with sloping lines of equivalent potential temperature. The diagram was used to identify air masses and was an effective tool for its time. Figure 26 gives an example of hemispheric surface analysis. The first analyzed maps for the Northern Hemisphere were constructed in 1935 at M.I.T. in a project supported by Bankhead-Jones Act funds. H. C. Willett was the immediate head of this project and I had the good fortune to work with him on these hemispheric analyses that led to the development of extended forecasting methods. Figure 27 brings us to upper-air structure; this was the kind of cross section routinely constructed at M.I.T. It is a cross section of a polar continental air outbreak making its way rapidly into the northeast. I believe this was one of the first (1933) of this type of cross section. Note the big subsidence inversion ahead of the cold front. You'll notice that this section is made not only from soundings in space (Chicago/Omaha, etc.), but also from three soundings at

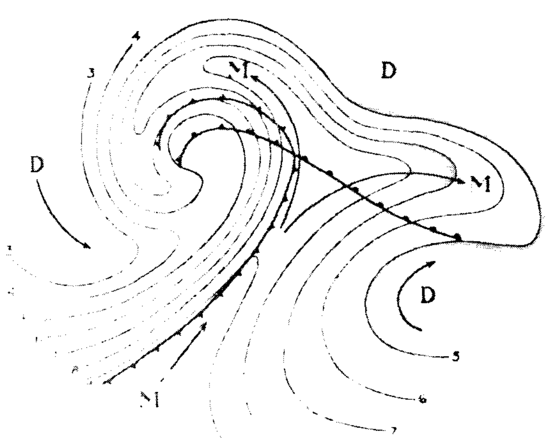


FIG. 34. Namias's schematic isentropic analysis of moist and dry tongues around an occluded depression (Namias, 1939).

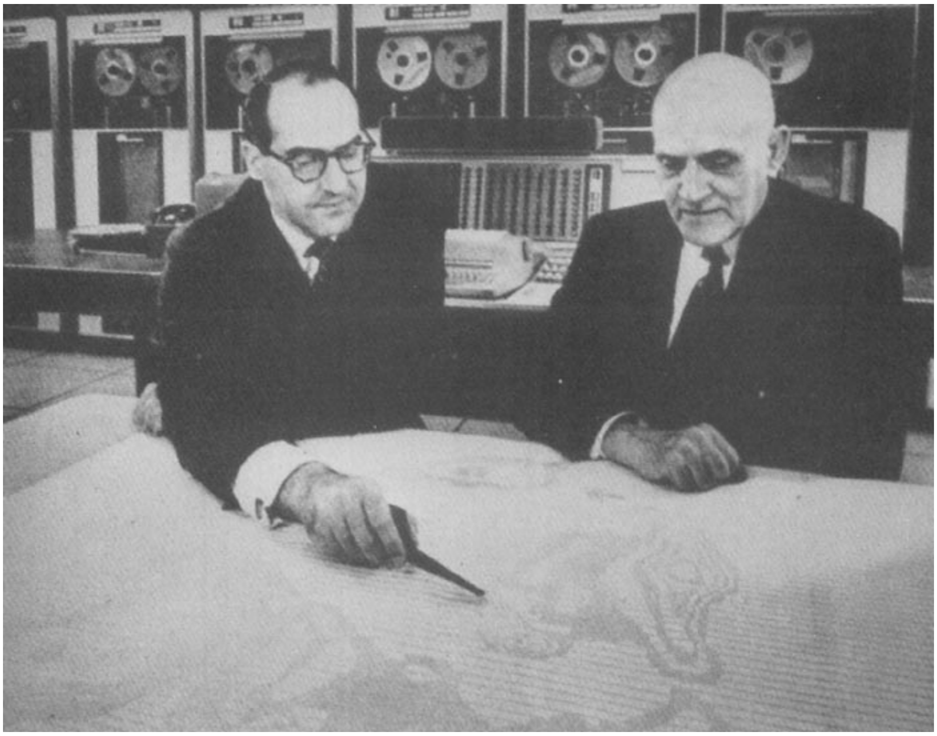


FIG. 35. Smagorinsky and Reichelderfer with electronic computer and computerized forecast map (AMS, 1963).

Boston, where M.I.T. had an airplane. At 7:00 am, 3:00 pm, and 5:00 pm, ascents were made through this front. We had no radiosondes at that time and, having participated in some of the flights (in a plane with glass bottom and sides), I can tell you that these were exciting days in meteorology. Figure 28 shows the 12-hourly positions of the front, and Fig. 29 gives a mesoscale analysis of the same front in terms of hourly positions determined from autographic records gathered from many points in New England. These figures are presented to give the flavor of what was done in the early 1930s.

Figure 30 is most historically important. In the 1930s, we in America had upper-air data only over the United States, but Rossby was working on his classical paper (Rossby, 1939) on long waves. One day he said, "Namias, why don't you try to get some sort of an idea what's going on aloft over the oceans where we don't have any upper data?" Following his lead, I constructed Fig. 30, making approximations as to lapse rate over the oceans from surface data and attempted differential analysis. The resulting 10000 ft chart excited Rossby immediately, for the long waves were vivid. Not even bothering to find a sheet of paper, Rossby immediately set to work right on the map to compute the motion of these waves according to this frequency equation. This triggered a whole new chain of thinking in the 1940s. Figure 31 illustrates our ideas of the relationship between the long waves and the individual cyclone family. Actually, Jack Bjerknes had the fundamental idea many years ago, and he was a key figure in the concept of long waves with an article written in 1937 (Bjerknes, 1937).

Figure 32 is an example of one of Palmén and Newton's beautiful cross sections. Note the jet stream, the frontal structures, and the excellent craftmanship. Of course, their book (Palmén and Newton, 1969) is a classic in these matters. Finally, after the satellite arrived, Vincent Oliver in the early '60s put together a mosaic (Fig. 33) that showed the surface fronts clearly in this cloud system picture over the North Pacific. However, already in the 1930s we at M.I.T. had developed isentropic analysis, which in 1939 led to one of our models (Namias, 1939, Fig. 34), showing the flow of moisture and dry air around an occluding cyclone. Comparing this with Fig. 31 indicates that we had a fairly good idea of the motion of the tongues that satellites have shown so dramatically.

Finally, Fig. 35 is a picture of Joe Smagorinsky and Reich going over one of the first numerical computerized predictions—the start of an entirely new era. Some people have thought, and perhaps still think, that the era of computing makes all that I have said merely matters of historical interest. Perhaps so, but I find that forecasters everywhere still pay attention to fronts. They now have real upper data and they have dozens of charts emanating from NMC; however, the frontal and the air mass concepts still play an important role in practical weather forecasting. There are new studies indicating that even numerical analysis may be better done with the help of isentropic surfaces than with constant-level surfaces. Some work of Shapiro (1975), Bleck (1974), Reed (1958), and Reed and Danielson (1959) give fresh ideas relating to upper-level frontogenesis. Terry Williams, who has done much work on fronts, believes that the fronts develop after the cyclones. I don't believe that fronts are dead, and in conclusion I want to reemphasize that I think that frontal and air mass methods will be with us for a long time. I find it difficult to imagine them disappearing from the forecasting scene.

Acknowledgments. My thanks go to C. K. Stidd for his assistance in helping to locate reference material and for editorial assistance and to Carolyn Heintskill for transcribing the tape of my lecture and for typing the manuscript. I also wish to thank F. W. Reichelderfer for his fascinating and historically important letters to me giving his recollections of the early days when air mass and frontal analysis was introduced on the American scene.

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A History of Numerical Weather Prediction in the United States

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1. Introduction

On such occasions it is customary, I believe, to offer one's apologies in advance. In the first place, I am not a historian or even much of a scholar. Certainly not a thorough scholar. In fact, it has occurred to me that it was probably a mistake to ask one of the minor actors in the drama to comment on the quality and interpretation of the entire performance. As one of the cast, I must admit to certain prejudices as to what the important problems are and have been, which approaches have been most promising, and which have been the most significant scientific and technological developments in a young, rapidly growing, and rather specialized field.

Accordingly, I should apologize for the somewhat pretentious billing for this talk. I can say in my own defense that the

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choice was not mine. Our worthy chairman was forced to concoct a title on short notice and wanted to give me the widest latitude possible. I appreciate his motives, but it would be more accurate to describe this history as "A Highly Personal and Anecdotal Account of Numerical Weather Prediction over the Past Thirty Years." I will try to be objective, and not to fall off the fine line between undue modesty and undue immodesty. I cannot presume to tell "The Definitive History" of numerical weather prediction.

I cannot live up to the advertising in another respect. That is, one cannot maintain proper historical perspective of a succession of related and complementary scientific developments if he restricts his view to events that took place only in a single nation or in a very recent era, isolated from past accomplishments in the whole international scientific community. Science just doesn't work that way. Accordingly, I shall review briefly the ideas and work that led up to the flowering of numerical weather prediction in the United States, and will occasionally refer to related or parallel work in this field abroad.

As a final note of apology, I hope I will be forgiven if I inadvertently overlook some important contributions, or if I appear not to appreciate the full significance of some development that I single out for attention. There is little of a long history that can be said in an hour. Well, so much for apologies.

2. Definition and early history of the problem

To whittle the subject down to barely manageable proportions, I am going to define numerical weather prediction as the process of solving the equations that govern the behavior of the atmosphere, starting with approximately known initial and boundary conditions. I will speak most specifically about purely numerical methods for solving those equations, but will not exclude analytic methods. However, I cannot begin to include all of dynamical meteorology.

In one very real sense, the whole thing began with Isaac Newton. Even though the theoretical framework of fluid dynamics was not yet complete, Newton's second law of motion alone suggested to his contemporary Halley (and, 50 years later, to Hadley) a simple and plausible explanation for the existence of the persistent northeast trade winds. This was the first instance of the new mechanistic view as applied to the large-scale meteorological motions of the atmosphere. I will not elaborate on these works, since they have already been described in more general histories. There was, however, no predictive element in these early treatments, for the simple reasons that the thermodynamics was still missing and the phenomenon under discussion was very nearly stationary in time. The laws of fluid motion and the principle of mass conservation were well known, but the relation between the heating of a fluid and its thermodynamical state was not. But with the discoveries of Boyle, Charles, Count Rumford, Laplace, and Joule, and with von Helmholtz's (1858) formulation of the first law of thermodynamics in the mid-19th century, the last major piece of the purely hydrodynamical puzzle was dropped into place. To put the situation as of 1858 in perspective, the set of equations that describe the behavior of a nonviscous fluid in adiabatic motion was then formally complete, in the sense that the number of state variables was exactly equal to the number of independent equations.

It was not until 1904, however, that Vilhelm Bjerknes—in a remarkable manifesto and testament of deterministic faith—stated the central problem of numerical weather prediction. This was the first explicit, coherent recognition that the future state of the atmosphere is, *in principle*, completely determined by its detailed initial state and known boundary conditions, together with Newton's equations of motion, the Boyle-Charles-Dalton equation of state, the equation of mass continuity, and the thermodynamic energy equation. Bjerknes went further: he outlined an ambitious, but logical, program of observation, graphical analysis of meteorological data, and graphical solution of the governing equations. He succeeded in persuading the Norwegians to support an expanded network of surface observation stations, founded the famous Bergen School of synoptic and dynamical meteorology, and ushered in the famous polar front theory of cyclone formation. Beyond providing a clear goal and a sound physical approach to dynamical weather prediction, V. Bjerknes instilled his ideas in the minds of his students and their students in Bergen and Oslo, three of whom were later to write important chapters in the development of numerical weather prediction in the United States. We shall refer to Rossby, Eliassen, and Fjörtoft later.

It is perhaps unfortunate that V. Bjerknes was so strongly influenced by C. A. Bjerknes's (père) predilection toward differential geometry and graphical methods. This was a serious limitation, simply because graphical operations could then be carried out only manually.

In 1922, the Cambridge University Press published one of the strangest, but most imaginative, contributions to the whole literature of meteorology, written by a rather obscure, slightly eccentric, and unconventional Englishman by the name of Lewis Fry Richardson. Its title was *Weather Prediction by Numerical Process*, and it outlined a rational method by which tomorrow's weather could be calculated from today's meteorological observations. Solidly based on fundamental physical principles and the eternal mathematical verities, Richardson's method of prediction might have been expected to remove one of the greatest of nature's uncertainties—the weather to plant, sow, harvest, hunt, fish, or sail by. But Richardson's book did not remove those uncertainties. It was a candid report of an admitted, but glorious, failure.

In the limited correspondence and personal contact between V. Bjerknes and Richardson, there is ample evidence that the latter was either influenced by Bjerknes' view of the *physical* problem, or at least agreed with it. It is clear, however, that Richardson had his own ideas about the *mathematical* formulation of the problem. Both understood the necessity of approximate methods in solving highly nonlinear equations, but, whereas Bjerknes was inclined toward graphical methods, Richardson had an early appreciation of discrete variable methods—notably the method of finite differences. The principal virtue was that discrete variable methods were reducible to simple arithmetical computations that could, in principle, be carried out by an automaton.

Richardson (later a versatile and highly original statistician and economist) had a lively interest in the new method of finite differences and set out to apply them to the problem of weather prediction. Over a period of months, between ambulance trips to the Front during the last stages of World War I,[2] he completed a test calculation *by hand* and half-finished a manuscript in which he described his method and results. Ironically enough, all his papers disappeared in the general confusion of war. They were eventually found under a coal heap in Belgium, returned to Richardson, and were later expanded into *Weather Prediction by Numerical Process*.

Richardson's trailblazing book on weather prediction is an oddly quixotic effort. He describes his method in meticulous detail, but his computations predicted tendencies so large that the large-scale atmospheric disturbances would move at speeds comparable with those of sound waves. The latter conclusion was clearly at odds with observation.

Near the end of his book, he describes a phantasmagorical vision of the "weather factory"—a simply enormous organization of specialized human computers, housed in a huge hall, directed by a conductor perched on a raised pulpit, and communicating by telegraph, flashing colored lights, and pneumatic tubes. He estimated that, even using the new-fangled mechanical desk calculators, it would take about 64 000 human automata to predict weather as fast as it actually happens in nature.

Richardson's preface ends with a rather wistful, but prophetic statement: "Perhaps some day in the dim future it will be possible to advance the computations faster than the weather advances and at a cost less than the saving to mankind due to the information gained. But that is a dream." That "dim future" came 25 years later.

3. The renaissance of numerical weather prediction

Aside from more fundamental difficulties, the most discouraging aspect of Richardson's proposed method was the sheer volume of calculation required. It was apparent that an absolutely necessary ingredient in the success of any practical scheme of "numerical weather prediction" was a computing device that was capable of calculating a one-day forecast in less than 24 hours. Even Richardson probably underestimated the "administrative overhead" in dealing with automata. In retrospect, we now see that a one-day prediction, based on a simplified version of the hydrodynamical equations, requires on the order of 10^9 numerical and logical operations. The requirements for total data storage (memory) capacity and rate of data transfer from storage to processor are equally severe. What was clearly needed was a computing organism capable of performing something like 10^4 operations per second.

During World War II and the years immediately following, substantial progress was made in designing numerical processors in which the "switching" elements were not cogged wheels or electromechanical relay switches, but consisted essentially of radio "tubes" (or electronic switches), which have virtually no mechanical inertia. By 1945, in fact, Eckart and Mauchly had designed and built processors with speeds of the order of one logical operation per millisecond. These developments brought the basic operating speed of the processor to within one order of magnitude of that required for the routine application of numerical methods to weather prediction.

But there were still two basic deficiencies in the system. First, the data storage device—an acoustic delay line—was limited by the density of sound impulses that could be cyclically regenerated and propagated through a mercury-filled tube. Second, and more important, the programming of the processor was completely external and "human-limited" in the sense that the entire sequence of instructions to the processor was written out in advance and conveyed to it instruction-by-instruction through a manually wired plugboard and by setting many switches.

The big breakthrough, however, was not dependent on sheer hardware development. It arose from von Neumann's realization that computing machines of this class must be "self-programming"—i.e., it should not be necessary to tell the computer what to do in complete detail. If, as a simple example, the same sequence of operations is to be performed on different sets of data, one may achieve a degree of self-programming capability by storing the operands (numbers to be operated on) at enumerated "addresses" or locations in the machine's memory and, in addition, storing in its memory the execution orders, *which include the address of the operand*. Thus, the basic execution cycle for one set of data (or operands) may be reused merely by concluding the cycle with a sequence of instructions to change the addresses of the operands that appear in the basic execution cycle. One then repeats the whole cycle. This is the essential logical basis of "stored-programming," which broke the human bottleneck of writing out the entire sequence of instructions in advance. This feature of the stored-program machine made it ideally suited to the demands of large-scale hydrodynamical calculation.

Early in 1946, von Neumann singled out the problem of numerical weather prediction for special attention. Although he had a deep appreciation of its practical importance and intrinsic scientific interest, he also regarded it as the most complex, interactive, and highly nonlinear problem that had ever been conceived of—one that would challenge the capabilities of the fastest computing devices for many years.

1946 was a year of ferment, for the formulation of the problem and the means of solving it were at last moving toward each other, albeit not by design. (Appendix A is the initial proposal to establish the Meteorology Project.)

Late in 1945, I had been assigned to the so-called Divergence Project at UCLA, the objective of which was to calculate surface pressure tendencies by vertical integration of the hydrostatic and continuity equations, using direct observations of winds and pressure. I was a little frustrated, having become aware of one major difficulty and two serious limitations on the applicability of the results to practical forecasting. These were:

1) At any particular level, the horizontal divergence of the wind field tends to consist of two individually large, but almost compensating, effects; namely, the confluence of streamlines and variations of wind speed along the streamlines. In fact, a simple error analysis indicated

[2] Richardson, a Quaker and pacifist, did not shirk his duty to relieve suffering. He drove an ambulance.

that the error due solely to roundoff errors in the reported winds was fully as large as the true divergence. This, as it later turned out, is a fundamental source of difficulty and will be referred to later as the "divergence error."

2) One of the shortcomings of the "tendency equation" is that it involves several variables. Thus it is not self-contained, nor is it easily combined with other variants of the hydrodynamical equations to form a complete set.
3) To determine the pressure tendency at any arbitrary level, one must determine the vertical mass flux through the bottom of the column, which requires that one know the vertical air speed at any level. The latter is not, of course, observed directly, nor was it immediately obvious how it could be calculated indirectly.

Working more or less in isolation, I was discouraged enough by my estimate of the pernicious "divergence error" that I virtually abandoned the original objectives of the Divergence Project and concentrated on two basic limitations on the whole approach. I first derived a diagnostic relation between the vertical air speed and the contemporaneous fields of pressure and horizontal velocity. This gave me a complete and fairly manageable set of equations. What I did not know was that my "new" equation had been derived by Richardson at least 28 years earlier.

Next, I set about devising a numerical method for solving the complete set of equations, essentially by Taylor-expanding all of the variables around their initial values. This involved a lot of substitution and resubstitution from several messy-looking equations. My mistake lay in seeking a single equation in a single unknown: it would have been easier if I had calculated the time evolution of each variable separately in a stepwise fashion. On later analysis, however, it emerged that the finite-difference form of my equation was equivalent to the finite-difference form of Richardson's equations. But I didn't know about Richardson then. This incident proves the value of reading. If I had read the book, I would have saved several months of pencil-chewing and head-scratching.

In the spring of 1946, I voiced some of my concerns and aired my views to a brand-new Ph.D. at UCLA, a fellow by the name of Jule Charney, whose office was next to mine. He had just finished a brilliant thesis on the instability and structure of baroclinic waves, and I thought that if anybody could criticize my ideas, he could. He could and did. By and large, however, we agreed on the nature of the problem and its attendant difficulties. Unfortunately for me, Charney left on a National Research Council Fellowship, first to visit Rossby's Institute of Meteorology at the University of Chicago, and then to work with Eliassen and Fjörtoft at the University of Oslo.

Meanwhile I ground away at an old Monroe desk calculator, trying to figure out short-cuts and becoming increasingly depressed by the burden of hand calculation. Then one fine afternoon in the early autumn of 1946, Prof. Jörgen Holmboe called me in, said that he was aware of what I was trying to do, and handed me an article from the *New York Times Magazine*. It was an interview with Prof. John von Neumann of the Institute for Advanced Study and Dr. Vladimir Zworykin of RCA, in which they announced their intention of developing a very high speed electronic computing machine and of applying it to the prediction of natural weather and of calculating the effects of human intervention in the natural processes of the atmosphere. The implications of this development did not fall on deaf ears. Indeed, it was downright heady stuff.

To mix metaphors, the grass didn't grow long under my feet. Next day, I called my commander, Gen. Ben Holzman, and requested authorization to travel to Princeton to meet with von Neumann. Also, if that were granted, would he please arrange the meeting. I was a pretty brash young man, but I felt that the chances of a greenhorn first lieutenant making any headway with a giant of von Neumann's stature were virtually nil unless I had a patron. Gen. Holzman grumbled a bit, but agreed to it if I traveled as extra crew on a military aircraft that was headed East anyway. The following day the arrangements were clear, and I made my way to Princeton via B-29, bus, stagecoach, train, oxcart, and the PJ&B.[3] My séance with von Neumann is not very clear in my memory: it couldn't have lasted more than an hour, and I was slightly overawed. But I had my speech pretty well set and blurted it out. Mostly, I just said what I wanted to do and why, and how I proposed to go about it. We talked a while about the computing problems. After about half an hour he asked if I would like to join his Electronic Computer Project as a meteorologist working on problems of numerical weather prediction. That question took no pondering. Then he asked how my assignment should be arranged. I suggested that he call Gen. Holzman and request it. He called, talked for a few minutes, held the phone and said Gen. Holzman would like to speak with me. The conversation was very short and one-sided. It went something like, "Well, I guess you'd better go back and get your gear. Orders will follow."

Unbeknownst to me at that time, there had been a now-famous meeting of interested meteorologists at Princeton in August 1946, instigated primarily by that great diplomat and entrepreneur, Carl-Gustav Rossby. The list of participants included the cream of the U.S. dynamical meteorologists, well-placed officials in the U.S. weather services, and representatives of potential funding agencies: John von Neumann, C.-G. Rossby, Harry Wexler, J. Jaw, B. Haurwitz, V. P. Starr, R. B. Montgomery, H. C. Willett, A. Cahn, Jr., H. Panofsky, G. A. Hunt, J. E. Miller, C. L. Pekeris, J. Namias, W. M. Elsasser, J. Charney, Lt. Cmdr. D. F. Rex, and R. Elliot. To my knowledge, this was the first time that all of the essential ingredients of success in this venture were brought together: a well-formulated problem; the technological means of solving it; the people, money, and other support needed to solve it; and, finally, the institutional mechanisms around which the whole effort could be organized. In the latter regard, Dr. Harry Wexler, Dr. Francis Reichelderfer, and Dr. Earl Droessler were particularly influential in gaining support.

Largely as a result of this meeting, von Neumann had already assembled a formidable and almost intimidating array of talent by late fall of 1946. Resident at the Institute at the moment of my arrival there were:

Gilbert Hunt, a wartime-trained meteorologist, but at bottom a mathematician. He was then preoccupied with his doctor's thesis, but looked into the problem of numerical

[3] PJ&B means Princeton Junction and Back.

weather prediction to the extent of generalizing Jean Leray's proof that solutions of the Navier-Stokes equations actually exist. I received considerable help from him in finding limiting values of the phase-speed of dispersive waves.

Dr. Chaim Pekeris, who had studied at M.I.T. with Rossby in the pre-World War II days. His interests were primarily in the behavior of high-energy blast waves in nonhomogeneous media, and in tidal theory, either atmospheric, crustal, or oceanographic.

Prof. Paul Queney, of the Sorbonne, whose research lay mainly in the linear theory of steady-state flow over long ridges. When I first arrived, I shared an office with Queney in a sort of dormer just above the eaves of Fuld Hall, the main building of the Institute for Advanced Study. Our scientific discourse was negligible. Between my schoolboy French and his faulty English, however, we managed to figure out his income tax.

There had been a fourth member of the group, Albert Cahn, who departed shortly before I came on the scene. Cahn, who had collaborated with Rossby on the problem of geostrophic adjustment, left a single, brief memorandum in which he simply restated the hydrodynamical equations, but with no prescription for solving them. So you can see that it was a pretty mixed bag.

One by one, all of these stalwarts left: Hunt, to Cornell; Pekeris, to Israel; Queney, back to France. Moreover, von Neumann was deeply engaged in other affairs. Once again I was almost alone, except for the fairly regular exchanges of visits with Profs. Haurwitz and Hans Panofsky, then both at N.Y.U. Panofsky had definite ideas about numerical approximations, and Haurwitz (from whose book I had learned most about dynamical meteorology) encouraged me to learn more about hydrodynamics.

In spite of my isolation, this was not at all a sterile period. I had discovered Richardson's book and read it. His results confirmed my deep suspicions of the "divergence error" and laid bare a similar type of error, due to calculating the horizontal acceleration of air as the small difference between the pressure-gradient and Coriolis forces per unit mass, both of which may individually contain sizable errors. I read systematically through Lamb's *Hydrodynamics*. From von Neumann and Goldstine I learned something of numerical analysis and was amazed to discover that the solutions of finite-difference equations may be nothing like the solution of the corresponding differential equation. In particular, the stability of *numerical* solutions does not depend on the absolute magnitude of the finite increments of space and time, but on their ratio.

Through reading and discussions at the Institute, I learned some lovely mathematical techniques, but that wasn't getting us much further toward the main problems. I was acutely aware that one small boy wasn't enough to grapple with them.

In February of 1947, I wrote a rather long and rambling letter to Charney,[4] who was still at Rossby's Institute at the University of Chicago, propounding a few questions. One of these, I recall, had to do with distinguishing sound, gravity, and planetary waves in the initial state. A related question was why don't the large-scale transient cyclones of the mid-latitudes travel at the speed of sound or gravity waves. In his return letter, Charney's first (and not serious) argument was anthropomorphic: If cyclones traveled at the speed of sound, all humanity would have been blown off the face of the earth. Q.E.D.: Cyclones are not sound waves.

His second answer was that it must have to do with the stability and dispersion properties of a rather gently forced system—e.g., one which is not continually disturbed by thermonuclear blasts. In his reply he proposed that he stop off at Princeton for a few days late in March (1947), when we could discuss these questions at greater length and leisure.

This came to pass, and a very stimulating time it was. I think that Charney and von Neumann had met at least a year before, but they certainly met on this occasion. After several days, innumerable pitchers of beer, and many late hours, Charney went on to Oslo. By this time he had convinced himself that something had to be done to distinguish sound, gravity, and Rossby waves.

I had begun some self-educational studies of nonlinear Rossby waves and waves in nonhomogeneous media, but became increasingly distressed about the practical problem of matching the meteorological effort to the development and eventual emergence of a stored-program machine. Since my days at Princeton were numbered, I expressed some concern about this, but I needn't have bothered. Von Neumann, between his myriad enterprises and distractions, was fully aware of the problem and had already considered how to deal with it. He asked my opinion of this fellow Charney as a long-term member of the group and the scientific leader of a small nucleus of people working on the physical and mathematical formulation of concrete numerical experiments. I assured him he could find no better combination of mathematical savvy and physical insight, but also ventured to suggest that Eliassen be invited for a protracted visit, since he and Charney had complementary experience and similar interests, and also had been working well together. In any event, negotiations were opened, and both Charney and Eliassen were duly invited.

That von Neumann's judgement was eminently sound was evidenced by the underground reports of Charney's work in late 1947 and its subsequent publication in *Geofysiska Publikasjoner* in 1948. This was certainly the most significant contribution to numerical weather prediction since Richardson's magnum opus, and far exceeded Richardson's work in its profundity and implications. Briefly, in his paper "On the Scale of Atmospheric Motions," Charney made an ingenious analysis of the magnitudes of the various dynamical, kinematic, and thermodynamical effects that are reflected in the hydrodynamical equations. Even more important, he showed that the ostensible difficulties due to the "divergence error" and the almost exact mechanical balance between the pressure gradient, gravitational, and Coriolis forces could be avoided by discriminate, but systematic, introduction of the geostrophic and hydrostatic approximations, and that these conditions characterize the large-scale meteorological motions in middle and high latitudes. Finally, he derived a single partial differential equation in one unknown—pressure or isobaric height—whose solutions demonstrably do not cor-

[4] The reply to which is appended (Appendix B). Charney's letter of 12 February 1947 contains the germinal idea that later led to his famous paper "On the Scale of Atmospheric Motions" (1948).

respond to sound or gravity waves (Charney, 1949). Charney's 1947 formulation, later known as the quasi-geostrophic model, simultaneously skirted two major difficulties: first, it imposed much less stringent conditions for computational stability, and second, it did not demand that the horizontal divergence or accelerations be computed as small differences between large and compensating terms, each of which is subject to sizable percentage errors. These features alone evaded the two fundamental difficulties inherent in the practical application of Richardson's method.

At this point I must not fail to recognize and commend a paper by Eliassen, published in the same 1948 issue of *Geofysiska Publikasjoner* as the one in which Charney's paper appeared. Eliassen's paper was concerned not only with the elegant and systematic use of isobaric coordinates, but also includes the derivation of a "quasi-geostrophic" equation that is essentially equivalent to Charney's. Quite literally, something was in the wind, even if it blew from slightly different quarters.

Charney arrived in Princeton in midsummer of 1948, and Eliassen followed by a couple of months. I was then slated to organize and direct one of the divisions of the newly established Air Force Cambridge Research Laboratories (AFCRL) in Cambridge, Mass., and was due to leave Princeton late in the fall of 1948. That prospect was exciting in many ways: it was certainly challenging. I did, however, feel twinges of regret for leaving the group at Princeton at a time when the action was really starting.

One of my first official acts at AFCRL in the fall of 1948 was to build up a small group of people to capitalize on some of the ideas of Rossby and Charney. More specifically, we were concentrating on the propagation of Rossby waves in 2-dimensional flows.

Meanwhile, Charney and Eliassen initiated and completed a nice study of the effects of orography, based on linear theory, but including the mechanism of Ekman "pumping" through the boundary layer. To my knowledge, this was the first time this effect had ever been applied in the analysis of a *real* geophysical problem. Their paper appeared in *Tellus* in 1949.

Eliassen returned to Oslo in the fall of 1949. As if by prearrangement to maintain a steady flow of Norwegians through the Princeton Project, Fjörtoft appeared to take his place. Soon after his arrival, Fjörtoft, Charney, and von Neumann collaborated on the design of a numerical prediction experiment, using the equations for the nondivergent barotropic model, but starting with real initial conditions at about the 500 mb surface.

At that time the Princeton machine was still far from operational. Accordingly, the calculations were programmed for the computer ENIAC (Electronic Numerical Integrator and Calculator), the only extant electronic machine, at Aberdeen Proving Ground, where von Neumann was a consultant. The programming, done by plugboard and manual switching of a huge bank of keys, was supervised primarily by Prof. G. W. Platzman, Dr. Joseph Smagorinsky, and Dr. John Freeman. For reasons of which I am unaware, the whole operation had to be carried out continuously, on an around-the-clock basis, with the whole gang coming in and going off shift. The first job was pulled off successfully early in April 1950. The results looked good, certainly better than Richardson's prediction. Later there was a triumphal celebration. I have a rather bad reproduction of a photograph of some of the participants and visiting dignitaries (Fig. 1), but I'm sure you will recognize most of them. Going from left to right: Harry Wexler, John von Neumann, M. H. Frankel, Jerome Namias, John

FIG. 1. Visitors and participants in the 1950 ENIAC computations (left to right): Harry Wexler, John von Neumann, M. H. Frankel, Jerome Namias, John Freeman, Ragnar Fjörtoft, Francis Reichelderfer, and Jule Charney.

Freeman, Ragnar Fjörtoft, Francis Reichelderfer, and Jule Charney. It was a great day—the day of the first successful numerical weather prediction. The results are summarized in a paper entitled "Numerical Integration of the Barotropic Vorticity Equation," published by Charney *et al.* (1950) in *Tellus*.

A second expedition to ENIAC, organized by Platzman and Phillips, took place in June 1951. This was principally a test of numerical procedure in a case of analytical initial conditions. The results displayed a new type of numerical instability, probably *nonlinear* in nature, due to aliasing errors. This was the first clear manifestation of this kind of phenomenon: as we shall see later, however, it took another five years to get it sorted out, and it isn't yet laid to rest.

4. The rapid proliferation of research in NWP

Needless to say, the *Tellus* paper of 1950 excited considerable interest, but, even before its publication, the basic approach and numerical methods had spread through the grapevine. At the same time, everyone was aware that those calculations were based on the principle of absolute vorticity conservation for 2-dimensional flow, which precluded the intensification of circulation centers and did not provide for the formation of new centers where none existed before. Accordingly, there was a general rush to develop baroclinic models—i.e., models whose vertical structure was simple enough that the equations could be solved without undue computational strain, but general enough that they could simulate cyclogenesis and conversion of available potential energy to the kinetic energy of growing disturbances.

In a relatively brief span, 1951–53, no less than six simple baroclinic models were proposed, two of which were tested in a few real cases: all were variants of the general quasi-geostrophic model developed by Charney in his paper of 1949. They all also contained some elements of adiabatic thermodynamics. The first of these was Phillips "two-layer" model of 1951. Eady (1952) and Eliassen (1952) discussed some propagation and stability properties of "two-parameter" models, whose states are characterized by the horizontal distribution of two variables—e.g., the heights of two geopotential surfaces, or the height and temperature of a single geopotential surface. Independently, Charney and Phillips (1953), Sawyer and Bushby (1953), and Thompson (1953) formulated essentially equivalent "two-parameter" models, and Charney and Phillips carried out tests in a single case of spectacular cyclogenesis: that was the famous Thanksgiving Day storm of 1950. Their results were encouraging, but slightly suspect, since certain coefficients in the equations had been "tuned" or adjusted to yield optimum agreement with observations.

In the following year, Bushby and Hinds (1954) published the results of tests of the Sawyer-Bushby model in a number of cases. In June 1954 Thompson and Gates completed the analysis of a series of 120 forecasts based on Thompson's vertically integrated two-parameter model, using real initial data. This was the most comprehensive test of both barotropic and two-parameter baroclinic models up to that time (Thompson and Gates, 1956). It was carried out by the Numerical Prediction Project at AFCRL, staffed jointly by Air Force and Weather Bureau people, military and civilian, in preparation for the imminent application of numerical methods to short-range weather prediction.

Our findings at that time, to summarize them briefly, were that:

1) The general level of performance of the two-parameter baroclinic model in predicting 500 mb height fields was virtually indistinguishable from that of the non-divergent barotropic model.
2) The quality of 1000 mb forecasts was slightly lower than that of 500 mb forecasts.
3) There was a strong indication that the accuracy of all forecasts was adversely affected by orographic effects over and in the lee of the Sierra Nevada and Rocky Mountains.
4) The arbitrary specification of boundary conditions around an area of continental proportions rapidly contaminates the prediction in the interior, seriously infecting about one third of the area in a period of 24 hours.
5) Spatial truncation error results in a systematic underestimate of the eastward speed of propagation, particularly for small-scale disturbances.

To a considerable extent, these deficiencies are still with us.

Perhaps one of the most significant facts which emerges from the foregoing description of the development of quasi-geostrophic models is that, by 1952, there were no less than four sizeable research groups who were concentrating on the problem, namely: the Meteorology Project at the Institute for Advanced Study, the Atmospheric Analysis Laboratory of AFCRL, the Napier Shaw Laboratory of the British Meteorological Office, and the International Meteorological Institute of the University of Stockholm, working in cooperation with the University of Oslo. The development of numerical prediction had become an organized and well-supported research movement, comprising a substantial fraction of the total meteorological research effort. Let us admit at once, however, that this would not have happened without collateral advances in the technology of computing, communications, and numerical analysis.

5. The establishment of operational NWP

By the summer of 1952, there was mounting evidence that the crudest of numerical methods was capable of attaining an average accuracy comparable with that of forecasts prepared by conventional methods. Recognizing the potential of more sophisticated numerical methods and the equally important advantages of data-processing by high-speed automatic computing machines, a number of well-placed scientists and military officers brought these new developments to the attention of the Joint Meteorological Committee under the Joint Chiefs of Staff. As a result, this committee, composed of the heads of the Air Weather Service of the U.S. Air Force, the U.S. Weather Bureau, and the Naval Weather Service, commissioned a special subcommittee in late 1952 to review the current state of development, to estimate the trend of development, and to advise the Joint Meteorological Committee on the desirability of establishing an operational numerical weather prediction unit. An amended resolution of the Joint Meteorological Committee requested this same subcommittee to investigate the requirements of a numerical prediction unit, to advise on the feasibility of activating such a unit, and to lay plans for its establishment. With excellent cooperation between the three U.S. weather services, the subcommittee completed its survey, made its recommendations, and drew up the plan for the first operational numerical weather prediction unit by late in the summer of 1953. Briefly, the subcommittee found that numerical methods of weather prediction had already advanced far enough to justify putting them into practice, that it was feasible to do so, and that the best way to form an effective organization of people and physical facilities was to pool the resources of the three weather services. A mark of the subcommittee's understanding and foresight was its recognition that the further development of numerical prediction methods would be a necessary, slow, and generally unspectacular process, and that it should go hand-in-hand with the daily routine of numerical weather forecasting. Accordingly, the subcommittee recommended that a research and development group should be an integral part of the first operational numerical prediction unit.

The subcommittee's recommendations were put into effect immediately upon the parent committee's approval, and the Joint Numerical Weather Prediction (JNWP) Unit was officially established on 1 July 1954. By that time, a nucleus of key people had been assembled in Washington, and after performance tests of several production models, a high-speed electronic computer was ordered for delivery in the following spring. From its beginning and up to 1961, the JNWP Unit was jointly staffed, financed, and supported by the three U.S. weather services, with Air Force and naval officers working side-by-side with their civilian scientific colleagues. In 1961, it was made a division of the National Meteorological Center, then under ESSA and now NOAA.

At the inception of the JNWP Unit, the Director was Dr. G. P. Cressman, now [1976] head of the U.S. National Weather Service. I was head of the R&D Section, consisting originally of Dr. F. G. Shuman (now [1976] head of the National Meteorological Center), Major H. A. Bedient (Air Force), Cmdr. Paul Wolff (Navy), and Lt. Cmdr. William Hubert (Navy). The early members of the Applications Section were Dr. J. Smagorinsky (head), Charles Bristor, Dr. G. Arnason, Louis Carstenson, and Lt. Col. H. Zartner (USAF). The chief and general factotum of the Analysis and Operations Section was Edwin Fawcett. Although the original group is now widely dispersed, all have risen in the world. To see this, all you need to do is look at the top echelons in NOAA's administrative structure.

The first of the JNWP models were Thompson's two-parameter baroclinic model and a stripped-down barotropic version. Both had previously been coded up by Gates and Zartner in 1953 at AFCRL. Although the latter circumstance did not dictate the JNWP Unit's choice of the IBM 701 as its first computer, it had been anticipated that the 701 would be a strong contender. Accordingly, the AFCRL code was written for and tested on the 701 at IBM Headquarters in New York during the winter and spring of 1954. Not long after the establishment of the JNWP Unit, we started programming a three-level, quasi-geostrophic model, whose design was begun by Cressman while he was in Princeton in 1953. This model became operational at the beginning of routine numerical weather prediction on 15 May 1955. The practice of numerical prediction was launched and on its way. What was a gleam in the eye in 1945 was a working reality in 1955.

6. Later developments in deterministic prediction

That isn't to say, however, that the forecasts were perfect. By and large, they accounted for about 65% of the variance in day-to-day changes of the large-scale circulation patterns, so there was plenty of room for improvement.

At that time, it was natural to assume that a considerable part of the residual error was due to the physical approximations of the quasi-geostrophic model, and that the use of the original, unmodified hydrodynamical equations (or "primitive" equations) would do much to correct the defects. Although this was even then demonstrably not the whole case, an appreciable effort has been put into the formulation of the "primitive" equations over the past 20 years.

In effect, the return to the primitive equations took us straight back to Richardson, but with one important difference: we then realized that even small errors in the initial data may generate gravity-inertial oscillations of large amplitude, which may obscure or severely distort the large-scale perturbations of primary interest. The rather stringent conditions for computational stability must still be satisfied, but this is, after all, only an economic constraint. It is also necessary, however, to adjust or "balance" the initial conditions in such a way that they do not generate large-amplitude gravity waves. These new difficulties were clearly recognized in Charney's (1955) paper on "The Use of the Primitive Equations of Motion in Numerical Prediction." Later papers, dealing with some important technicalities of the integration problem, are by Platzman (1958) and Hinkelmann (1959).

The problem of "balancing" the initial conditions was attacked independently by Bolin (1956) and Thompson (1956). In the latter paper, it was shown that a necessary and sufficient condition for the exclusion of gravity-inertial oscillations is the omission of the total derivative of divergence from the divergence equation. With this approximation, the divergence equation is a "balance" equation, a diagnostic relation between the pressure and wind fields.

The next big advance, at least in my opinion, was Phillips's introduction of radiative and convective input of available potential energy and dissipation of kinetic energy into a hemispheric model. Starting with conditions of relative rest, Phillips first calculated the time evolution of the zonally symmetric state and then superposed a random and zonally asymmetric perturbation at the time when the zonal motion should have become baroclinically unstable. After a long time-integration (about three weeks of simulated time), he then computed the zonally averaged statistics of the model. These were found to agree remarkably well with observed monthly and seasonal statistics of the real atmosphere. This experiment pointed the way for extending short-range prediction methods to the problem of medium-range forecasting and climate studies. This work was done virtually single-handed, and, for it, Phillips deservedly received the Napier Shaw Prize in 1956.

Although Phillips (1956) was first on the scene in this particular field, it is only fair to point out that the refinement of the radiative and transport calculations in more sophisticated "general circulation models" has been an arduous and time-consuming enterprise—in total about two orders of magnitude greater than Phillips' original effort. I refer specifically to the work of Smagorinsky (1963), Mintz and Arakawa (1964), Leith (1965), Kasahara and Washington (1967), and Somerville *et al.* (1974). I would also like to emphasize that this problem will not be solved at a single stroke, or by some kind of "breakthrough." It will, in fact, require a long-term, cooperative effort on the part of creative scientists from every branch of the atmospheric sciences. We've come a long way, but we still have a long way to go.

At the present time [1976], the standard numerical prediction model of the U.S. National Weather Service is a six-level representation based on the primitive equations and an initialization based on the "balance" equation, developed by F. G. Shuman and his colleagues over a period of years. The finite-difference formulation is very complicated, with various linear smoothing and unsmoothing operations which make its non-linear behavior extremely difficult to analyze. Perhaps it is a little uncharitable to say that the behavior of the six-level primitive equation model at 500 mb is not strikingly better than that of the old barotropic model. In some special respects and in certain regions and situations it is better, but only slightly so. Something is still amiss.

Before leaving the evolution of deterministic prediction models, I should at least mention some of the important contributions to the purely mathematical representation of the atmospheric system and to the technological aspects of numerical equation-solving. With regard to the former, one must give due recognition to the early work of Haurwitz (1940) and later generalizations of orthogonal representations by Baer and Platzman (1961). Extensions to empirical orthogonal representations were effected by Oboukhov (1960), Lorenz (1963), and Holmström (1963).

In the realm of numerical analysis, one cannot avoid citing the von Neumann perturbation method for establishing the necessary conditions for computational stability, work that is not clearly documented, but certainly dates back to at least the early '40s. As it turned out, however, the necessary conditions were not always sufficient in the case of highly nonlinear equations. This was first shown by Phillips (1959), who exhibited some simple examples of computational instability due solely to aliasing errors in calculating the interaction between modes of different scales. Although everyone is now aware of the existence of this type of instability, no one has yet found a completely satisfactory prescription for curing it.

7. Predictability and stochastic-dynamic prediction

Up to 1956, virtually all of the people involved in the development of numerical methods took a strictly deterministic view of the prediction problem—i.e., that the future state of the atmosphere is completely determined by its present state. In 1956, I found myself in the invidious position of defending this view in public debate. I may have won the argument then, but I certainly wasn't comfortable with my own reasons. Even if the proposition were true, the detailed current state of the atmosphere is known only in some probabilistic sense, and the ensuing prediction of the future is also correct only in that same sense. The question then arises: Granting that the reconstructed initial value of a variable at each gridpoint is its most probable true value, but with a known error distribution, how does the most probable value and its associated error distribution evolve with time through the course of the prediction? If, for example, the error distribution becomes more and more "smeared-out," the prediction may be no better than a sheer guess, and possibly even worse than a forecast based only on the climatological mean value.

Later in 1956, I tried to devise a mathematical technique for dealing analytically with this problem, and slightly more than half succeeded. Fortunately, Oboukhov saw my paper (Thompson, 1957) on the predictability question and passed it on to one of his most brilliant students, E. A. Novikov, who added the mathematical rigor and refinements needed to make this a complete work. I gather that this was his thesis problem. It was published in 1959.

The question did not surface again until 1963, when Lorenz examined the gradual departures of states evolving from near-analogs of the initial state of the actual atmosphere. Although this study was not completely conclusive owing to the fact that no near-analogs were found, the observed rate of departure was in good agreement with Novikov's and my theoretical estimates.

The question was revived again by G. D. Robinson in his Presidential Address to the Royal Meteorological Society in 1967. He pointed out that, assuming a nonlinear "transfer of uncertainty" from unresolvable small scales of motion to larger scales, all predictive value would be lost after about two days. This argument was not totally convincing, however, because it led to an estimate of predictability that is lower than the level that is actually achieved in practice. Moreover, Robinson's estimate was based on Kolmogoroff's famous "—5/3 power" energy spectrum of 3-dimensional isotropic turbulence, whereas it has been observed by Wiin-Nielsen (1967) and Kao *et al.* (1966) that the spectrum of large-scale atmospheric "turbulence" closely approaches the "—3 power" law predicted by various theories of 2-dimensional or quasi-geostrophic turbulence. A consequence of the stronger decrease of energy toward small scales is that the "transfer of uncertainty" to larger scales is slower, increasing the range of predictability by a factor of two or three.

Questions of this kind have been investigated in a broader and more realistic theoretical framework by Lorenz (1969) and by Leith and Kraichnan (1972), using the techniques and formalism of recent theories of turbulence.

Recognizing that predictions should ideally be stated in terms of probability distributions, Epstein (1969) proposed to compute at least their low-order moments—i.e., the *ensemble* mean value and the variance around the mean. This approach to stochastic dynamic prediction leads to closure problems that are identical to those encountered in the statistical theory of turbulence or in any theory of the statistical behavior of an inherently nonlinear system. Incomplete as it was, I regard this development as being highly significant and very promising. It has been pushed further by two of Epstein's students, Fleming (1971a, b) and Pitcher (1974), and by Leith, who has outlined a simple Monte-Carlo method for computing the evolution of the probability distribution.

To many of the meteorologists I have talked with over the past two or three years, some of these newfangled statistical-mechanical notions seem pretty esoteric. Let me point out, however, that probabilistic information is precisely what we need in filling out the "payoff table" and in determining the *optimum economic strategy in the face of any kind of uncertainty.* Perhaps I should also add that these are *not* strange notions, but are peculiarly American. The concepts of classical statistical mechanics were laid down in the 1880s by Josiah Willard Gibbs, a professor at Yale, regarded by his European colleagues as the greatest American scientist of his time.

8. The outlook

The popular view of history, I expect, is that of a chronicle of long-gone and rather dusty events. If that were true, my account is not a history. I regard the development of numerical weather prediction as a process of growth and evolution of ideas, the most important stage of which is the present. I therefore conclude my account with a few remarks about the current state of affairs.

There are now and always will be three major sources of error in numerical predictions. They are: 1) not totally removable errors in the specification of initial conditions; 2) defects of the physical model and its mathematical formulation; and 3) approximations of numerical representation. At the present time, the errors arising from these three different sources are roughly comparable in magnitude and are not, therefore, easily isolated.

With regard to the first category of error, I would like to draw your attention to the Global Atmospheric Research Program (GARP) and its subprogram FGGE (the First Global GARP Experiment), due to be launched in about 1978. This program was first proposed in 1961 (in the NAS-CAS report) and later in 1967 at the International GARP Planning Conference, with the principal objective of nailing down the errors of prediction due to incomplete initial data and poor parameterization of small-scale transport processes. It isn't history yet [1976], but it promises to reduce one of the major sources of error.

With reference to the physics of current numerical models, it is evident that the very large-scale components of the circulation patterns are handled badly—particularly wave numbers 2 and 3. These quasi-stationary modes are undoubtedly associated in some way with the variable surface properties of oceans and continents—reflectivity, heat capacity, heat conductivity, roughness, topography, and the like. These effects have been incorporated in a number of general circulation models, but not with notable success, possibly owing to the rather cavalier treatment of vertical transport.

Finally, it is far from clear that finite-difference methods are best or even well-suited to the requirements of numerical weather prediction. Except in instances when processes operate in a discontinuous fashion, the advantages evidently lie with spectral methods or quasi-Lagrangian finite-element representations. I have the impression that it is now time to back off to a prudent distance, shake off our earlier preconceptions and later investments, and start afresh.

In the phraseology of Herodotus, such were the customs and manner of numerical predictors of weather. Thus ends my story. Thank you.

Appendix A. Initial proposal to establish the Meteorology Project

THE INSTITUTE FOR ADVANCED STUDY
Princeton, N.J.

May 8, 1946

Office of Research and Inventions
Attention Lt. Commander D. F. Rex, Room 3446
Navy Department
Constitution Avenue
Washington 25, D.C.

1. The Institute for Advanced Study is a New Jersey corporation with its seat in Princeton, New Jersey. The Institute would be prepared to accept a contract to carry out a project with the objective and under the conditions as described in what follows.

Objective of the project.

2. The objective of the project is an investigation of the theory of dynamic meteorology in order to make it accessible to high speed, electronic, digital, automatic computing, of a type which is beginning to be available, and which is likely to be increasingly available in the future. It is also expected that these investigations will give indications as to what further observations are necessary—both of the laboratory type and of the field type—in order to make theoretical work, that is supported by such high speed computing, more fully effective.

The primarily relevant details of these ideas are as follows:

3. These are some typical problems of dynamic meteorology, which are also probably among the most critical ones from the point of view of the present status of fundamental theory:

(a) What is the mechanism and the flow pattern of the general, planetary circulation of the atmosphere? Can such a circulation be at all defined in any zonal-average sense, with zonal symmetry, i.e. disregarding (or rather averaging over) the actual irregular distribution of the continents?

(b) Can (a) be significantly treated in the troposphere alone, or is it necessary to draw at least the lower stratosphere, too, into the discussion?

(c) A stability analysis of the polar front, or of extended fronts in general?

(d) What is the mechanism and the flow pattern of the major cyclones? What can be said about their formation, their progress and their stability?

(e) What is the detailed, quantitative functioning of the release mechanism of local instabilities?

Quite apart from observational difficulties, to which we will return, e.g. in 11. below, these problems are well known to lead to analytical difficulties of a prohibitive character. In other words: Even if one were certain which of the numerous possible mathematical-physical formulations of these problems corresponds best to reality, the equations to which these formulations correspond are of a very difficult partial-differential or even integro-partial-differential type, further complicated by alternative distinctions defined by inequalities. It is utterly hopeless to try to resolve problems of this type by general mathematical analysis, and it has been recognized for a long time that numerical computational methods are the only ones which offer any prospect of really informative and specific results.

Numerical computation, on the other hand, has been very limited in its capabilities up to a generation ago. The improvements which have been introduced during the last generation—mechanical and electrical "desk" multipliers (these are rather generally arithmetical machines) and partially automatically sequenced electro-mechanical punch-card machines—changed this picture somewhat, but not very radically. Quite recent equipment—of a fully automatically sequenced and purely electrical (relay) type or even of an electronic type—has more extensive potentialities, but it has not yet had an opportunity to make its influence fully effective on the computing situation.

For these reasons the efforts in meteorological theory were in the main limited by what was practical in actual computing, i.e. with computing methods and with computing equipment of the periods preceding the present one. It is therefore essential to visualize what these limitations are.

4. The speed of most computing equipment is in the main determined by its multiplication speed. This statement is valid with definite qualifications, all of which are fulfilled in the cases that we now consider, but which should nevertheless be evaluated and kept in mind:

(a) This applies only to digital machines, which solve a problem by resolving it into discrete arithmetical operations. It does not apply to analogy machines, which may work on entirely different principles, and at any rate by continuous operation. However, the existing types of analogy machines are neither sufficiently precise nor sufficiently flexible to be adequate for problems of the type described in 3. above. The remarks which are valid for digital machines only do therefore apply in the present situation.

(b) Even for digital machines this statement must be taken with definite limitations and interpretations. It is, of course, not true that multiplications are the only time-consuming operations in a calculation. It is true, however, that other arithmetical operations are either a good deal faster (addition, subtraction) or a good deal less frequent (division, square rooting) than multiplication. On the other hand, non-arithmetical operations may consume considerable time: e.g. storing results, gaining access to and effecting the use of stored results—both these operations forming what is known as transfers—and also general logical control and discrimination operations. It is usually true that the time required for all arithmetical operations together will not exceed twice the pure multiplication time.

(c) For the non-arithmetical—transfer and logical—operations, this may be said: they may require a good deal more time than the arithmetical operations, if the problem is not primarily mathematical—that is, algebraical or analytical—but rather combinatorial or logical. (E.g. various forms of sorting.) For primarily mathematical problems, however, the non-arithmetical time should not exceed the multiplication time seriously, or else there is usually reason to believe that the components of the machine are not well matched, that its system is not properly planned, integrated and balanced. These remarks apply very definitely to the present situation.

(d) For the reasons (a)–(c) it seems to be appropriate to assert this: for the meteorological problems of the type indicated in 3. above, and assuming a well planned and balanced machine, the total computing time should not be essentially more than, say three times the pure multiplication time.

(e) In talking of multiplication time, it is necessary to specify what precision is intended. Indeed, if n-decimal precision is intended, multiplication means n by n digit multiplication. If various n's are compared, the work involved in a multiplication varies in proportion to n squared. In most machines this requirement is divided evenly between the equipment and the duration of the operation: Both are essentially proportional to n.

In meteorological work 5 to 7 decimals have usually been considered necessary. In most scientific work in the more involved parts of fluid dynamics 8 to 10 decimals were used. Most modern machines have 8 to 10 decimals, the Harvard machine allows either 11 or 23 decimal digit operation. We may therefore consider 10 decimals as a reasonable standard. The machines which will be built in the future may be binary instead of decimal, the one to be discussed in 5. below will certainly be binary. 10 decimal digits are equivalent to 33 binary digits. Since these future machines are likely to allow somewhat higher precisions, 35 to 40 binary digits seem a more reasonable standard.

With these qualifications in mind, the speeds of computing, past present and future, may be characterized as follows:

(A) Fast "desk" machines, the basic organs of what might now be called "human" or "hand" computing, have multiplication speeds of about 10 seconds (10 decimal digits).

(B) Semi-automatic equipment, as referred to in 3. above is not essentially faster. Thus the standard IBM multiplier has a multiplication speed of 7 seconds (8 decimal digits).

(C) The modern fully electrical (relay) machines, referred to in 3. above, have higher speeds: multiplication speeds of 3.5 to something like .7 seconds (varying between 6 and 11 decimal digits).

(D) The only existing fully automatic, electronic machine (ENIAC, Army Ordinance Department, University of Pennsylvania) is faster by several orders of magnitude: Multiplication speed of 3 milliseconds (10 decimal digits). However, this machine has a storage process which must be considered slow by such standards (punch-cards holding 1 to 8 numbers, available in linear order only, at a rate of one in .6 seconds); therefore it is not well balanced in the sense of (c), (d) above.

(E) Electronic machines which are now being planned or built should obtain multiplication speeds of 1 millisecond to .1 millisecond (varying between 30 and 40 binary digits). (Cf. e.g. 5. below.) They should be well balanced in the sense of (c), (d) above.

Thus accelerations over past computing methods by factors of the order of 10 and possibly much more—order of 1000—are already possible, and accelerations by factors of 10,000 to 100,000 will become possible in a few years, as the projects mentioned in (E) above are carried out.

5. Specifically, concerning the projects mentioned in 4. (E), this might be said:

Several such projects are now in various stages of execution, at the University of Pennsylvania, at M.I.T., at the Institute for Advanced Study, and possibly also at other places. In what follows we will refer to the Institute for Advanced Study project only, since this could be directly integrated with this proposal.

We propose to build, with help from other agencies, a fully automatic, electronic, digital machine with the following characteristics:

(a) Binary operation, with decimal-binary and binary-decimal conversions at the ultimate inputs and outputs. However, for purely scientific problems the conversions are of subordinate importance.

(b) Precision between 36 and 40 binary digits.

(c) Vacuum tube operation at about a megacycle rate.

(d) Multiplication time of .1 to .2 milliseconds. Division time of .2 to .3 milliseconds. Addition and subtraction times of .01 to .02 milliseconds.

(e) An electronic memory of about 4,000 numbers (36 to 40 binary digits each) with transfer speeds of .01 to .02 milliseconds.

(f) Fully automatic, electronic logical and mathematical control, by coded instructions as in (e).

(g) Ultimate inputs and outputs on several tapes—probably magnetic tapes of the sound-recording type.

(h) Alternative outputs on oscilloscope screens, allowing direct graphing of the results—these graphs can be viewed or recorded by photographing.

(i) Automatic checking, probably by running two identical machines in parallel, continuously compared at many points by electronic coincidence circuits. This allows an automatic identification of most malfunctions, and recognition of the part of the machine in which they occurred. Further automatic checking by arithmetical means.

(j) Size of the machine (assuming doubling according to (i)): Less than 9,000 vacuum tubes, dissipating less than 20 kilowatts.

(k) Sample logical control-code, which allows the "setting up" of problems with not essentially more work than for a human computer group.

We anticipate that a preliminary model may work in about two years and a final model in three years. We hope to carry out this program with considerable help from the Princeton Laboratories of R.C.A.

We expect that such a machine will change the conditions, methods and applications of computing fundamentally. It should compute 50,000 to 100,000 times faster than is possible at present (cf. 4. (A), (B) above and (d) here); it should therefore change the entire inner economy of computing. It will therefore make the developing of entirely new methods of approximation mathematics highly indicated and profitable. It will accordingly cause a complete change in our estimation as to which problems can be solved by computation.

For these reasons we propose to use this machine solely for exploratory and research work: To develop new approximation and computing methods, to test them, to explore new fields in applied mathematics and in mathematical physics, which become now accessible to computation.

Among the fields which we intend to study in this manner, the one of dynamic meteorology is among the most important. Another field, which should have the highest priority, is that of turbulent fluid motion—and this is also essential for a more fundamental approach to dynamic meteorology.

6. In carrying out this program considerable preparatory studies will be required, for which the 2 to 3 years needed to build the machine offer a welcome and natural opportunity—and one of not at all too long duration. The studies on new approximation and computing methods, referred to in 5. above, will be carried out during this period, in the sense indicated. Studies of meteorological theory are necessary in the same sense.

A careful analysis of the present status of meteorological theory, carried out in particular by Dr. John von Neumann of the Institute for Advanced Study and Dr. C. G. A. Rossby of the University of Chicago, indicates that even if computing equipment of the type indicated in 5. above were immediately available, we would not be able to use it at once. This is even valid for some more limited, but nevertheless very interesting and important, problems, which might be solved by ENIAC. Indeed, the possibilities that are opened up by these devices are so radically new and unexpected, that the theory is entirely unprepared for them. There was no practical motivation in the past to work out those parts of meteorological theory on a mathematical and analytical level, which, in order to become really effective, would require calculational methods that are 1,000 to 100,000 times faster than what seemed possible at the time! A complete reassessment or revaluation of the theory is therefore an absolute prerequisite.

7. It is therefore our proposal to carry out this reassessment. This should be done by a group of about 5 or 6 first-class younger meteorologists, who should work in the closest possible association with the group which is planning and building the machine referred to in 5. and 6. above. For this reason we consider it essential that the group should be located in Princeton, and that it should work under the general direction of Dr. John von Neumann, who is directing all phases of the computer program of the Institute for Advanced Study. It is furthermore quite essential to provide for ample consulting opportunities, in order to be able to secure the interest and the cooperation of the leading meteorologists of the country, as well as that of several physicists and engineers whose help is essential. The last mentioned circumstance deserves special emphasis: We anticipate that new physical measurements will gradually turn out to be essential for the adequate integration of our program, and that these will in their laboratory phase require the help and advice of various physicists, and in their field phase new instrumentations, and hence the help and advice of meteorologists with a broad administrative experience and of engineers.

Time Schedule and Working Schedule of the project.

8. We anticipate that the project as outlined above will have to extend over several years. Indeed, the phase discussed in 2. and 6., 7. above is only a preliminary phase, while the principally fruitful phase begins only where the former ends: When a machine as described in 5. above becomes available, the exploitation of the theoretical methods developed in the preliminary phase can begin. At this moment the main need is to discuss the preliminary phase. It seems reasonable to time it so as to have it coextensional with the period required for the building of the machine—i.e. to last 2 to 3 years. This duration, however, also seems to be reasonable and appropriate per se.

It is again desirable to elaborate this in somewhat more detail.

9. Assuming that an adequate group of meteorologists will be assembled in Princeton about the fall of 1946, it should take about 6 months to carry out a preliminary analysis of the main representative problems of dynamic meteorological theory—e.g. of the problems 3., (a)–(e), or of some equivalent list. During this period these main physical and observational-meteorological uncertainties should be assessed, alternative mathematical formulations of the resulting possibilities should be developed, and the analytical structure of the theories which are thus obtained should be investigated. At this point an outside board of meteorological and physical consultants should be brought in, to evaluate the respective merits of the alternatives which are evolved, to assess the relative difficulties of the experimental and observational work which may be required in each case, and to advise the project as to the directions which hold most promise. This should require a few months' work. After that it will be possible to carry out a concentration of the project towards two or three definite problems and specific problem formulations—presumably certain alternatives derived from 3. (a)–(e) above. About another 6 months' work by the Princeton group may then be required to work out the analytical details of these specific problems. This would take the project well to the end of 1947.

In the year which follows definite approximation and computing techniques for the selected problems should be worked out. During the second half of 1948 the first model of the machine should become available. This will make the testing of the techniques in question possible, at a gradually increasing rate as the first model and the methods to use it become familiar. With the end of this period, in the course of 1949, the final model of the machine should be completed and the project, as outlined, would have also achieved its objective.

10. The possibilities opened up by this work need only be referred to in a very general manner. Entirely new methods of weather prediction by calculation will have been made practical. It is not difficult to estimate that with the speeds indicated in 5. above, a completely calculated prediction for the entire northern hemisphere should take about 2 hours per day of prediction. A new, rational basis will have been secured for the planning of physical measurements and of field observations in meteorology, since complete mathematical theories and the methods to test them by comparing experience with the rigorously calculated consequences of these theories will have been obtained. And finally the first step towards influencing the weather by rational, human intervention will have been made—since the effects of any hypothetical intervention will have become calculable.

11. It is much more difficult to predict what new measurements and observations will turn out to be desirable. One thing, however, seems to be very probable: that more radiation measurements, more information about the radiative properties of such components of the atmosphere as water vapor and carbon dioxide will be necessary. It seems also probable that more information about the air flows in the southern hemisphere will be desirable: Because of the relative paucity of large continental masses, the southern hemisphere would seem to be a better testing ground at least for theories of the general, planetary circulation (cf. 3. (a) above) than the northern hemisphere.

Personnel.

12. As mentioned in 7. above, the nucleus of the proposed project would be a group of 5 or 6 first class younger meteorologists. Every effort should be made to secure the services of Dr. H. Wexler from the U.S. Weather Bureau to lead and supervise this group. There seems reason to hope that this will indeed be possible. Other very desirable candidates for inclusion in this group are Dr. H. Pekeris, now at Columbia University, Dr. R. B. Montgomery, now at the Woods Hole Oceanographic Institution, and Captain G. Hunt of the Army Air Forces, now at Princeton. Further selections should be made as the group crystallizes.

The level of this group should be definitely very high and academic. The general coordination of the various phases of this project with each other and with the Institute's computer project should rest with Dr. John von Neumann.

13. Another essential phase of the project would consist in securing the services of a prominent group of consultants and advisers, as mentioned in 7. and in 9. above. This group should include such meteorologists as Dr. C. G. A. Rossby of the University of Chicago, Dr. H. U. Sverdrup of the Scripps Oceanographic Institution at La Jolla, Dr. J. Bjerknes of the University of California at Los Angeles; physi-

cists with their main interest in questions of radiation, molecular physics and astrophysics, such as E. Teller of the University of Chicago, and S. Chandrasekhar of the Yerkes Observatory; one or more aerodynamicists, including Dr. Th. von Karman of the California Institute of Technology; and various experts in other fields, such as W. Weaver of TDRC and the Rockefeller Foundation, and V. K. Zworykin of the Princeton Laboratories of RCA. There is reason to believe that a majority of the persons mentioned would accept. The total group whose advice might be sollicited at various times may consist of 8 to 10 persons.

14. The clerical help and the physical equipment required by this group is not likely to be considerable. About 2 clerk-computers may suffice for the first purpose, and nothing beyond ordinary office facilities is needed for the second. The services of the Institute's computer project will be automatically available. Experimental and observational help should be secured through personal connections, and through the advisory group mentioned in 13. above.

Equipment and Facilities available and needed.

15. This subject is partly covered in 14. above. Beyond that, this may be said: The Institute for Advanced Study can provide some office space in its present building, Fuld Hall. This, however, will not be adequate, and additional space will have to be secured by rental in Princeton. This is satisfactorily feasible.

The Institute will provide the services of certain members of its staff: Dr. John von Neumann for the overall direction and coordination of the project, the members of its computer project for cooperation and consultation when required.

The Institute will take care of the general administration of the project, of securing personnel for the staff and the consulting and advisory body.

Yearly Budget.

16. The yearly budget requirements of the project are as follows:

Staff salaries:	
6 members, averaging \$5,500	\$33,000
Overhead:	
40% of the staff salaries. This includes rental of office space.	13,200
Consulting:	
10 consultants, averaging 15 days per year, at \$25 per day.	3,750
Travel:	
For 10 consultants, averaging 2 trips per year at \$125 per trip, plus \$1,500 per year for staff travel.	4,000
Clerical, computing:	
2 clerk-computers, averaging \$2,500	5,000
Miscellaneous expenses	2,000
	\$60,950.

Thus a total yearly budget of about \$61,000 would seem to be necessary.

Frank Aydelotts
Director,
Institute for Advanced Study

Appendix B. Letter from Jule Charney to Philip D. Thompson

February 12, 1947

Lieutenant Philip D. Thompson
Institute for Advanced Study .
Princeton, New Jersey

Dear Phil,

I thoroughly agree that the questions you propound lie at the very heart of the whole problem, not only of numerical forecasting but of the solution of the equations of motion by any means whatever, and I am very pleased to hear that you are now grappling with them. As you know, I have long been aware of these questions and have from time to time sounded off at some length about them. I am therefore not only willing but anxious to discuss them with you.

Let us begin with your last question, "Why don't the large scale atmospheric disturbances move with the speed of sound?". One answer was given by a scientific pundit writing in the Readers Digest. It is obvious he says, that man exists only because of a very improbable concatenation of events. If the solar radiation were twice as great the oceans would dry up and man would simply find existence too uncomfortable. Or if the earth rotated at a much reduced speed he would freeze in winter and roast in sumer, etc., etc. Donc, Dieu existe. One could add in the same vein that if cyclones traveled with the speed of sound man would be whisked right off the earth, which is manifestly impossible according to our learned scientist. In case these anthropomorphic arguments leave you cold, and you do not believe in the Bible or even in the Readers Digest, I propose the following argument.

In the terminology which you graciously ascribe to me we might say that the atmosphere is a musical instrument on which one can play many tunes. High notes are sound waves, low notes are long inertial waves, and nature is a musician more of the Beethoven than of the Chopin type. He much prefers the low notes and only occasionally plays arpeggios in the treble and then only with a light hand. The oceans and the continents are the elephants in Saint-Saens' animal suite, marching in a slow cumbrous rhythm, one step every day or so. Of course, there are overtones; sound waves, billow clouds (gravity waves), inertial oscillations, etc., but these are unimportant and are heard only at N.Y.U. and M.I.T.

To become literal we might say—the energy that goes into an atmospheric disturbance depends on the initial mode of excitation. A forced perturbation of long period produces a disturbance of long period. A perturbation in which energy is released so fast that the air does not have a chance to get out of the way could produce sound waves of very large amplitude to consume this energy. But with the exception of volcanic eruptions and atom bombs such agencies are never found. Even the atom bomb converts only a small part of its energy into waves of concussion.

Let us illustrate by considering the motion of waves in a constant barotropic zonal current. The equations of motion are

$$\frac{\partial u}{\partial t} + u\frac{\partial u}{\partial x} + v\frac{\partial u}{\partial y} + w\frac{\partial u}{\partial z} = 2\Omega v \sin\theta - \frac{1}{\rho}\frac{\partial p}{\partial x} - 2\Omega w \cos\theta$$

$$\frac{\partial v}{\partial t} + u\frac{\partial v}{\partial x} + v\frac{\partial v}{\partial y} + w\frac{\partial v}{\partial z} = -2\Omega v \sin\theta - \frac{1}{\rho}\frac{\partial p}{\partial y}$$

$$\frac{\partial w}{\partial t} + u\frac{\partial w}{\partial x} + v\frac{\partial w}{\partial y} + w\frac{\partial w}{\partial z} = 2\Omega u \cos\theta - g - \frac{1}{\rho}\frac{\partial p}{\partial z}$$

and for waves of small amplitude and infinite lateral extent, propagated in the x-direction, they become

$$\frac{\partial u}{\partial t} + U\frac{\partial u}{\partial x} = fv - \frac{\partial \pi}{\partial x}$$

$$\frac{\partial v}{\partial t} + U\frac{\partial v}{\partial x} = -fu - \frac{\partial \pi}{\partial y}$$

$$\frac{\partial \pi}{\partial t} + U\frac{\partial \pi}{\partial x} - fUv = -\phi\frac{\partial u}{\partial x}$$

where U is the zonal speed, $f = 2\Omega\sin\theta$, u, v, π respectively the velocity components and barotropic pressure function ($\int\delta p/\rho$) of the disturbance, ϕ is the undisturbed value of $\partial p/\partial\rho$ and as usual we neglect the vertical components of acceleration and coriolis force as well as the horizontal component of coriolis force involving w. These equations were solved by Rossby for a disturbance of the form

$$u = A\, e^{2\pi i/L(x-ct)}$$

$$v = B\, e^{2\pi i/L(x-ct)}$$

The solution gives the value of the velocity c:

$$U - c - \frac{\beta L^2}{4\pi^2} = \frac{L^2 f^2}{4\pi^2}\frac{c}{\phi - (U-c)^2}$$

where β is equal to df/dy and is assumed to be constant. If we had assumed that the atmosphere were incompressible and homogeneous ϕ would be the dynamic height.

Now here is the important point. The last equation has three roots. One is very nearly equal to the solution of this equation without the $(U - c)^2$ term in the denominator of the right hand side. The other two roots are nearly equal to those obtained by setting the denominator on the right equal to zero. This means that there are three modes of vibration, and it is easy to see that the first root corresponds to long waves and the remaining two to gravitational waves traveling in opposite directions. (Sound waves are eliminated by the assumption of no vertical acceleration (quasi-horizontal motion)). The general solution for a given initial disturbance would embrace both long inertial and gravitational waves. But if, for example, the initial disturbance were harmonic and had a period equal to that of the long waves no energy at all would go into the gravitational wave components. In general, of course, every disturbance, if broken down into harmonic components by Fourier analysis, would exhibit components with all periods, and therefore some of the energy would produce gravitational oscillations which, you will observe, have velocities of the same order of magnitude as that of sound. In an isothermal barotropic atmosphere, for example, two solutions of the above velocity equation are given approximately by

$$(U - c)^2 = \frac{\partial p}{\partial \rho} = RT$$

whereas for sound we have

$$(U - c)^2 = \frac{c_p}{c_v} RT \sim RT$$

But since most of the energy of the initial disturbance goes into long period components very little of the energy will appear in the gravitational wave form.

This leads us to the next problem, namely, how to filter out the noise. Pardon me, but let us again think metaphorically. The atmosphere is a transmitter. The computing machine is the receiver. The receiver is a very good one indeed, for it produces no appreciable noise itself, i.e. all noise comes from the input. (I am supposing that you can compute to any desired order of accuracy.) Now there are two ways to eliminate noise in the output. The first is to make sure that the input is free from objectional noises, or the second is to employ a filtering system in the receiver. Translating, the first method implies that the unwanted harmonics shall be eliminated from the raw data by some type of harmonic analysis; the second that you transform the equations of motion and make approximations in such a way that the bad harmonics are automatically eliminated. Let us consider the second method and illustrate by means of the foregoing example of wave motion. If, in the solution of the equations of motion, whenever a term containing the factor $1 - (U - c)^2/\phi$ appears, we replace if by 1, then the resulting equation for c would be*

$$U - c - \frac{\beta L^2}{4\pi^2} = \frac{L^2 f^2}{4\pi^2} \frac{c}{\phi}$$

instead of

$$U - c - \frac{\beta L^2}{4\pi^2} = \frac{L^2 f^2}{4\pi^2} \frac{c}{\phi - (U - c)^2}$$

Thus the equation would have only one root and that one would correspond to the long waves. But this does not tell us what to do with the equations of motion themselves. If you work backward you find that the approximation is equivalent to ignoring the x-component of the acceleration i.e., to assuming that the north-south perturbation velocity is geostrophic. Now don't jump to the conclusion that the latter approximation may always be made. We can do it here because we have assumed no variation in the streamline pattern in the north-south direction. I do not know what will happen if you consider waves of finite lateral extent as Haurwitz does. Here the problem becomes more complicated since Haurwitz assumes that $\nabla_H \cdot \mathbf{V} = 0$, which, of course, is tenable only for barotropic motion. In my paper on baroclinic waves I find that one has to make a number of approximations of the type $(U - c)^2/\phi \ll 1$ to arrive at a tractable system of equations, from which gravitational waves, Helmholtzian waves, sound waves and inertial oscillations are eliminated. But I also consider only waves of infinite lateral extent. I still don't know what types of approximation have to be made in more general situations.

On the other hand, don't think that compressibility is what botches up the works. Even if you were to replace the actual atmosphere by a non-homogeneous incompressible atmosphere with the same stability you would still have gravitational waves. However, if you accept the consequences of the above reasoning you will perhaps share my conviction that there *is* a general type of approximation or transformation or what have you that *will* eliminate the noise and the problem is now to find it!

Enough of this. Let us change the subject. Do you remember my suggestion that you study simple types of finite amplitude motions as a preliminary step to attacking the general forecasting problem? In particular, the Rossby wave model? Well, for various reasons I do not think that that particular study will lead to anything very interesting. If a barotropic system is stable, then the horizontal divergence is negligible and the first order approximation is very nearly the exact solution. On the other hand, if the motion is unstable and developes into vortices, the successive approximations will be significant. I have begun an attack on several such problems and the results look promising.

I would like to discuss some of these things with you personally since the time scale of interchange of ideas by correspondence is just too great. If I had the dough I would hie myself to Princeton and have it out with you, but naturally I haven't. Our Quarter ends on the 21st of March and I am catching the boat for Norway on the 22nd so I will not even be able to stop over in Princeton. With all that Navy money lying around why don't you invite me to come to Princeton for a couple of days? In any case, write and let me hear your reactions. Also give my best regards to Panofsky.

Sincerely yours,

Jule Charney

*This approximation is justified since ϕ is of the order of the square of the speed of sound.

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announcements[1]

Antarctic ocean-bottom cores available for study

Cores of ocean bottom sediments and other geological samples collected near and in Antarctica are now available to qualified scientists for study. The specimens, which consist principally of 12 900 m of piston, trigger, and phleger cores from hundreds of locations in the southern oceans; 4200 kg of grabbed, trawled, and dredged rock specimens from 600 ship stations; and 1150 m of drilled cores from the ice-free valleys of southern Victoria Land, were obtained between 1962 and the present, under National Science Foundation (NSF)-sponsored projects. Interested scientists need not have an NSF

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[1]Notice of registration deadlines for meetings, workshops, and seminars, deadlines for submittal of abstracts or papers to be presented at meetings, and deadlines for grants, proposals, awards, nominations, and fellowships must be received at least three months before deadline dates.—*News Ed.*