long range weather forecasting— history, current status and outlook¹

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Abstract

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Harry Wexler was a close student of developing techniques in the field of long range forecasting, and a contributor to them, realizing that improvement of long range forecasts represents one of the most important goals of meteorology. His scientifically-oriented administrative ability played a large part in enlarging the role of this subject in the United States. Indeed, one of the principal reasons he pioneered in inaugurating the World Weather Watch program was to lay a firmer foundation for long-range forecasting. I was one of many who gained benefit and stimulation from a long friendship with him, and my lecture will recall some of the instances.

First, I want to describe the nature of the long range prediction problem as seen through the eyes of a pragmatist, and to present a balanced picture of what is now possible and what may become possible in the next decade or so. One might hope that this 10-year forecast of "weather predictability" will turn out better than a 10-year forecast of the weather itself!

Next, I will speak on the history of long range forecasting over the past century, as traced through the work of inventors of synoptic, statistical and physical approaches to the problem. In spite of decades of frustration imposed by lack of adequate data, ignorance of large-scale physical processes, and the absence of intensive and large-scale effort, meteorologists working on long range problems have made encouraging progress.

1. Personal recollections of Harry Wexler

FIG. 1.1. Picture taken from Dr. Wexler's high school Record Book, 1928.

Mr. President, Ladies and Gentlemen:

It is a special pleasure and honor for me to present tonight the fourth Wexler Memorial lecture—special because of my intimate friendship with Harry Wexler from boyhood on. We were close friends—and at times competitors—throughout grammar, junior high and high school. We lived a few blocks from each other in Fall River, Mass., were members of the same Boy Scout troop, dated together, married sisters, and carried on the major part of our careers in the U. S. Weather Bureau. Because of this close association over many years and my high esteem of him as a person and scientist, I feel that I should preface this lecture with recollections of some of the small incidents in his early life which offered clues to the potential, the character, and the ability of the man.

Earlier lecturers have spoken about his mature years—his scientific achievements, his wide-ranging interests, his friendly and jovial spirit, and his role as ambassador plenipotentiary on the international scientific scene. These accomplishments and his rise to eminence came as no surprise to me; I was in the habit of making long-range predictions long before I got into meteorology. What fascinated me about Harry from grammar school on was his industry and insatiable scientific curiosity. In junior high school when most of us were girl-watching, he could usually be found after school hours in the laboratories, conducting science experiments in solitary peace and quiet—or perhaps organizing a science club. Afterward he would return home to play "mathematical recreations" with his brothers and frequently with me.

During high school (Fig. 1.1) he kept up this steady pursuit of science, interrupting it only to run—efficiently—a large paper route. His scholasticism and drive was engendered partly by his older brother Charles who went to Harvard and later became a well known mathematician. At high school he and I competed in many courses; in

¹ Wexler Memorial Lecture, presented at the AMS 49th Annual Meeting, 29 January 1968.

FIG. 2.1. Photo taken in front of the ENIAC, Aberdeen Proving Ground, 4 April 1950, on occasion of the first numerical weather computations carried out with aid of a high-speed automatic computer. (Pictured left to right: H. Wexler, J. von Neumann, M. H. Frankel, J. Namias, J. C. Freeman, R. Fjørtoft, F. W. Reichelderfer and J. G. Charney.)

physics we were selected as lab partners. For reasons still obscure to me, Harry would right off rush to the apparatus, set it up, and hope to conduct the experiment—hardly reading the instructions which I perused while he acted. Despite this apparent handicap, his impetuous method usually succeeded in getting the general organization of the apparatus right—only adjustments of detail were necessary to get results. In a sense, this characteristic of plunging into things right away, of capturing the essence of a problem and leaving details to others, persisted throughout his career. When a problem was solved in principle it was for him a “dead problem” and he would be off to another challenge. I believe that it was this remarkable talent for seeing forests and not trees that contributed to his uncanny ability to predict and push forward with the implementation of plans for major scientific advances involving meteorological satellites, numerical weather prediction (Fig. 2.1), and the study of planetary atmospheres. American meteorology was indeed fortunate to have a man of such foresight in the front ranks during the precarious few decades subsequent to the Great Depression.

To give an idea of Harry’s objectivity and willingness to bend over backward in search of truth I recall a multiple choice test which we had in high school. The statement “The moon is made of green cheese” was to be classed as (1) probable (2) ridiculous (3) possible (4) improbable, or (5) incorrect. He checked off (3)—arguing that no one really had first hand knowledge. (I’m not sure what he’d put down now in view of the lunar probes.)

It was in his senior year at Harvard that I got him interested in meteorology. We were sitting on the bank of the Charles River when I suggested that he leave his chosen field of pure mathematics (which during the depression was not exactly a field with a guaranteed future) and consider entering meteorology. After I had pointed out a few of its challenging problems and praised to the skies our head professor, Rossby, he became interested—enough so that he began to become curious right then and there about the heat and moisture exchange between beach, air and the Charles River. An hour’s visit with Professor Rossby set his course once and for all, and he enrolled in the M.I.T. graduate school that fall.

Shortly after sampling the courses at M.I.T. Harry decided that he wanted to become a “triple-threat”—dynamicist, synoptician, and instrumentalist. Not surprisingly, he attained his “triple threat” objective later on—but the third area of excellence was not instruments but scientific administration.

His introduction to synoptic meteorology, particularly in the laboratory, was at first painful. As a mathematician he questioned the use of pencils to construct isobars, since the thickness of the lines obviously conflicted with the exact definition of an isobar as a line of equal pressure. His draftsmanship left much to be desired, and at first glance his analyses were, in the words of an exasperated professor, “vile.” But he doggedly kept at synoptics until he eventually earned the respect of even hypercritical forecasters at the Weather Bureau. He even engaged in weather forecasting for a period—I think largely to prove that a theoretician could make a go of it. Yet his main interest lay in quanti-

tative researches. But his hard-acquired knowledge of synoptic meteorology and the "real" atmosphere later made possible outstanding researches in a variety of fields ranging from hurricane structure to air pollution, anticyclogeneses, and climatic change.

His joviality and sense of humor are legendary. On the occasion of Professor Bergeron's first visit to the U. S. Weather Bureau, Wexler was showing him through the Analysis Center, when Bergeron, in the midst of a milling crowd of workers asked "Why are they rushing about so?" Wexler replied "Don't you know? They're racing against the weather—but they always lose."

Although I could devote this entire lecture to pleasant recollections of Harry Wexler as a man and scientist, it would be more in keeping with these lectures and probably Wexler's wishes that I survey a particular field of my scientific competence—especially since there will be many more memorial lectures and thus other opportunities to recall his traits and accomplishments.

Thus I have chosen to speak on long range weather forecasting—a branch of our science which is at long last achieving dignity among meteorologists, and is now attracting the best brains in our field. Throughout his career Wexler did all he could to encourage its pursuit and defend its workers against hatchet-men. Besides, he contributed numerous ideas—both verbally at conferences and map discussions and in his scholarly papers on the general circulation, anticyclones, long waves, and climatic change.

Below are listed those papers of his which had the closest bearing on extended and long range forecasting.

My lecture tonight will treat the history, current status and future of long range forecasting. Perhaps I should add the cliché "The opinions expressed here are entirely those of the speaker and not necessarily those of ESSA."

*Publications of
Harry Wexler especially
germane to extended
and long-range
forecasting*

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2. Predictability and detailed extended and long range forecasting

Meteorologists have always dreamed of predicting weather in detail in both space and time. Some of them have felt that the problem was solved in reality or in principle. Today some feel that detailed long-range prediction is just within grasp. In past years one could ascribe such optimism to a high enthusiasm-to-information ratio, usually accompanied by reluctance to employ or accept statistical testing procedures. Today, the number of eminent meteorologists who see detailed weather prediction (out to two or three weeks at any rate) as a realizable achievement over the next several years has substantially increased; indeed the optimism has become so universal that one finds references to the imminent era of detailed predictions in most of the world's technical publications of meteorology and even in the lay press. Naturally, the opinions of the world's top meteorologists are varied, as I soon found out at the meetings of the International Union of Geodesy and Geophysics last fall in Switzerland. But among the enthusiasts are the most highly respected in our field, whose opinions are generally solidly based. What has happened to bring about this wave of optimism? What is its foundation?

Let us take a look at the scientific and technological advances of the recent past, for these may be the building blocks of a new edifice. Obviously critically important for long-range forecasting, advances have been made in computer technology and in observational systems. More important, meteorology has acquired a generation of top-flight dynamicists able to employ these technological advances in the quest for objective weather prediction. The experiments of this group have excited the entire world community of meteorologists by revolutionizing weather forecasting as well as research methodology. Short-range numerical prediction, the first problem attacked, succeeded from the very start by objectivizing the computation of the redistribution of vorticity, and thereby captured the main component of the up-to-date forecaster's thinking of the decade of the 1940's. I do not mean to downgrade the importance of this achievement when I say it was a "quick victory," for it was a hard battle, fought not at a university meteorological department, nor in a government laboratory, but in the "ivory tower" atmosphere of the Institute for Advanced Study. It was, after all, accomplished in a period less than one tenth as long as the entire life span of scientifically-based weather forecasting. (I might appropriately insert that Harry Wexler played no small role in this victory through his determined effort to keep going the numerical prediction project,

though the principal credit goes to the scientists like von Neumann, Charney, Fjørtoft, Eliassen and others.) Although many improvements in numerical methods followed, leading to reasonably successful three-day forecasts, no new breakthroughs comparable to the development of the barotropic model occurred until Phillips began his numerical simulations of the general circulation. These in turn have led to remarkable experiments by many investigators whose names are already etched in our hall of fame—Smagorinsky, Manabe, Mintz, Arakawa and Leith. These pioneers, following Phillips' lead, set out to duplicate climate in the broad sense of the complex workings of the general circulation. They have succeeded far beyond expectations of many meteorologists and climatologists. After such a spectacular second "quick victory" what remains? *Only* the ability to predict the fluctuations around the climatological mean—daily, weekly, monthly, seasonal, or beyond. I underscore *only* because of what follows.

It was von Neumann (1955) who, in outlining the order of the stages to be achieved in numerical weather prediction, wrote: (1) short-term fluctuations; (2) climate; (3) long-term fluctuations. He saw clearly the imminence of reasonably prompt success in (1) and (2)—a remarkable prediction in itself. But it may be forgotten that he classed long-range forecasting as a problem of much higher order of difficulty than either (1) or (2). It comes then as quite a surprise that our top-echelon scientists foresee accurate numerical predictions, not only of fluctuations around monthly or seasonal means, but even of detailed weather out to two or three weeks.

The rationale for this optimism has been given in a report by a panel of experts (Panel on International Meteorological Cooperation, 1966). They describe a predictability test that was performed on three different iterative models of the general circulation. Each model was run twice for at least 30 days beyond an arbitrary interruption time, once undisturbed and once with a perturbation of the temperature field added. When the fields developing from the unperturbed and perturbed initial conditions had diverged enough to differ by as much as two randomly chosen fields, the limit of detailed predictability (about two weeks) was said to have been reached. However, as Robinson (1967) later remarked: "This experiment may tell a lot about the models but not necessarily anything about the real atmosphere." Robinson also stressed a possible lack of a gap in the spectrum of atmospheric turbulence—a lack which could be fatal for long-range detailed prediction. These comments, delivered in a Presidential address before the Royal Meteorological Society, make Robinson the foremost dissenter on the numerical prediction scene. His detractors claim he will soon have to "eat his words."

Except for talk over coffee cups, no one experienced in long-range forecasting has spoken out publicly or written on this matter. This is not surprising because of the complexities of numerical modelling (often baffling to the practitioner), because of the above-described quick victories with shorter period prediction, and because of the unavailability of results on long-range forecasting with real atmospheric input. In spite of these three good reasons to keep quiet, I shall try to sum up my position in the hope that it may assist in maintaining a balanced view—the cliché for it is, I believe, "cautious optimism."

As a pragmatist with over 30 years experience in extended and long-range forecasting, I am primarily influenced by this humbling experience and by the harsh yardstick of verification. I shall mention two recent tests:

Sets of daily charts by two extended-range prediction models have been compared with actuality by our Extended Forecast Division staff. The first comprises a series of 14 forecasts utilizing Shuman's 6-layer primitive equation model (Shuman and Hovermale, 1967). Departures from normal of sea-level pressure, predicted and observed, were extracted for a grid covering North America, the values standardized and correlation coefficients computed between forecast and observed out to six days. The results were compared to persistence and to two partially subjective predictions by experienced forecasters, one drawing only on standard baroclinic and barotropic prognostic charts, the other on the Shuman model prognostic charts in addition. Results are shown in Fig. 1.2. We can draw at least two conclusions from this figure: (1) Extension of numerical predictions to 6 days can assist in improving present methods, particularly when modified by experienced forecasters. (2) This model will have to be greatly improved if skill out to two or three weeks is to become possible.

The second kind of chart tested was the daily prediction of precipitation, in two long runs, computed by the most sophisticated numerical model now in operation, that of Smagorinsky (Miyakoda, Smagorinsky, Strickler and Hembree, 1967). The even days from one of these runs, along with the observed charts, are shown in Fig. 2.2. At first

FIG. 1.2. Statistical evaluation of 14 National Meteorological Center 6-layer primitive-equation (PE) model forecasts, and corresponding subjective forecasts, of sea-level pressure over North America made from December 1966 to June 1967. Subjective I was made without, and subjective II with, the aid of the PE predictions. (See text for further explanation.)

glance, the success of these rainfall predictions seems overwhelming. They frequently capture the incidence of rains in the Pacific Northwest, the Gulf Coast, and the Southeast, and also correctly indicate little or no precipitation in the heartland of the United States and in the far Southwest. But these are also the characteristics of the climatological frequency of precipitation, particularly when forecast amounts under 0.10 inch are disregarded as they are for this experiment (the model is not capable of distinguishing in this range). It is, of course, most encouraging to see that the model is "in tune" with climatology. This is a necessary but not sufficient condition for skill, for to possess skill a forecast must predict precipitation better than climatological probability. In other words, one must ask questions like these: When rain is predicted in the dry areas of the far Southwest, how often is it observed? Again, when no rain is predicted for the Pacific Northwest, how accurate are the predictions? When rain is observed over the Great Plains, how correctly is it forecast? Would a random or other no-skill forecast have done as well? An analysis of this sort must be completed before one can lay claim to detailed and skillful predictions.

In an attempt to find satisfactory answers to these vexing questions, my colleague Dr. Gilman and I have analyzed the results of these two experimental predictions, one for a 14-day period in January 1964, and the other (just shown) for a 12-day period in January 1966. In our verification only the occurrence of measurable precipitation was taken into account, not actual amounts. In the predictions, amounts less than 0.10 inch were taken as forecasts of no precipitation, since the model is known to be incapable of handling precipitation in the 0 to 0.10 inch range. A grid of 100 stations, shown in Fig. 2.2 (observed, day 2), was used for the verification.

In order to calculate an expected chance figure of success, the predicted precipitation was assumed to be scattered around the country as a whole, at random but allowing for the normal place-to-place variation of climatological probability of occurrence in January. Thus, if 30% of the country was forecast to have precipitation, much more of it would be expected to fall over the Pacific Northwest and over the Southeast than over the normally dry Southwest or western Great Plains. The number of hits to be expected from the "scrambled" model forecast, together with the actual number of hits, were used to compute a skill score for each day. A similar verification was carried out on a control

EXTENDED FORECASTS OF DAILY PRECIPITATION OCCURRENCE

BY GFDL MODEL
JANUARY 5-16, 1966

FIG. 2.2. Extended forecasts of daily precipitation occurrence over the United States by the Geophysical Fluid Dynamics Laboratory 9-level primitive-equation model for January 5 to 16, 1966. Shaded areas are predicted precipitation (left) greater or equal to 0.10 inch, and observed precipitation (right) greater or equal to 0.01 inch for even days of the period. Verification grid shown on chart for observed, day 2.

EXTENDED FORECASTS OF DAILY PRECIPITATION OCCURRENCE

BY GFDL MODEL
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forecast of persistence of the field of 24-hour precipitation observed just before starting time. The results are shown in Fig. 3.2, in which a perfect forecast scores 100, and a forecast that might as well have been randomly scrambled scores 0.

Since the 0.10 inch cut-off was empirically determined by Smagorinsky from the results of the 1964 case, it is probably unwise to count on this case as representative. It is also difficult to understand the generally negative scores for persistence after the fifth day, although this probably implies a distinct change in weather regime. Except for the second day, the predictions appear to have positive skill all the way out to the 14th day.

Perhaps more representative is the case in January 1966, where no adjustments after the fact were made. Here we see definite and substantial skill over persistence out to three

FIG. 3.2. Evaluation of extended forecasts of daily precipitation occurrence by the Geophysical Fluid Dynamics Laboratory 9-level primitive-equation model. (See text for explanation.)

days, and positive skill relative to climatological probability out to eight days. Beyond this, incoherent oscillations about the zero line set in. Averaging the two cases smooths out the curves somewhat.

Of course, two trials do not allow one to draw firm conclusions, and I hope that the great effort and expense of preparing such forecasts (at present, 18 machine hours on the UNIVAC 1108 for each day's prediction) will not halt the preparation of more of them. In a recent communication Dr. Smagorinsky has informed me that by reprogramming on a faster computer (CDC 6600) and eliminating some modelling details, the computation time can be reduced to less than one half. But on the basis of the evidence at hand and long experience in extended forecasting, I will venture that:

- 1) Extended runs by dynamical models can make possible day-by-day precipitation predictions at ranges up to 5 days or possibly a week. Beyond this it is unlikely that detailed predictions (day-by-day for specific places) will meet with much success.
- 2) Similar runs may be of value to a mean forecast out to perhaps ten days. Even beyond this period, statistics generated by iterative numerical predictions may be of some value, although this is a moot question to which I shall return later.

I hope that these conclusions will turn out to have been too conservative. In that case, they will only mark the conversion to pessimism of a life-long radical optimist.

There are certainly some pertinent counterarguments to the conservative point of view regarding detailed long-range forecasting as stated by Robinson and by me. First, the results presented above are from recently-developed primitive equation models and it is likely that improvements in modelling will be made. Second, the initial data upon which these forecasts were made cover only the portion of the Northern Hemisphere north of about 20N. Theory and evidence indicate that the entire global atmosphere must be considered for extended range predictions. This probable fact indeed has been given as the principal reason for the WMO World Weather Watch and the associated ICSU-IUGG GARP programs.

No meteorologist in his right mind can question the desirability of carrying forward these programs in the excellent manner recommended by the panels of outstanding scientists who have planned them. But even with the best implementation there arise these nagging questions:

- 1) Will the global atmosphere be observed in sufficient horizontal and vertical detail so that cumulative errors in iterative numerical predictions will not destroy the values of the prognoses after several days?
- 2) Is it economically feasible to set up observation platforms and systems to achieve the required resolution of the initial state?
- 3) Even granting that fine scale resolution can be obtained through more advanced development of satellites, interrogated floating balloons and other still more exotic sensors, will man ever understand the complex workings of the complexly complex

ocean, air and continent system to such an extent that he can carry on successful prognostic iterations so as to make appreciably skillful forecasts of weather at given places more than a week in advance? Or does the atmosphere possess a certain indeterminacy so that it hardly "knows" its own detailed behavior beyond this time?

Since only time will disclose the answers to these questions, let me now turn to the present methods of forecasting for periods of a month or longer, and their historical development. At the heart of this "conventional" attack on the problem lies the supposition that time-averaging processes bring into focus another class of atmospheric systems and motions. At the very end of this lecture I shall make more specific predictions as to probable developments in long-range weather forecasting in the years to come.

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3. Aspects of long-range forecasting²

Summary

The history of long-range forecasting over the past century is traced in this lecture through the work of innovators of synoptic, statistical, and physical approaches. The development of scientific methods appears to have been initiated by the researches of Teisserenc de Bort, who, by averaging sea-level pressure fields over intervals of a month or so, brought to light the "centers of action" and in addition, related prevailing weather conditions over long periods to their strength and position. Spurred on by the undeniable relationships found by de Bort, research workers the world over attempted to develop regional weather types and predict their occurrence. These methods employed to predict were generally devoid of physical reasoning and thus after several fruitless decades led to an era of disillusionment which still dampens the enthusiasm of the meteorological community for long-range forecasting.

New light was cast on the centers of action beginning in the 1930's with the work of several groups, especially that under the direction and stimulus of C. G. Rossby in the U.S.A. These advances stemmed from the discovery of a new class of large-scale and slow-evolving systems in the mid-troposphere (planetary waves) and from the concept of vorticity conservation. This concept provided for the first time a physical rationale for teleconnections and extended forecasting, and ultimately led to the development of short-range (up to a few days) numerical prediction models.

Current long-range forecasting seems to focus on influences external to the atmosphere, particularly feed-back phenomena associated with abnormal boundary influences imposed by oceanic surface temperatures and snow or ice cover, and to focus less sharply on irregular solar influences on the atmosphere. Empirical and synoptic studies carried on with new sophisticated techniques and computing machines are also being developed. The goal now within sight is the development of numerical models for long-range forecasting—statistical and physical, as well as a combination of both. It remains to be seen whether iterative schemes will succeed or whether ensemble approaches using time-averaged states with the help of statistical and largely thermodynamical methods will render the desired objectivity along with increased reliability in predictions.

² This report is reprinted, with minor revisions, and with the kind permission of the publishers, from an article of the same name in *Archiv f. Meteor., Geof. u. Bioklim.*, Suppl. 1, 1966, 96–133.

Introduction

The solution of the problem of long-range forecasting has eluded generations of meteorologists. Although numerous attacks have proceeded in many countries around the world, there is no universally adopted methodology, and at first glance various methods appear to be entirely unrelated. However, this state of affairs is more apparent than real, since the correlation of different meteorological elements in time and space frequently makes possible the discovery of lag relationships in a variety of ways. Because of this circumstance, the present paper will stress the approach with which the author is most familiar, namely that used in the United States Weather Bureau, for it employs synoptic, statistical, and physical techniques and, indeed, contains implicitly many of the considerations used in long-range forecasting elsewhere.

This report, then, consists of a brief description of: (1) the historical highlights of long-range forecasting over the past hundred years; (2) the nature of climatic fluctuations of the order of months and seasons with which the long-range forecaster must be concerned; (3) the synoptic, statistical, and physical methods presently employed at the U.S. Weather Bureau; and (4) the current status of long-range forecasting and avenues most likely to lead to future improvements (some avenues were discussed earlier in this lecture).

History of long-range forecasting attempts

Table 1.3 indicates the names of some outstanding people who devoted significant effort to long-range forecasting and who were innovators of the methods enumerated in the top row. The vertical time scale refers to the period when the innovator *began* his activities in the field. In most cases, his work continued for several years or decades. The author apologizes in advance for any incompleteness or bias in this historical account.

In the first three columns are represented some of the primary researches which have formed the basis for modern scientific long-range forecasting. The birth of the subject can perhaps be traced to the work of Teisserenc de Bort, for it was he who first pointed out the existence of macroscale pressure patterns which he called the centers of action—a

Table 1.3. *Innovators of long-range forecasting procedure according to period when their work began. Numbers refer to references found at end of paper*

Period	Centers of Action and Weather Typing	Correlations and Regressions	Teleconnections	Analogues	Cycles & Harmonics	Singularities & Symmetry Points	Key Periods	Cosmic Influences	Kinematics & Synoptics	Feed-Back Studies or Surface Influences	Thermodynamic & Dynamic Numerical Models
1880	Teisserenc de Bort 1				Howard 26 (1842)	Buchan 34 (1867)	Old Sayings like St. Swithin's			Blanford 10	
1890	Clayton 2	Blanford 10			Brückner 66						J. J. Thomson 58
	Abercromby 3										
	Hildebrandsson 4									Hildebrandsson 4	
1900										Meinardus 48	
		Lockyers 11	Walker 12							Schott 49	
1910		Walker 12						Walker 40		Petersen 50	
		Braak 13							Arctowski 46	Helland-Hansen & Nansen 51	
1920								Clayton 41		C. F. Brooks 52	
	Multanovsky 5			C. E. P. Brooks 22				Baur 42		C. E. P. Brooks 53	
	Baur 6	Groissmayr 14	Defant 19	Mascart 23	Alter 27	Weickmann 35				Weise 54	
1930		Baur 15	Exner 20		Petitjean 28	Schmauss 36					
					Berlage 29		Baur 39			Sandström 55	
1940	Bjerknes 7				Abbot 30			Abbot 43	Baur 6	Rodewald 47	
	Rosby 8		Rosby 8						Namias 9		Rosby 8
	Namias 9	Wadsworth 16	Martin 21		Haurwitz 31	C. E. P. Brooks 37		Willett 24			Stewart 59
1950											
		Klein 17		Willett 24	Takahashi 32	Grappe 38					Phillips 60
		Gilman 18		Craddock 25					Craddock 25		Blinova 61
1960										Namias 56	Smagorinsky 62
										Bjerknes 57	Mintz 63
1970					Van Mieghem 33			Brier & Bradley 44			Charney 64
											Adem 65

term which is still appropriate. With the help of maps covering North America, the Atlantic, and Eurasia, Teisserenc de Bort not only illustrated the centers of action but also pointed out their association with prevailing weather conditions. This work was performed around 1880, during a period when meteorologists were concentrating on the tracking of individual cyclones and anticyclones, and must therefore be considered most outstanding. Later a number of investigators including H. H. Clayton, Abercromby and Hildebrandsson began to develop weather types, so that the climatological statistics for periods of a month or so took on new meaning. It soon became evident that the weather abnormalities of a given period were linked to the positions and configurations of the centers of action and that it would be necessary to forecast these positions and configurations in order to predict the prevailing weather.

Since no one had advanced a satisfactory physical theory for the generation and maintenance of abnormal positions of the centers of action, it was natural to try statistical prediction techniques. Thus, in addition to the people listed above, Blanford, the Lockyers, Walker, Braak and many others entered into a search for simultaneous and lag relationships. The most striking results were the simultaneous correlations between sea-level pressures in different parts of the world. Thus Walker's North Atlantic, North Pacific, and Southern Oscillations, and Defant's and Exner's further work on North Atlantic pressure distribution and zonal indices became common knowledge to the world meteorological community. However exciting these studies of simultaneous correlation were, they did not provide the key to prediction of the position and orientation of the centers of action over periods of a month or more.

Around 1920, however, some fresh ideas were introduced, clarifying the characteristics of the centers of action and relating them to the interacting synoptic-scale features of weather maps—cyclones and anticyclones. Among the pioneers in this period of discovery were Multanovsky, who developed the concept of the "natural period," and Baur, who introduced the term "Grosswetterlagen." During the "natural period" a series of cyclones, and a series of anticyclones, would trace paths within areally restricted envelopes. Multanovsky also introduced the term "synoptic season," during which the natural periods would be more or less constant in length. The natural period might be as little as 5 days in one "synoptic season" and as much as ten in another. Baur went a step further and statistically proved the reality of larger-scale meteorological systems which steer the cyclones and anticyclones in preferred directions. Baur's concept of steering forms the cornerstone of much of the practice of modern-day synoptic forecasting, although electronic computers have made possible new methods of determining steering through the concept of vorticity redistribution. Multanovsky and Baur and their followers set up exhaustive catalogues of circulation and weather types useful in long-range prediction.

The next major discovery having long-range forecasting implications came with the work in 1935–40 of Rossby and his colleagues (of whom the author was one). Working with synopticians and holding frequent discussions based on the first airmass and frontally-analyzed hemispheric charts, Rossby formulated a theory for the behavior of the centers of action. This work immediately cast considerable light on the teleconnections determined statistically by earlier workers, and in addition developed an objective procedure for predicting the positions of the centers of action through the concept of the vorticity redistribution associated with long waves in the westerlies and the zonal westerly index. Large-scale planetary wave systems and their motions had already been identified and discussed by J. Bjerknes in 1937, so that he, too, should be considered a pioneer in long-range forecasting, even though modesty would probably not permit him to accept this accolade. Subsequent to the work of Rossby and collaborators, an operational Extended Forecast Unit was set up under my direction in the U.S. Weather Bureau where we tried to extend Rossby's ideas. Our work was greatly assisted by the expansion of the network of upper-air observations over most of the Northern Hemisphere and by strong government support during the war years. In addition to developing many new climatological aids for purposes of long-range forecasting (including mean maps, frequency distributions of singular features, etc.) we also treated problems of a time scale longer than the 5 days to a week with which Rossby's group had worked. It is our work on these problems that I shall focus on in subsequent portions of this paper.

Let me proceed now to some remarks on the remaining columns of Table 1.3. The procedure of selecting analogues as forecast aids has probably attracted meteorologists since the time when a few years' file of weather maps first became available. Analogue methods have been used in long-range as well as in short-range forecasting. C. E. P. Brooks and Mascart were active in this sort of work during the 1920's. More recently Willett and Craddock have devoted a good deal of attention to it. Whereas Brooks and Mascart dealt largely with sea-level pressure distributions, Craddock and the British consider a long series of temperature maps as well. Willett, like the Russian and Japanese investigators, includes the phase of the sunspot cycle along with meteorological parameters in selecting analogues. The analogue method has the appeal of representing real events in the past and, in a sense, constitutes an "easy solution" to a difficult problem. Unfortunately, the use of analogues for purposes of long-range forecasting is greatly restricted by the lack of a sufficiently long series of charts and data. It follows that closely parallel monthly mean conditions over large areas are difficult if not impossible to find. Nevertheless, increasing masses of data and fast sorting provided by electronic computers keep alive the search for improvement in long-range forecasting through analogue techniques.

The next three columns enclosed by the heavy vertical lines represent procedures which today receive little attention from long-range forecasters. The first of these, the use of cycles, has suffered many blows, the latest and perhaps most devastating being power spectrum analysis. Power spectra fail to show significant cyclical phenomena in the troposphere other than the diurnal and annual cycles. Therefore the application of harmonic analysis in space and time has not been especially fruitful for prediction. It appears to be better suited to diagnosis and description.

The next column, Singularities and Symmetry Points, identifies a number of fascinating possibilities for long-range prediction which have not, however, materialized. Unfortunately, singularities shift in time and character sufficiently to destroy their practical usefulness. Similarly, while symmetry points are occasionally observed in meteorological time series, no method has been developed to determine when symmetry will arise.

The concept of Key Periods (next column) implies that the dominant character of the weather of a forthcoming season is indicated by the character of some shorter period in the preceding season—perhaps a pentad. Although there seems to be some statistical justification for this procedure, the results, like those from singularities, leave so much to be desired that one is justified in questioning its practicality.

The last four columns deal with subjects currently receiving a great deal of attention in long-range forecasting research as indicated in World Meteorological Organization Technical Note No. 66. Cosmic influences are believed by many to be fundamental causes of abnormalities in terrestrial weather and climate. Most investigators of these lines have striven to show that irregular variations of solar activity are important factors. Only a few of the many present-day researchers have been listed, the principal ones being Baur and Willett. The pioneering work of Clayton and Walker should receive mention. Since some workers utilize the phase of the sun-spot cycle (or some associated solar index) in addition to meteorological parameters (e.g., in analogue selection), it is not possible to separate the portion of forecast success due to the solar input from that due to terrestrial considerations. The entire subject of solar weather relationships remains, as it has for decades, controversial. Perhaps new solar observations from satellites will permit more definitive studies. I prefer to leave this complex subject out of my discussion.

The studies cited in the next column treat the motion and behavior of macroscale systems with the help of synoptic mean charts and kinematics. The long-period continuity which often shows up in anomalies of the centers of action (that is, in movements of centers of maximum deviation from normal) was pointed out in the 1920's by H. H. Clayton, C. E. P. Brooks, H. Arctowski; in the 1930's by F. Baur and M. Rodewald; and by myself in the 1940's.

Because of the availability of more extensive data and highspeed computing methods, kinematic and synoptic methods have recently been developed much further than ever before. Kinematics may now include (implicitly) the net influence of seasonal change associated with the changing radiation balance, as I shall illustrate later on.

The next column, Feed-Back Studies or Surface Influences, refers to work on topics which have recently come into prominence for long-range forecasting. Strangely enough, these topics were investigated intensively around the turn of the century by such people as

Blanford, Hildebrandsson, Meinardus, Schott, Petersen, Helland-Hansen and Nansen, C. F. Brooks, C. E. P. Brooks, Weise, and Sandström. Here again fresh and extensive data, high-speed computing, and a better knowledge of the general circulation have led to a revival of interest. Some newer ideas will be discussed later.

The final column, Thermodynamic and Dynamic Numerical Models, is presently more of an expression of hope than a realization of a practical methodology. Obviously, until long-range forecasting can be developed to a numerical procedure, it will remain in an unsatisfactory state. In this column I have included the work of Rossby, Phillips, Smagorinsky, Mintz, Charney, Blinova, and Adem. Also to be considered are the many meteorologists who have dealt with the problem of the general circulation of the atmosphere. While these studies are leading to a good understanding of the mechanics of the general circulation and the determinants of climate, this will not automatically provide a solution for long-range forecasting, a prediction problem of higher order of complexity.

Some statistical aspects of the long-range forecasting problem

It has long been recognized that a great deal of coherence exists in meteorological data, not only in space but in time as well. The coherence in time is associated not only with the day-to-day autocorrelation found in continuous meteorological elements but also with persistent recurrence of circulation patterns and associated weather events over time intervals from a few days to a week on upward. Thus the standard deviations of monthly means are surprisingly high, and long-period spells or regimes often characterize seasons or years. Such coherence may be looked upon as the statistical manifestation of climatic fluctuations with which the long-range forecaster is concerned.

The coherence of circulation and weather patterns in *space* has been well known, at least qualitatively, from early work on the centers of action. More recently this problem has been attacked with the help of advanced statistics. Among the more illuminating of these researches has been the work of Gilman with empirical orthogonal functions. Gilman found out how many functions were required to reconstruct temperature and precipitation anomaly patterns (see Fig. 1.3). He found that three such functions could

FIG. 1.3. Reconstruction of monthly temperature and precipitation over the US by empirical orthogonal functions. Number of functions on the abscissa; cumulative explained variance on the ordinate. Winter months, 1899–1939; 30 temperature stations, 64 precipitation stations. “Transformed” precipitation $p = \sqrt{x} + \sqrt{x+1}$ where x = untransformed precipitation in inches (after Gilman).

reproduce the pattern of monthly mean temperature anomaly over the U.S. so that 81% of the variance was accounted for, as may be seen from Fig. 1.3. Twenty functions accounted for practically all the variance. On the other hand, precipitation required 20 functions to account for an 80% reduction in variance. From Fig. 1.3 it is therefore clear that precipitation forecasting for monthly periods is much more difficult than temperature forecasting. A qualitative way of stating these results is that the space scale of precipitation patterns is often rather small compared to the space scale of temperature patterns.

Another question relating to the nature of long-range forecasting involves the frustrating characteristic that procedures developed during one decade or score of years may not hold up in succeeding periods. This dilemma came to light with shocking effect in the classic failure of predictions of the Asiatic Monsoon begun around 1910 by Walker. Another example is strikingly illustrated in Fig. 2.3, taken from the work of Gilman.

FIG. 2.3. Statistical regression forecasts of winter monthly temperatures at 30 stations from hemispheric empirical orthogonal functions of 5-day mean pressure. Year (Jan.-Feb.-Mar.) on the abscissa; average skill score (within one class of forecast) on the ordinate. Zero skill means random drawing from the climatological frequency distribution. Trend line fitted by least squares (after Gilman).

Using sea-level pressure, temperature, and precipitation data from the years 1899 to 1939, he devised a linear regression method for predicting temperature and precipitation from the empirical orthogonal functions of pressure. He purposely left the post-war period from 1948 to 1961 free for testing equations derived from the earlier years. As will be seen from Fig. 2.3 the predictions started out to be reasonably successful—that is, with positive skill scores. However, a rapid decline soon set in so that by the late 1950's all the skill had vanished. While it is difficult to interpret these results, they suggest that there is some fundamental missing ingredient not presently incorporated into the prediction system. It is my personal feeling that this ingredient is external to the atmosphere and may be the complex effect of abnormal boundary surfaces associated with variable snow cover and variable ocean temperatures. On the other hand some may argue that such failures are due to the lack of consideration of extra-terrestrial phenomena associated with solar variations. Whatever the answer, it appears that there is at present much room for studies attempting to isolate physical factors which play a role in producing climatic variations of a month or more.

A word should be said here—without attempting a survey—about verification studies, for these studies, by providing assessment of error, not only keep investigators on an even keel, but also offer clues for further investigation. Here again the computer makes possible the easy computation of all sorts of statistics—variance, correlations, root-mean-square errors, etc. In addition, it makes possible the evaluation of forecast probability estimates produced either subjectively or objectively.

The problems of specification and teleconnection seen in modern light

Procedures of typing weather according to the geometry of the pressure pattern, while quite helpful in the past decade, have been superseded by advanced computer techniques. We have already mentioned the use of empirical orthogonal functions. A technique which has shown considerable skill and practical value in extended forecasting is the screening method developed by Miller and applied judiciously to many elements by Klein. The central idea of the method is represented in Fig. 3.3, wherein 5-day mean temperature anomalies for a given city (Cleveland, Ohio) in 10 winters are related to the 700 mb heights at the various grid points given in the upper part of the figure. Machine computation “screens” for the point of highest correlation, then removes the effects of this correlation, finds the highest remaining correlation, and so on. The final product is a multiple regression equation expressing the temperature departure from normal at Cleveland in winter as a function of other points as indicated by the formula in the figure. For the U.S. this is done for some 40 points for each season. A typical example of a specification by this procedure is shown in Fig. 4.3. Klein has also applied the screening technique to the specification of precipitation patterns (example

FIG. 3.3. Upper: Network of 70 grid points used to delineate the field of 700 mb heights used in devising specification equations. Lower: Multiple regression equation used in predicting 5-day mean temperature at Cleveland, Ohio (located by heavy solid dot) during the winter, as a function of 5-day mean 700 mb height centered 2 days later (H) and five-day mean surface temperature centered on forecast day (T) at points given in parentheses. The location of the predictors is given by the open circles, the order of selection by the number inside the circle, the type of variable by the letter above the circle, and the multiple correlation coefficient after inclusion of the given predictor by the decimal below the circle (after Klein).

in Fig. 5.3) and has arrived at usable results. Specification skill scores³ obtainable for monthly-mean temperatures over the U.S. show averages of 31 for temperature and 21 for precipitation. While these results are far from perfect, it must be remembered that only the 700 mb mean monthly pattern is used to specify the temperature. Klein also employed screening to specify sea-level pressure patterns associated with 700 mb mean patterns. An example is shown in Fig. 6.3. The explained variance for such a procedure when applied to monthly charts comes to 68%.

These specification procedures are applied routinely to prognostic monthly mean contours and offer a first approximation of the sea-level temperature, pressure, and precipitation patterns.

The errors in the specification of the past month's temperature from observed contour heights often afford clues as to abnormal boundary effects like variable snow cover.

It is clear from work carried on thus far that specification is much easier than prediction, and that an important cause of poor forecasts is inability to predict the mean circulation at one or more levels.

We shall now make a few remarks about spatial interdependence. The discovery of teleconnections, many of which were found by Sir Gilbert Walker, has also been greatly facilitated by high-speed computers. Using hand methods, Martin outlined an attack on this problem utilizing centers of positive or negative 700 mb anomaly and finding out the probability of sign of anomalies in adjacent and remote areas. His work has been extended by O'Connor, who used a finer grid of points (10° apart) over most of the Northern Hemisphere, capitalized on a much larger data sample, and used electronic computers. Fig. 7.3 shows the influence of an Icelandic positive or negative mid-tropospheric height anomaly on the circulation elsewhere, particularly in Europe, over the

³ A skill score is a percentage measured on the scale from chance expectation (defined as zero) to perfection (defined as 100). That is, skill equals per cent correct minus per cent expected correct by chance, divided by 100 minus per cent expected correct by chance. The chance expectation is calculated on the model of a random drawing from a frequency distribution, usually the climatological one—as in this case.

Monthly temperatures are here classified in five categories whose climatological frequencies are .125, .250, .250, .250, and .125; monthly precipitation in three categories of climatological frequency .333 each. Seasonal temperature and precipitation are also classified in three equally-likely categories.

MID APR. — MID MAY 1961

FIG. 4.3. Specified and observed departures from normal of mean surface temperature for the 30-day period from mid-April to mid-May 1961. Analysis is in terms of the five temperature classes customarily used in the U.S. Weather Bureau.

AVERAGE SKILL SCORE (35 CASES) 31

Atlantic, and over Eastern North America. These lines of equal probability of sign may be interpreted as isopleths of anomaly. Charts such as Fig. 7.3 have been printed out for all areas north of 20° in the Northern Hemisphere and are quite useful in long-range forecasting. Obviously, it is first necessary to select those anomalies in which the forecaster has high confidence before using these as foundations for constructing the edifice of the complete contour pattern.

We stress that specification or the use of teleconnections is of limited value unless more is known about the *evolution* of the general circulation over the period for which the forecast is desired. Some examples given below illustrate characteristic evolutions and methods used to predict them.

Synoptic-statistical-physical method of long-range forecasting

In order to arrive at a prediction of the general circulation for a month ahead, we in the U.S. Weather Bureau employ numerous tools which have their bases in synoptics, statistics, and large-scale physics. Naturally, these tools are presently weighted on the synoptic and statistical side because of the poor state of knowledge of atmospheric

MID DEC. 1964 — MID JAN. 1965

FIG. 5.3. Specified and observed monthly precipitation for the 30-day period from mid-December 1964 to mid-January 1965. Analysis is in terms of three equally probable precipitation classes.

physics. However, the synoptic and statistical tools are never used blindly, but are always evaluated against the background of physical insight. We believe that such a combination of methods can yield the most successful results. Rather than discuss these methods in detail, I shall illustrate them by describing certain synoptic sequences in which the large-scale features on monthly mean charts appear to have an evolution that could to some extent be anticipated using our methods. The description which follows should strike a sympathetic chord in those who have used other methods of long-range forecasting, because there are many ingredients in common.

The first sequence of charts in Fig. 8.3 (seasonal means) shows the apparently regular eastward displacement of a trough with negative anomaly from the west-central Pacific in summer 1957 to the west coast of the United States in the spring of 1958. Although this regularity of motion could conceivably be the result of chance, there are reasons for considering it to have a particular explanation.

The steady eastward progression of the trough may have been associated with an anomalous pattern of water temperatures which in turn was created and maintained by anomalous prevailing wind systems and by certain adjustments in the mean planetary

SEPTEMBER 1964

FIG. 6.3. Specified and observed monthly mean sea-level pressure (mb) for September 1964.

FIG. 7.3. Probability, in per cent, of sign of 5-day mean 700 mb height departures from normal for all winter periods from 1947 to 1963 with *positive* centers (16) at 60N 30W on left chart, and *negative* centers (14) at 60N 30W on right chart. Heavy line is 50% probability, and interval is 10%.

FIG. 8.3. Seasonal 700 mb mean contours (solid) and isopleths of departure from normal (broken) for summer 1957 through spring 1958. Contours are labelled in tens of feet as are also the centers of maximum departure from normal.

wave trains. These adjustments were perhaps made necessary by normal seasonal variations in the position of the Asiatic trough and changes in strength of the zonal westerlies over the Pacific.

In more detail, the essence of the explanation is that the anomalous prevailing winds in the vicinity of the Pacific trough create an anomalous Ekman drift of the surface water, which in turn leads to warmer-than-normal masses east of the trough, and, similarly, colder-than-normal surface water to its west (inferences supported to some extent by observed water temperatures) (see Fig. 9.3). This sharpened longitudinal temperature contrast is rapidly and frequently imparted to overlying air masses, causing, in the domain of the mean trough, greater baroclinic development of cyclones than otherwise. This enhanced development then provides longevity to the mean trough. The trough's geographical location, however, would be influenced also by neighboring parts of the general circulation, especially the upstream trough over eastern Asia. This latter quasi-stationary feature of the general circulation can be found in almost the same location each winter, so that, with only slight variations in position and intensity, it is a climatological fixture. The almost unending series of cyclones along the front of the Asiatic monsoon draw energy from the contrasting air masses deployed by the Asiatic High and west Pacific anticyclone and, occluding, bring about a marked increase in the Pacific westerlies from summer to autumn to winter—another climatological event with high probability of occurrence. From Fig. 8.3 it is clear that both phenomena, the

JULY 1958

FIG. 9.3. Upper left: Sea-level mean isobars (solid) and isopleths of departure from normal (dashed) for July 1958. Isobars and anomaly centers labelled in millibars. Upper right: Normal surface wind rose for July and that for July 1958 as computed from surface weather map analyses for the hatched 5° square shown in chart in upper left. Length of bars indicates relative frequency of winds from indicated direction. Lower left: Computed sea-surface temperature anomalies using advection in the Ekman layer for July 1958 (F). Lower right: Observed sea-surface temperature anomalies for July 11–20, 1958 (F).

anchored Asiatic trough and the accelerated westerlies, were observed as the seasons evolved, and consequently, the locus of mean upper-level trough development in the Pacific moved eastward in phase with the baroclinic effects induced by the anomalous water temperatures. Thus, a continued positive interaction between the two processes was permitted.

A similar series of events was also observed from the autumn of 1959 to the following winter. However, the time required for the eastward transit of the trough across the Pacific was considerably shorter—of the order of a few months rather than seasons.

Perhaps it should be emphasized that this sequence of events does not proceed every year, although it is apparently frequent enough to influence the appearance of normal monthly upper-air patterns. The initial state of the general circulation as well as the water temperature anomalies may differ from one year to another. For example, if a mean trough with negative anomaly and the accompanying water temperature deviations has not existed in the west-central Pacific during the summer and early autumn a different series of mean patterns may follow.

We shall now discuss a striking case of *retrogression* of planetary wave features in monthly mean patterns over the North Pacific from February through May 1965. It is probably not accidental that this case of retrogression occurred between late winter

FIG. 10.3a, b. 30-day mean 700 mb contours (labelled in tens of feet) for February through period mid-May to mid-June 1965. Heavy broken line indicates retrogressive Pacific ridge.

FIG. 11.3. *Upper:* Mean sea-surface temperature departures from normal (in F) for Spring 1965.
Lower: Mean sea-level isobars (solid) and departures from normal (broken) for Spring 1965.

and spring, because in contrast to the earlier situation the zonal index declines and the Asiatic monsoon trough retreats (climatologically probable events). Fig. 10.3a and 10.3b illustrate the retrogression of a ridge originally off the west coast of North America in February to a position in the Bering Sea by mid-April to mid-May and to northeastern Siberia in mid-May to mid-June. The retrogression of the ridge was partly associated with the climatologically forced retrogression of the Asiatic coastal trough and diminution of the zonal westerlies from winter to spring and the climatologically forced tendency of the Hawaiian trough to disappear. A new trough, generated in the Gulf of Alaska in April, retrograded to the Bering Sea and Aleutians. The generation of such Gulf of Alaska troughs is now known with considerable certainty to be caused by the transport of cold air from the Alaskan Arctic into the Gulf of Alaska. Once a retrograding ridge passes the eastern Aleutians it deploys the cold air southward and rapid diabatic heating of this cold air over the relatively warm Gulf waters promotes intense convection and cyclogenesis. Later on in mid-May to mid-June the unusually strong cyclogenesis of 1965 appears to have been assisted by sea-surface temperatures, which were substantially above normal during the spring of 1965 (Fig. 11.3 upper). The warm water temperatures had been produced mainly by abnormalities in sea-level pressure through an Ekman drift to the right of the anomalous sea-level component of geostrophic wind (Fig. 11.3 lower).

The prediction of such retrogression, often difficult, was rather well supported this time by kinematic charts. Four of these charts, March through mid-April to mid-May, are shown in Fig. 12.3. The contours are for 30-day mean observed patterns and centered 10-day tendencies are represented as broken lines with the centers of maximum 10-day change indicated by numbers. The tendencies are computed by subtracting a 30-day mean centered 5 days before the middle month period from a 30-day mean centered 5 days after. The five unknown values are predicted by numerical models (baroclinic to 36 hours and barotropic beyond). Such kinematic charts are especially valuable in assessing the slow evolution of macroscale features. By combining them with normal monthly changes in mid-tropospheric height, one may construct first estimates of the likely height distribution from two weeks to a month in advance. After these objective calculations have been performed, subjective judgment enters into the decision-making. Martin's and O'Connor's teleconnection charts then become important. For example, the retrogression of the Pacific ridge is so strongly indicated in Fig. 12.3 that its 30-day

FIG. 12.3. Kinematic 30-day displacements (heavy arrows) for ridges and troughs of 700 mb mean charts based on 10-day centered tendencies (broken lines).

FIG. 13.3. Mean 700 mb contours (solid) and isopleths of anomaly (broken) (both labelled in tens of feet) for Winter (Dec., Jan., Feb.) 1962–63.

predicted position given by the end of the arrow would form one of the cornerstones of the forecast, and teleconnection probabilities from this base point would hint at the formation of other systems—such as a Gulf of Alaska cyclone, once the ridge has crossed the critical area of the Eastern Aleutians.

The final aim is to prepare a monthly mean prognostic circulation chart from which temperature, precipitation, and sea-level pressure patterns can be estimated by means of the specification method described earlier.

For longer time ranges—for example, seasons—the use of kinematic techniques has not been found successful, because the change of season is overwhelmingly important. Therefore, the use of other techniques, particularly statistical ones, becomes more relevant. Before briefly mentioning some of these statistical procedures, let me refer to the circulation during the highly anomalous winter 1962–63 of which all Europeans are especially cognizant, for some clues as to the identity of useful statistical predictors may be found in this period. The mid-tropospheric mean contour pattern and anomalies for the winter are shown in Fig. 13.3, where it is seen that four major waves dominated the hemisphere. Particularly noteworthy was the very strong trough (minus 400 ft. departure from normal) north of Hawaii. Of course, this trough was associated with much stronger-than-normal cyclogenesis in this area. If one could predetermine this area of cyclogenesis, it can be shown through the teleconnection studies or through the concept of vorticity redistribution that a strong ridge extending from the west coast of North America into Alaska would be called for. This, in turn, would produce a strong trough along the east coast of North America that would be reintensified by cyclogenesis along the edge of the intensely cold Arctic air flowing into the trough. The early development of the east coast cyclones and their fast occlusion far southwest of normal (off Newfoundland instead of Iceland) may have been a major factor in creating the large quasi-permanent block which persisted over Iceland and Northern Europe. While it is impossible at present to assign any area of the map priority in setting up this pattern, it seems that the Pacific trough might be a good contender. It may be significant that this trough developed over a pool of unusually warm water (Fig. 14.3), which developed during the summer and fall of 1962 in the very area where major cyclogenesis proceeded during winter. Until these ideas are proven with the help of a numerical, physically-based forecasting model, only circumstantial evidence can be presented for such a hypothesis.

If the surface abnormalities do in fact produce abnormalities in the pressure and wind distribution aloft, it may be possible to obtain some idea of the seasonal character of the circulation well in advance, not only through studies of abnormalities of sea-surface temperature but also through the influenced meteorological records. Therefore we regularly plot and analyze sea-surface temperatures over the Pacific and Atlantic and snow cover over North America. We also prepare prognostic upper-level charts, using regression equations derived from data of the past 20 to 30 years. Equations for each

FIG. 14.3. Isopleths of departure from normal (in F) of sea-surface temperature for the months September through December 1962. Shaded areas are colder than normal.

FIG. 15.3. See text for explanation.

FIG. 16.3. Examples of good (upper), average (middle), and poor (lower) seasonal temperature forecasts.

grid point are based on local season-to-season and month-to-season autocorrelations. An example is shown in the upper left of Fig. 15.3. And finally, contingency tables for individual states of the country are composed from a 60-year record and probabilities for the subsequent season plotted.

Fig. 15.3 (upper right) shows the contingency table solution for the summer of 1959, along with the final temperature prediction and the observed temperature. The skill scores of such seasonal forecasts are very modest, as is shown in Table 2.3. Yet the predictions are significantly superior to climatological probabilities and to persistence. Examples of a good, fair, and poor temperature forecast drawn from the population which makes up the statistics of Table 2.3 are given in Fig. 16.3.

TABLE 2. 3. Distribution of seasonal temperature forecast skill scores by season, Winter 1959 until Fall 1967 (35 seasons).

	Forecast	Persistence
Winter	15	8
Spring	2	-5
Summer	8	12
Fall	9	11
Annual	8.6	6.5

Among other groups in the world who have been making seasonal predictions publicly or experimentally, we must mention the veteran Baur, who has developed elaborate statistical procedures for drawing conclusions. Baur's frequently successful forecasts of wet or dry summers in Europe should encourage many workers in this field and should elevate in the mind of the public at least the subject of seasonal prediction. No discussion of long-range forecasting would give a complete picture of potentialities

Numerical *physical* objectives

without substantial reference to the fundamental research proceeding on the general circulation—for progress on this central problem automatically carries with it the possibility of progress on the long-range weather forecasting problem. Numerical methods applying purely hydrodynamical and thermodynamical methods to the general-circulation problem are being pursued vigorously in many areas of the world. At present, highly simplified models are being tested to determine the primary mechanisms in the atmosphere which give it its characteristic time and space scales and also the variability of these scales. Stated simply, these researches are designed to answer the question as to how solar energy is ultimately utilized and how the atmosphere responds in order to transfer heat most efficiently. After this question is answered and some gross characteristics are explained, detailed answers can be sought. Unfortunately these *details* may require even more exact knowledge of such elusive physical processes as release of latent heat, momentum and water-vapor exchanges, internal turbulent exchanges, radiation transfers and in fact, the entire gamut of meteorological processes.

It is possible that some of these processes can be omitted or perhaps greatly simplified for certain predictions. On the other hand, there is no guarantee that a completely physical solution will be found, and for this reason alone the empirical approach must proceed. Besides, it appears likely that, as with short-range numerical forecasting, empirical knowledge will be highly necessary in making optimum practical use of any physically-based long-range forecasting model. Furthermore, theoreticians can ill afford to ignore decades of empirical research in setting the direction of their efforts.

In recent years attempts have been made in the Extended Forecast Division, particularly by J. Adem, to formulate a numerical prediction model for periods of a month or a season. The basic equations used are those of conservation of thermal energy in the surface of the Earth and in the mid-troposphere. The model predicts the anomalies of temperature in the underlying surface and in the mid-troposphere. Besides radiation other forms of heating are generated within the model. This is done by expressing such heating as a linear function of variables predicted in the model.

The anomalies directly incorporated are those in the storages of thermal energy which are introduced by prescribing in the previous interval the temperature of the surface water in the oceans and the temperature of the mid-troposphere, as well as the anomalies in the short-wave radiation absorbed by the surface; which in turn are introduced by prescribing the albedo (snow cover) at the end of the previous month.

The numerical experiments show that important anomalies of the evaporation at the surface, of the vertical turbulent transport of sensible heat from the surface, of the condensation of water vapor in the clouds and of the cloudiness are introduced by the anomalies of the computed temperature fields. Furthermore, these induced anomalies of the heating functions and of the cloudiness in turn introduce changes in the anomalies of the temperature fields. The first such prediction using Adem's first model is shown in Fig. 17.3. Preliminary verification of a year's series of forecasts shows skill over climatology, persistence or return to normal for both air temperatures over North America and for sea-surface temperatures over the North Pacific and North Atlantic oceans.

FIG. 17.3. Temperature departures (in $^{\circ}\text{C}$) from normal at the surface of the Earth for January 1963. (A) Predicted; (B) Observed (from Adem).

*Optimism for
the future*

Numerical experiments are also being carried out to test the possibility of using satellite data in this model.

We note many recent developments which may well lead to significant, practical improvements in long-range forecasting techniques:

- 1) Greater and improved sources of data in the world (including satellite measurements, constant level satellite-interrogated balloons, ocean buoys and other sensing devices);
- 2) Fast and economical methods for processing these data and for computing by statistical and physical methods;
- 3) Better understanding of the dynamics of the general circulation through studies of long waves, heat and momentum transports, transformation and exchanges, etc.

There is a new generation of young meteorologists well versed in physics, mathematics and electronic computing. One of our tasks is to introduce them to the real problems of long-period atmospheric behavior, in the hope that they may apply their talents with greater success than earlier generations.

Since I cannot resist the temptation to make predictions, I foresee routine preparation of the following economically valuable forecasts:

- 1) Detailed weather predictions out to one week. Given the funding, these are possible now.
- 2) Forecasts of weekly weather regimes out to one month—probable in a few years.
- 3) Prediction of monthly means and other weather statistics with double the present skill—in 5 to 10 years.
- 4) Prediction of seasonal general weather characteristics—in 5 years.
- 5) Rough estimates of the character of a season one year ahead—in a decade.
- 6) Estimates of average climatic deviations from normal for a 5 or possible 10-year period—perhaps in 10 years.

Although I hesitate to hazard a guess of developments of forecasting capability beyond a decade, the new generation of dynamically-oriented climatologists may provide material for an entirely new chapter on forecasting.

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