

a retrospective view of Richardson's book on weather prediction*

George W. Platzman

The University of Chicago, Ill.

Contents

Abstract	514
Foreword	514
1. Prologue	516
2. Summary of the book	517
3. Contemporary opinion	519
4. The prediction model	523
5. Richardson's dynamics	528
6. The initial data and forecast	531
7. Bjerknes, Margules, and Dines	537
8. Epilogue	541
Notes	544
References	550

Abstract

In 1922 Lewis F. Richardson published a comprehensive numerical method of weather prediction. He used height rather than pressure as vertical coordinate but recognized that a diagnostic equation for the vertical velocity is a necessary corollary to the quasi-static approximation. His vertical-velocity equation is the principal, substantive contribution of the book to dynamic meteorology.

A comparison of Richardson's model with one now in operational use at the U. S. National Meteorological Center shows that, if only the essential attributes of these models are considered, there is virtually no fundamental difference between them. Even the vertical and horizontal resolutions of the models are similar.

Richardson made a forecast at two grid points in central Europe and obtained catastrophic results, in particular a surface pressure change of 145 mb in 6 hours. This failure resulted partly, as Richardson believed, from inadequacies of upper wind data. Underlying this was a more fundamental difficulty which he did not seem to recognize clearly at the time he wrote his book: the impossibility of using observed winds to calculate pressure change from the pressure-tendency equation, a principle stated many years earlier by Margules. However, he did point in the direction in which a remedy was later found: suppression or smoothing of the initial field of horizontal velocity divergence.

* *Weather Prediction by Numerical Process*, by Lewis F. Richardson. Cambridge, University Press, 1922, xii + 236 pp., 4°. Reprint, with a new introduction by Sydney Chapman: New York, Dover Publications, 1965, xvi + 236 pp., 8° paperback.

The 6-hr time interval used by Richardson violates the condition for computational stability, a constraint then unknown. It is sometimes said that this is one of the reasons his calculation failed, but that interpretation is misleading because the stability criterion becomes relevant only after several time steps have been made. Since Richardson did not go beyond a calculation of initial tendencies—in other words, he took only one time step—violation of the stability criterion had no effect on the result.

Richardson's book surely must be recorded as a major scientific achievement. Nevertheless, it appears to have had little influence in the decades that followed, and indeed, the modern development of numerical weather prediction, which began about twenty-five years later, did not evolve primarily from Richardson's work. Shaw said it would be misleading to regard the book as "a soliloquy on the scientific stage," but in fact that is what it proved to be. The intriguing problem of explaining this strange irony is one that leads beyond the obvious facts that when Richardson wrote, computers were nonexistent and upper-air data insufficient.

Foreword

The reprinting of Richardson's book by Dover Publications is a fortunate concurrence of a name famous in meteorology and a publishing house which has earned the respect of the scientific community. For this occasion I willingly accepted the invitation of the BULLETIN's Editor to submit a review; but whereas the conventional brief format was intended, what emerged is manifestly very different! Lest this outcome be thought to stem from mere prolixity, I must mention several points that, in my view, came to justify so many more words than expected.

First, although its specific influence on later developments in dynamic meteorology and weather forecasting is debatable, Richardson's astonishingly prescient book is without doubt an important and fascinating document, the brilliantly original work of a remarkably imaginative mind. Second, not since the polar-front model was introduced almost 50 years ago has a scientific advance had such profound and widespread influence through the whole of meteorology as has the

modern development of numerical weather prediction, which began about 25 years after the book was published. Now, after almost two decades of this development, we can view Richardson's book in broader and deeper perspective than was possible before. Third, although Richardson as a name is known to every student of meteorology (fortunately we have the Richardson number!), and although the existence and nature of his book probably are equally well known, yet I doubt that there is adequate appreciation even by specialists, of how rich is the book in substance that is noteworthy in the present day. Finally, the general meteorological reader heretofore, I believe, has not been given a satisfactory opportunity to understand the scope and meaning of Richardson's book as a whole. For this reason, what I have written is entirely nonmathematical in exposition. I hope in that way to have succeeded in epitomizing this historic work in a manner that can be widely understood.¹

From remarks made to me by Prof. Sverre Pettersen and Prof. Arnt Eliassen, I was fortunate in learning of the existence of an important file of manuscript and other materials collected by Richardson for a second edition of his book. This was in the custody of Oliver M. Ashford (World Meteorological Organization, Geneva), who very kindly made it possible for me to examine it, and since has conveyed it for permanent retention in the Library of the Royal Meteorological Society, London. The file, about an inch thick, consists of an annotated

copy of the book in the form of unbound printed sheets, interleaved with manuscript sheets and letters to and from Richardson. The manuscript sheets are for the most part rough notes stimulated by correspondence and publications subsequent to 1922. Probably the bulk of this material is on radiation and turbulence. Most of the letters relate directly to the book: several from Shaw and from Jeffreys, a few from Dines, one from Whipple, and some others. The earliest entry (on the "Whistle of shell from French 75 mm gun") is dated June 1, 1918; the latest (a reference to experimental work on skin friction) was written in 1951. On one of the manuscript sheets, undated, Richardson records his intention of writing a new chapter on "The arithmetical handling of discontinuities," apparently having in mind the idea of accommodating his numerical scheme to the recently-advanced polar-front model. Although not an adequate basis for a second edition, the 'revision' file is a revealing window to the meteorological side of Richardson's life. I shall refer to it again.

In writing about such a singular book, published 45 years ago on a subject that in the past decade has evolved with astonishing rapidity, it is natural to meet many questions that touch upon the history of this subject. Although I do not claim special competence in historical matters, I have not resisted the temptation to speak about some of these questions, and about a few of the men whose work was related to Richardson's in an important way. This I have done mainly in Sections

p. 9, equation 8. For $\cot \phi$ read $\frac{\cot \phi}{a}$.

p. 42, equation 13. The value of a_μ is wrong. Please refer to pp. 158 to 162, especially to p. 161.

p. 77, equation 22. For $-\frac{2\kappa\pi^2}{r^3}\Theta$ read $+\frac{2\kappa\pi^2}{r^3}\Theta$.

p. 77, footnote. After "a wrong sign" insert the words "before the second term of (5·2) and (5·3) and."

p. 89. Please refer to Ch. 8/2/15 beginning on p. 171.

p. 136, equation 9. The last form should read

$$\frac{\partial}{a\partial\phi} \left(\frac{1}{\sin \phi} \right) = -\frac{1}{a} \frac{\cot \phi}{\sin \phi}.$$

p. 136, equation 8. Add to the second member the term

$$-\frac{\tan \phi}{a} \frac{g}{2\omega \sin \phi} \cdot \frac{\partial \log \theta}{\partial e} \left(h - h_2 - \frac{b\theta}{g} \right).$$

p. 137, equation 10. Change the sign of the last form of the second member.

The two foregoing corrections in Ch. 6/6 #8 and 10 imply that the expression $\frac{\tan \phi + \cot \phi}{a}$ wherever it occurs in Ch. 6 should be replaced by $\frac{\cot \phi}{a}$. This change is necessary in Ch. 6/6 #11, 17, 18, 22, 23; in Ch. 6/7/2 #15A, 18 and again on the computing form P XIV on page 201.

p. 138, equation 22. Change the sign in front of $\frac{\partial \log \theta}{\partial e} \cdot \frac{\partial \log p_2}{\partial n}$ from plus to minus.

FIG. 1. "Additional errata" reproduced from the original edition of Richardson's book, not incorporated in the Dover reprint.

7 and 8. In Sections 4, 5, and 6 my aim is to give a technical analysis of the numerical and dynamical aspects of Richardson's prediction model, and of his forecast. These sections were written with the non-specialist in mind; but they may be skipped if the reader wishes to have only the flavor of this essay.

The notes consolidated at the end of the text are partly bibliographic and partly in the nature of commentary. Mainly, however, I have used these notes as an outlet for much unpublished material acquired in the course of this study. Included are excerpts from the 'revision' file, and letters I received from persons who had some direct involvement with Richardson.

The reprint before us is a fine example of the high standards maintained by Dover Publications in this field; its low price of \$2.00 is a strong inducement to own a copy. An added inducement is the new introduction, written especially for the Dover edition by Dr. Sydney Chapman. The principal defect I noticed in the reprint concerns the errata of the original edition.² There are two sets: one headed "Errata" on the bottom of p. xii (original edition); the other headed "Additional errata," tipped in between p. xii and p. 1. The reprint incorporates the former directly in the text, by correction of the original sheets. However, the "Additional errata" are not corrected in the reprint, and since these are the more important of the two lists, I have obtained permission of Cambridge University Press to reproduce them here (Fig. 1).³

Dr. Kenneth C. Spengler, Executive Director of the American Meteorological Society and the BULLETIN Editor, graciously consented to publish this essay; I should like to record my appreciation for the privilege thus accorded me. From Mr. Oliver M. Ashford I received much valuable information. I am especially indebted to him for the opportunity to examine the 'revision' file, and for permission to quote from this material. Dr. Stephen A. Richardson, a son of the author of our subject, searched for suitable photos of his camera-shy father and was most helpful in other ways as well. The portrait was provided by his brother Mr. Olaf K. M. Richardson, and the group photograph by his sister Mrs. Elaine D. Traylen. The very gracious cooperation of the Richardson family has been a pleasant experience for me. I also am grateful to Prof. Henry Stommel for his letter quoted in Note 4, to Sir Harold Jeffreys for his letter quoted in Note 10, to Prof. C. L. Godske for his letter quoted in Note 16, to Prof. Arnt Eliassen for information included in Note 22 and for illuminating remarks about some aspects of this essay, to Prof. J. Bjerknes for his letter quoted in Note 27, to Dr. Frederick G. Shuman for important corrections to the draft of Section 4, and to Janet F. Rice of Cambridge University Press for drawing my attention to some of the reviews that I have cited. Prof. Norman A. Phillips very kindly read the manuscript.

Cambridge University Press and Dover Publications gave permission to quote many passages from the book,

and to reproduce several illustrations. Boxwood Press gave permission to quote from *Arms and Insecurity*. The portraits of Bjerknes, Margules, and Dines are from the book by Neis (1956).

1. Prologue

A few years before Richardson's death in 1953, the following portrayal was written by O. M. Ashford (1949):

Dr. Richardson, who is a member of the well-known Quaker family, was born at Newcastle-on-Tyne in 1881. He finished his schooling at Bootham School, York, which is famous for its encouragement of leisure-hour pursuits; all prizes are awarded for work done outside the classroom, and it claims to have the oldest school Natural History Club in the world. From there he passed on to Durham College of Science and then to King's College, Cambridge, where he took his B.A. Natural Science Tripos, 1st Class, Part I, in 1903. Before joining the Meteorological Office, L. F. Richardson worked for three years at the National Physical Laboratory, and also spent some years at a tungsten lamp factory.

In 1913 he was appointed Superintendent of Eskdalemuir Geophysical Observatory, where he started work on the most absorbing problem of forecasting the weather by rigid calculation. He remained there until joining the Friends' Ambulance Unit, along with many other Quakers and pacifists, in 1916. During his two and a half years' service in France his thoughts frequently returned to his researches, and he revised the first draft of his book on weather forecasting. On returning to civil life, Dr. Richardson was posted to Benson Observatory, then at the height of its fame under W. H. Dines, where he was able to carry on with his researches. It must have been a blow to his colleagues when he decided, for pacifist reasons, to resign from the Meteorological Office on its being taken over by the Air Ministry in 1920. From then until his retirement in 1940, he worked in the education world, first at Westminster Training College and latterly as Principal of Paisley Technical College. In spite of the onerous duties of his profession, he at first found time to pursue his meteorological investigations. His magnum opus, *Weather Prediction by Numerical Process*, was eventually published in 1922 and was followed by many other important papers.

As might be expected in a man of Quaker upbringing, Dr. Richardson often wondered how his abilities could best be used to serve mankind, and he gradually turned away from meteorology to pioneer a completely new subject, a mathematical and psychological analysis of the causes of war. How rare it must be for a scientist honoured with a D.Sc. and F.R.S. (in 1926) to start studying again and take a B.Sc. in psychology (in 1929)! His new researches absorbed more and more of his time, and one of the main reasons for his early retirement was to enable him to devote all his energies to his task. From then on meteorology was regarded as a temptation to be resisted, so that his thoughts could be concentrated on his service to world peace. *Generalized Foreign Politics*, published in 1939, showed how this apparently abstruse mathematical work could produce results of great practical value, and it is greatly regretted that the revised and extended edition, *Arms and Insecurity*, is so far only available in microfilm.

A more detailed account of Richardson's life and work is given in the obituary by Gold (1954), to which a complete list of publications is appended. (*Arms and Insecurity*, mentioned above, and a later book *Statistics of Deadly Quarrels* were available only on microfilm at



Lewis F. Richardson, 1881–1953

the time of Ashford's note and Gold's obituary; they have since been published.*)

Richardson's unswerving opposition to war and his active dedication to peace had a profound influence on his career. In a moving tribute entitled "A Quaker Scientist" written shortly after his death, Richardson's wife tells us that "there came a time of heart-break when those most interested in his "upper air" researches proved to be the "poison gas" experts. Lewis stopped his meteorological researches, destroying such as had not been published. What this cost him none will ever know!" Between 1930 and 1948 Richardson published no meteorological papers.⁴

Of the total of 85 publications by Richardson in various fields, about half are in psychology—mainly on the dynamics and statistics of war—and one-third are in meteorology. Of the latter the most consistent theme is turbulence and diffusion, on which there are 12 papers, the first in 1919, the last in 1952. It is plain from the book and from his subsequent work that Richardson regarded an understanding of atmospheric turbulence as an important aspect of numerical weather prediction.

The circumstances under which the book was written, and the length of time between its conception and publication, are explained by Richardson in the final

paragraph of the Preface:

This investigation grew out of a study of finite differences and first took shape in 1911 as the fantasy which is now relegated to Ch. 11/2. Serious attention to the problem was begun in 1913 at Eskdalemuir Observatory with the permission and encouragement of Sir Napier Shaw, then Director of the Meteorological Office, to whom I am greatly indebted for facilities, information and ideas. I wish to thank Mr W. H. Dines, F.R.S., for his interest in some early arithmetical experiments, and Dr A. Crichton Mitchell, F.R.S.E., for some criticisms of the first draft. The arithmetical reduction of the balloon, and other observations, was done with much help from my wife. In May 1916 the manuscript was communicated by Sir Napier Shaw to the Royal Society, which generously voted £100 towards the cost of its publication. The manuscript was revised and the detailed example of Ch. IX was worked out in France in the intervals of transporting wounded in 1916–1918. During the battle of Champagne in April 1917 the working copy was sent to the rear, where it became lost, to be rediscovered some months later under a heap of coal. In 1919, as printing was delayed by the legacy of the war, various excrescences were removed for separate publication, and an introductory example was added. This was done at Benson, where I had again the good fortune to be able to discuss the hypotheses with Mr W. H. Dines. The whole work has been thoroughly revised in 1920, 1921. As the cost of printing had by this time much increased, an application was made to Dr G. C. Simpson, F.R.S., for a further grant in aid, and the sum of fifty pounds was provided by the Meteorological Office. For the construction of the index we are indebted to Mr M. A. Giblett, M.Sc. The discernment and accuracy with which the Cambridge Press have set the type have been constant sources of satisfaction.

It is remarkable that (as Dr. Chapman observes in his introduction to the Dover edition) Richardson had the fortitude to carry on this difficult scientific work despite the depressing and distracting circumstances of ambulance service in France.⁵

2. Summary of the book

Roughly 120 pages, about half the book's total of 236 pages, are taken up by what loosely may be termed dynamics, of which about 45 pages are concerned with turbulent transport. The major parts of the rest of the book are: thermodynamics (30 pages), radiation (25 pages), and numerical analysis (20 pages). In addition there are treatments of subsurface processes (15 pages) and water transport (10 pages). Of the 75 pages that deal with dynamics exclusive of turbulent transport, about one third is given to formulation of equations for the stratosphere and one third to a miscellany which includes review of the various steps of computation, arrangement of the initial data, the all-important computing forms, discussions of forecast errors, smoothing of initial data, and "some remaining problems." The other third contains the formulation of the main momentum equations (5 pages) and of the vertical-velocity equation (10 pages).

Richardson's own summary of this heterogeneous material can hardly be improved upon (Chapter 1):

* By Boxwood Press, P. O. Box 7171, Pittsburgh, Pa. 15213.

Finite arithmetical differences have proved remarkably successful in dealing with differential equations; for instance, approximate particular solutions of the equation for the diffusion of heat . . . can be obtained quite simply and without any need to bring in Fourier analysis. An example is worked out in a paper published in *Phil. Trans. A*, Vol. 210 . . . In this book it is shown that similar methods can be extended to the very complicated system of differential equations, which expresses the changes in the weather. The fundamental idea is that atmospheric pressures, velocities, etc. should be expressed as numbers, and should be tabulated at certain latitudes, longitudes and heights, so as to give a general account of the state of the atmosphere at any instant, over an extended region, up to a height of say 20 kilometres. The numbers in this table are supposed to be given, at a certain initial instant, by means of observations.

It is shown that there is an arithmetical method of operating upon these tabulated numbers, so as to obtain a new table representing approximately the subsequent state of the atmosphere after a brief interval of time, δt say. The process can be repeated so as to yield the state of the atmosphere after $2\delta t$, $3\delta t$, and so on. There is a limit however to the possible number of repetitions, because each table is found to be smaller than its predecessor, in longitude and latitude, having lost a strip round its edge. Only if the table included the whole globe could the repetitions be endless. Also the errors increase with the number of steps.

In Ch. 2 the working of the method is shown by its application to a specially simplified case. In Ch. 3 the coordinate differences are considered in relation to the average size of European cyclones, and the following differences are provisionally selected: in time 6 hours, in longitude the distances between 128 equally spaced meridians, in latitude 200 kilometres of the earth's circumference, and in height the intervals between fixed heights nearly corresponding to the normal pressures of 8, 6, 4, 2 decibars. Thus small-scale phenomena, such as local thunderstorms, have to be smoothed out.

In Ch. 4 the fundamental equations are collected from various sources, set in order and completed where necessary. Those for the atmosphere are then integrated with respect to height so as to make them apply to the mean values of the pressure, density, velocity, etc., in the several conventional strata. Incidentally certain constants relating to friction and to radiation are collected from observational data. It is found to be necessary to eliminate the vertical velocity from all the equations, and in Ch. 5 it is shown how this can be done. Special difficulties arise in connection with the uppermost stratum on account of its great thickness and the enormous ratio of density between its upper and lower surfaces. These difficulties are removed in Ch. 6, as far as high latitudes are concerned. In particular it is shown how the total mass transport above any level may be deduced from a pilot balloon observation which extends well into the stratosphere.

In Ch. 7 the arrangement of the tabular numbers in space and time is discussed with a view to securing the best representation of differential coefficients by difference ratios.

In Ch. 8 the whole system of arithmetical operations is reviewed in order. With regard to the horizontal differential coefficients the general method may be briefly described in the following four sentences: Take the differential equations and replace everywhere the infinitesimal operator . . . by the finite difference operator . . . Use arithmetic instead of symbols. Attend carefully to the centering of the differences. Leave the errors due to the finiteness of the differences over for consideration at the end of the process. With regard to the vertical differential coefficients, on the contrary, it is often possible to effect an exact transformation to differences, by means of a vertical integration. In arranging the computing, it has constantly to be borne in mind that the rate of change with time of every one of the discrete values of the dependent

variables must be calculable from their instantaneous distribution in time and space, excepting only those values near the edge of the horizontal area represented in the table. We may refer to this necessary property by saying, for brevity, that the system must be "lattice-reproducing."

In Ch. 9 will be found an arithmetical table showing the state of the atmosphere observed over middle Europe at 1910 May 20d. 7h. g.m.t. This region and instant were chosen because the observations form the most complete set known to me at the time of writing, and also because V. Bjerknes has published large scale charts of the isobaric surfaces, together with collated data for wind, cloud and precipitation. Starting from the table of the initially observed state of the atmosphere at this instant, the method described in the preceding paragraphs is applied, and so the rates of change of the pressures, winds, temperatures, etc. are obtained. Unfortunately this "forecast" is spoilt by errors in the initial data for winds. These errors appear to arise mainly from the irregular distribution of pilot balloon stations, and from their too small number.

In Ch. 10 the smoothing of initial observations is discussed.

In Ch. 11 is a collection of problems still waiting to be solved, with some suggestions for their treatment.

Ch. 12 is a list of Notation.

Richardson concludes his summary with an explanation of what is meant by "marching" and "jury" problems (the picturesque terms he introduced in earlier work on numerical solution of differential equations), and some remarks on the advantages of centered differences.

The organization in chapters and sections follows an orderly design which culminates in Chapter 9, "An example worked on computing forms." In a very real sense these remarkable forms are the quintessence of the book.⁶ (They are equivalent to what we now call a 'program' in the language of computer technology. One of them is reproduced below as Fig. 7.) There are twenty-three, "divided into two groups marked *P* or *M* according as the point on the map to which they refer is one where pressures (*P*) or momenta (*M*) are tabulated." (p. 186.) The first three forms contain some of the input data, and constants relating to thermodynamics and turbulence. The various calculations involved in the forecast are presented in the other 20 forms, which may be classified as follows:

Thermodynamic and hydrologic calculations (12 forms)

Radiation: P IV–VI

Ground surface and subsurface: P VII–X, XVIII, XIX

Free air: P XI, XII, XVII

Hydrodynamic calculations (8 forms)

Mass tendency and pressure tendency: P XIII

Vertical velocity: P XIV–XVI

Momentum tendency: M I–IV

It should be said that the results of the thermodynamic and hydrologic calculations have only a minor effect on the hydrodynamic calculations.

The book's greatest defect of style is proliferation of subsidiary detail without a sustained effort to build in the reader's mind a clear image of the central frame-

work of ideas. For this reason it is difficult to read in large doses. On the other hand, Richardson's literary and mathematical styles are very clear and leave little to be desired in rendering the substance of the book easily comprehensible. Moreover, the reader is rewarded not only by the scientific value of the work, but also occasionally by a refreshing departure from the otherwise stern tone. These diversions, also found in Richardson's other scientific writing, seem to reflect a blend of finely wrought humor and directness of manner. Some examples from the book will give their flavor. From Chapter 11 (p. 220):

It is conceivable that by a change of variables the equations could be much shortened. But as we are always required in the end to arrive at quantities of direct interest to the public, namely wind, rain, temperature and radiation, so it may be that analytical simplicity does not simplify the arithmetic. There is a tale of a philosopher who succeeded in reducing the whole of physics to a single equation $H=0$, but the explanation of the meaning of H occupied twelve fat volumes.

In speaking about the daytime increase of wind speed at anemometer level (p. 85): "One gets the impression that the upper wind planes away the night calm a shaving at a time." Again, the need for more experimental work on the absorption spectrum of the atmosphere for long-wave radiation evokes (p. 49): "In the meantime meteorologists must carry on business on premises which are, so to speak, in the hands of the builders." What is probably the best-known bit of pleasantry comes in a section on eddy motion, following an observation that small scales of turbulence accompany cumulus convection (p. 66):

One gets a similar impression when making a drawing of a rising cumulus from a fixed point; the details change before the sketch can be completed. We realize thus that: big whirls have little whirls that feed on their velocity, and little whirls have lesser whirls and so on to viscosity—in the molecular sense.

(The final phrase, often omitted in quotation, gives this couplet a Richardsonian flavor!)

Near the end of the book we find that famous and delightful fantasy "The speed and organization of computing," which stands as a banner to Richardson's audacious spirit (pp. 219–220):

It took me the best part of six weeks to draw up the computing forms and to work out the new distribution in two vertical columns for the first time. My office was a heap of hay in a cold rest billet. With practice the work of an average computer might go perhaps ten times faster. If the time-step were 3 hours, then 32 individuals could just compute two points so as to keep pace with the weather, if we allow nothing for the very great gain in speed which is invariably noticed when a complicated operation is divided up into simpler parts, upon which individuals specialize. If the co-ordinate chequer were 200 km square in plan, there would be 3200 columns on the complete map of the globe. In the tropics the weather is often foreknown, so that we may say 2000 active columns. So that $32 \times 2000 = 64,000$ computers would be needed to race the weather for the whole globe. That is a staggering figure. Perhaps in some years' time it

may be possible to report a simplification of the process. But in any case, the organization indicated is a central forecast-factory for the whole globe, or for portions extending to boundaries where the weather is steady, with individual computers specializing on the separate equations. Let us hope for their sakes that they are moved on from time to time to new operations.

After so much hard reasoning, may one play with a fantasy? Imagine a large hall like a theatre, except that the circles and galleries go right round through the space usually occupied by the stage. The walls of this chamber are painted to form a map of the globe. The ceiling represents the north polar regions, England is in the gallery, the tropics in the upper circle, Australia on the dress circle and the antarctic in the pit. A myriad computers are at work upon the weather of the part of the map where each sits, but each computer attends only to one equation or part of an equation. The work of each region is coordinated by an official of higher rank. Numerous little "night signs" display the instantaneous values so that neighbouring computers can read them. Each number is thus displayed in three adjacent zones so as to maintain communication to the North and South on the map. From the floor of the pit a tall pillar rises to half the height of the hall. It carries a large pulpit on its top. In this sits the man in charge of the whole theatre; he is surrounded by several assistants and messengers. One of his duties is to maintain a uniform speed of progress in all parts of the globe. In this respect he is like the conductor of an orchestra in which the instruments are slide-rules and calculating machines. But instead of waving a baton he turns a beam of rosy light upon any region that is running ahead of the rest, and a beam of blue light upon those who are behindhand.

Four senior clerks in the central pulpit are collecting the future weather as fast as it is being computed, and despatching it by pneumatic carrier to a quiet room. There it will be coded and telephoned to the radio transmitting station.

Messengers carry piles of used computing forms down to a storehouse in the cellar.

In a neighbouring building there is a research department, where they invent improvements. But there is much experimenting on a small scale before any change is made in the complex routine of the computing theatre. In a basement an enthusiast is observing eddies in the liquid lining of a huge spinning bowl, but so far the arithmetic proves the better way. In another building are all the usual financial, correspondence and administrative offices. Outside are playing fields, houses, mountains and lakes, for it was thought that those who compute the weather should breathe of it freely.

3. Contemporary opinion

The restraint of saying only what is truly germane to the subject in view is not much in evidence in Richardson's book, although it was by all accounts a conspicuous trait of its author's personality. This paradox is resolved by Dr. Chapman who, in his illuminating introduction to the Dover republication, remarks "He [Richardson] once told me that he had put all he knew into the book." Although often fascinating, the encumbering maze of detail tends to make the book impregnable except to the most determined assault. Indeed, matters might have been worse, for in the Preface Richardson says "In 1919, as printing was delayed by the legacy of the war, various excrescences were removed for separate publication . . .!"

Felix M. Exner, professor of geophysics at the University of Vienna, gives us some insight on the response

of a contemporary mind toward Richardson's book. His conclusion is (Exner, 1923) (translation):⁷

. . . One will succeed only gradually in extracting new ideas from this work, not only because the reading is difficult, but also because it contains too much heterogeneous material to be satisfactory. It is the opinion of the reviewer that the author would have rendered a better service to meteorology if he had set forth his basic studies of the separate processes independently of each other. A very valuable theoretical work would have been the result, whereas there will be only a few readers who will work through the book in its present form, for it carries too much of the stamp of personal intent. Whoever is convinced at the outset—and this will be the case with the majority of meteorologists—that the way to weather forecasting mapped out by Richardson is wrong or at least is very premature, will hardly muster the patience to study this work.

It would be very advisable, therefore, for the author either to make generally known in separate papers what is new in his calculations, or—still better—to publish a book on theoretical meteorology, which is free of the design of an immediate application to prediction.

These are not lenient remarks but they may strike a responsive chord if one has set out to read the book carefully!

An even more adverse review came from the other side of the Atlantic. Alexander McAdie, professor of meteorology at Harvard University, said "It can have but a limited number of readers and will probably be quickly placed upon a library shelf and allowed to rest undisturbed by most of those who purchase a copy." His further unsparing comments are (McAdie, 1923):

There is, however, a fatal weakness in the scheme in that local disturbances, or in the author's phrase "small-scale phenomena," must be smoothed out. But in our daily weather it is the small-scale phenomenon, or departure from the general flow, which works havoc with the forecasts.

Also in the integration of the fundamental equations, mean values are used. Now most observers know such values are not even the most frequent values.

The author excuses the failure of his method when applied in the one forecast made, by saying that it is "spoilt by errors in the initial data for winds." He believes that the pilot balloon stations were too irregularly distributed and too few. Such a confession, however, gives aid and comfort to the professional forecaster of today; and he will continue to rely upon surface conditions.

There are many signs and symbols and a much-needed page of guiding signs. Energy is expressed in ergs, not calories. The final chapter on "Units and Notation" will try the patience of most readers. A number of Coptic letters are introduced, lower case and upper case English and Greek alphabets plus all known mathematical signs not being sufficient to meet the author's need.

The uneasiness about Richardson having smoothed out small-scale phenomena was voiced by several reviewers, and while understandable from the viewpoint of the practicing forecaster, is hardly in accord with the fact—which surely was known even then—that such phenomena are strongly correlated with the large-scale circulation, the prediction of which must therefore be the necessary first step to forecasting the weather.

One of the most wholehearted contemporary endorsements came from Edgar W. Woolard (1922), then meteorologist in the U. S. Weather Bureau:

This book is an admirable study of an eminently important problem; being a first attempt in this extraordinarily difficult and complex field, it necessarily possesses, self-confessedly, many imperfections; however, it indicates a line of attack on the problem and invites further study with a view to improvement and extension. There are listed a number of problems still awaiting investigation. Perhaps the most serious handicap to studies in this field is the lack of adequate observational material. So far as the purely mathematical difficulties which the complexity of the subject introduces are concerned, they are surmountable. . . . It is sincerely to be hoped that the author will continue his excellent work along these lines, and that other investigators will be attracted to the field which he has opened up. The results can not fail to be of direct practical importance as well as of immense scientific value.

Dr. Woolard accurately assessed the path-breaking significance of the book. Unfortunately, Richardson himself did not continue to work along these lines, nor were other investigators attracted to the field he opened up.⁸

The English reviewers were generally favorable to Richardson's book, and because of the bold originality of the work were inclined to overlook the catastrophically bad forecast.⁹ Sydney Chapman, whose new Introduction noted above was written in 1965, contributed 43 years earlier what might be called one of the "official" contemporary reviews. Dr. Chapman (1922), then professor of mathematics at Manchester University, began in this way:

The enterprise contemplated in this book is of almost quixotic boldness. In its full scope it involves a central meteorological bureau for the world as a whole, having several hundred departments—working under a co-ordinating staff—each occupied with the meteorology of a particular section of the atmosphere. Each continually forecasts the weather of its own region, by numerical calculation applied to past data for the region, and to other data concurrently supplied by the departments for the adjoining regions. Predictions are issued without need for new incoming observations, because the course of the weather depends on its past history, and when this is adequately known the rest is theoretically calculable.

This the author calls a fantasy, and it is relegated to a concluding chapter; but it has been his inspiration, and is, after all, only a particular case of what natural science in general is striving towards; for it seems likely that the whole of physics and chemistry, with all their ramifications, will in time be reduced to mathematics, enabling the entire future course of material events to be predicted if only our powers of observation and calculation were equal to the task. The meteorological fraction of this great whole is itself stupendous, as the author recognizes, and is subject to the same doubt as to our ability ever to realize the dream. "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" (Preface). That the finance of the scheme would be on an enormous scale may be judged from the author's estimate that over sixty thousand computers would be necessary.

In conclusion, Dr. Chapman said:

Perhaps the best immediate outcome that can be hoped for at the present is that Mr. Richardson's discussion may help towards the solution of atmospheric problems by assisting direct research (after the event) into specially simple or peculiar states of weather. It may also help by calling attention to the kind of observations most needed for theoretical studies. The book is in any case one of great interest and individuality and full of evidences of a mind widely versed and keenly interested in every phase of meteorological activity.

Looking backward, it must be admitted that neither of the outcomes hoped for by Dr. Chapman materialized. Even to the present day the disposition of observing stations has been remarkably impervious to the implications of theoretical studies; and I am not aware that research in the more restricted aspects of weather has been much influenced by Richardson's book.

Another appraisal was made by Harold Jeffreys, then (as now) Fellow of St. John's College, Cambridge.¹⁰ Dr. Jeffreys (1922) gave his reaction as follows:

The method is one that appeals strongly to the mathematical physicist. It is necessarily laborious in its present form, and probably could not be worked with sufficient speed to make it a practical method of forecasting; but when forecasters have acquired experience in its use, they will probably find that a sufficient number of the quantities allowed for are comparatively small to make it possible to expedite the calculation considerably without great sacrifice of accuracy.

The value of the work is not confined to the application to forecasting, though the possibility of predicting the disturbing occasions when cyclones cause merriment in the daily press by moving in the wrong direction makes this the feature of most general interest. Its discussion of the physical properties of the atmosphere is so thorough that it constitutes a text-book of the subject. Copious references to original literature are given, and any meteorologist requiring serious information on any topic will do well to look first in this book.

Dr. Jeffreys does not guess how long it might take for the forecaster to acquire the experience needed to expedite the calculation, but a reviewer writing anonymously in the *Manchester Guardian* (March 28, 1922) suggested: ". . . it seems highly probable that the Meteorological Office of a hundred years hence will contain a forecasting section run on lines very much like those imagined by Mr. L. F. Richardson . . ."

F. J. W. Whipple (1922), Superintendent of Climatology in the Meteorological Office, wrote:

Before he went to Eskdalemuir and turned his attention to meteorology, Mr. Richardson had devoted much attention to the solution of problems in applied mathematics by numerical processes. The mathematician when confronted with a physical problem usually simplifies the conditions so as to make it amenable to his analytical machinery. The sledge-hammer method developed by Mr. Richardson requires no such preliminary paring down of awkward corners: it demands, however, conscientious attention to detail and no shirking of laborious arithmetic.

. . . even if the scheme were perfected, the really interesting weather would not be forecasted, for thunderstorms and tornadoes as well as the secondary features of the cyclones

of the temperate zone would be "smoothed out" in the highly generalized specification of the meteorological situation.

In view of this criticism, which it is fair to say, is put forward by Mr. Richardson himself, it hardly matters that the one forecast six hours ahead at one place, which he has computed, is sadly in error.

The trouble which he meets is that quite small discrepancies in the estimate of the strengths of the winds may lead to comparatively large errors in the computed changes of pressure. It is very doubtful whether sufficiently accurate results will ever be arrived at by the straightforward application of the principle of the conservation of matter. In nature any excess of air in one place originates waves which are propagated with the velocity of sound, and therefore much faster than ordinary meteorological phenomena.

One of the difficulties in the mathematical analysis of pressure changes on the earth is that the great rapidity of these adjustments by the elasticity of the air has to be allowed for. The difficulty does not crop up explicitly in Mr. Richardson's work, but it may contribute to the failure of his method when he comes to close quarters with a numerical problem.

The value of the book is not, however, to be judged by this failure. Its merit is that it insists on the study of all the various ways in which the meteorological elements act and re-act on each other. This co-ordination of knowledge has been the stimulus to many special researches valuable in themselves but appearing now, for the first time, in their proper relations.

. . . .

Whether our methods of forecasting are gradually developed from the present practice, or whether an entirely new system, such as the author's, is invented, he has done good service by helping meteorologists to obtain a clearer insight into the operation of the forces with which they deal.

I am inclined to think that Whipple intended his reference to Richardson's "sledge-hammer method" not as a demurrer to the efficacy of numerical integration, but rather as a mild rebuke for the retention of minor details (such as the thermal effect of a layer of dead leaves on the ground! . . . p. 195). The most penetrating remarks in his review are those concerning the generation and propagation of high-speed waves. Although sound waves are excluded from Richardson's equations (because he uses only the hydrostatic pressure), gravity waves are not excluded, and these can indeed be propagated with a velocity "much faster than ordinary meteorological phenomena." Whipple must be credited with astonishing perception when he suggests that this could endanger the success of a calculation with Richardson's model.

Richardson did the preliminary work on weather prediction in the years 1913–16 when he was Superintendent of Eskdalemuir Observatory, and the final work in 1919–20 at Benson Observatory, where the director was W. H. Dines. This work was carried on, according to Richardson (Preface) ". . . with the permission and encouragement of Sir Napier Shaw, then Director of the Meteorological Office, to whom I am greatly indebted for facilities, information and ideas." In view of his support of Richardson's endeavors, the review written by Shaw (1922), then professor at the Royal College of Science, is of particular interest. It is typical of the best of Shaw's garrulous style, replete with maxims, morals, and musings.



June 21, 1913: Superintendent-elect L. F. Richardson is introduced to local notables at a garden party at Eskdalemuir Observatory. This striking photograph was taken in front of the Superintendent's residence, now called Rayleigh House. Its publication here was made possible by the kindness of Richardson's daughter Mrs. Elaine D. Traylen, who says of it: "Lewis R. is seated in the centre of the front row in a typically casual attitude, light coloured suit, no hat, Dorothy, is immediately behind him, no hat! The imposing lady centre is my maternal grandmother, Rebecca Garnett. Dr. William Garnett extreme left centre with beard and specs." Seated immediately to Richardson's right is Dr. J. A. Harker, the outgoing Superintendent. The young man standing third from our left is L. H. G. Dines, son of William H. Dines and assistant for seismology on the staff of the Observatory. Among the visitors on this occasion is one whose name appears frequently in this essay: Sir Napier Shaw (Director of the Meteorological Office), third man from our right in the front row. Mrs. Shaw is seated directly behind Dr. Harker. (I am indebted to Mr. L. Jacobs of the Meteorological Office for a complete account of this photograph.) William Garnett, Richardson's father-in-law identified above by Mrs. Traylen was demonstrator for Maxwell at the Cavendish Laboratory, 1874-79. Shaw had also come under Maxwell's influence in the same period, as a Cambridge Fellow, and (with Glazebrook) succeeded Garnett as demonstrator.

The trial specimen is not such a good example of the art of forecasting that it tempts the reader forthwith to become one of the great orchestra. The change of pressure at the surface works out at 145 millibars in six hours. Our barometers allow for a range of 100 millibars at most; and, as a matter of observation, the change in the region in question was less than a millibar: the wildest guess, therefore, at the change in this particular element would not have been wider of the mark than the laborious calculation of six weeks. Nor is that all. Many of the chapters end in parenthetic expessions of regret or of suggestions for improvement. There are also many supplementary paragraphs which indicate that when the author comes to make another edition, as he or some one else undoubtedly will, he will write somewhat differently. And the reader will not be sorry, for in many ways the book makes hard reading. It is full of mathematical reasoning, a good deal of which is conducted "by reference." The reader who wishes to follow it must have a very handsome library and a few stepladders which Mr. Richardson does not provide.

A reviewer with less than the ordinary sufferance of his tribe might easily murmur: forecasting by numerical process seems so arduous and so disappointing in the first attempts that the result is a sense of warning rather than attraction. He might also wonder for whom the author is writing, and regard the book as a soliloquy on the scientific stage. The scenes are too mathematical for the ordinary meteorologist to take part in and too meteorological for the ordinary mathematician. But such complaint would be as misleading as the computed forecast. On the road to forecasting by numerical process nearly every physical and dynamical process of the atmosphere has to be scrutinised and evaluated; the loss of view into the future from the first summit is compensated many times by the insight which one gets into the working of Nature on the way. For example, the author draws from the miss of his forecast the conclusion that the observations of velocity used are a real source of error. Whether that conclusion is true or not, its further consideration is of the greatest importance in view of the multiplicity of observations of winds in the upper air and of the difficulties which their interpretation presents.

This strikes me as a gracefully tolerant attitude for one who gave official support to a long and arduous project which culminated in a forecast characterized by its author as a "glaring error" (p. 187). Shaw's final remarks are:

. . . The principle which lies at the bottom of his treatment of the subject is that the known laws of dynamics and physics as applied to the changes which take place are inexorable and are sufficient. The future can therefore be derived from the present by their application. They can be applied by the step by step method of finite differences with sufficient accuracy to obtain the general consequences of the present conditions. The illustration of the process is a most valuable contribution to meteorology and indicates a wholesome course of practical physics and dynamics of the atmosphere which may prove the basis of future teaching. Thus it will not only provide an acid test of meteorological theory but also be a valuable guide to the organisation of new meteorological observations.

Finally, perhaps the most important aspect of this contribution to meteorological literature is that a rigorous differential equation is not necessarily useless because it cannot be integrated algebraically. It opens the way to useful exercises less stupendous than calculating the weather, and indeed, whenever meteorology comes to be taught and learned, the book will be a rich quarry for the teacher and examiner.

From this account of contemporary opinion I think it is clear that Richardson's book was received with lively

interest by prominent meteorologists of the day. Its failure to achieve the degree of influence merited by the importance of the subject can not be ascribed to inadequate publicity!¹¹

4. The prediction model

In April 1955, less than two years after Richardson's death, the U. S. Weather Bureau began to issue numerical forecasts on an operational basis by means of an electronic computer, at the National Meteorological Center (NMC), near Washington, D. C. The physical characterization of the forecast problem and the corresponding mathematical equations—in a word, the 'model'—used at NMC has been until recently of the 'quasi-geostrophic' type, following a line of development begun by Charney *et al.* (1950). Such models do not admit gravity waves, and therefore differ significantly from Richardson's. However, in June 1966 the NMC put into operational use a 'primitive-equation' model designed by Shuman and Hovermale (1967), which is physically and in some respects mathematically equivalent to Richardson's, when viewed with sufficient generality. I do not mean that there was a conscious attempt to imitate Richardson's model, or even to build upon his work. On the contrary, the modern primitive-equation models for numerical weather prediction have evolved in a systematic way from quasi-geostrophic models, and incorporate procedures unknown in Richardson's day.

Partly for this reason I have found it interesting to make a rough comparison between the two models. This comparison will be facilitated if we use, as a bridge, a model designed by Eliassen (1949) which is a progenitor of the NMC model. Common to the Richardson and Eliassen models are five meteorological fields carried forward in the forecast. Each field is represented by a particular variable, as specified in Table 1. Richardson used height as a vertical coordinate whereas Eliassen used pressure: this accounts for the differences in choice of variables to represent the *P*- and *W*-fields. There also are differences in representation of *R* and *U*, *V* but these are minor. The point I wish to emphasize is that the two schemes involve the same five *types* of fields. (Richardson also included a moisture field, which I ignore in this discussion.)

TABLE 1. Prediction fields of the Richardson and Eliassen models

Symbol	Type of field	Corresponding variables used by	
		Richardson	Eliassen
<i>P</i>	pressure	pressure	height
<i>W</i>	vertical motion	total variation of height with time	total variation of pressure with time
<i>R</i>	mass	density	entropy
<i>U</i> , <i>V</i>	horizontal motion	horizontal momentum	horizontal velocity

Five 'primitive' equations are the starting point for both the Richardson and Eliassen models:

1. the hydrostatic equation,
2. the continuity equation,
3. the thermal-energy equation,
4. the two horizontal-momentum equations.

These are used to obtain five equations that govern the evolution of the five fields. The 'governing' equations have the same structure in the two models, as shown in Table 2. Moreover, the two schemes use the same primitive equations for P and U, V . However, there are differences in origin of their respective governing equations for W and R , one result of which is that sources of heat—such as from condensation of water vapor and from radiation—appear in the W -equation of Richardson's model and in the R -equation of Eliassen's. The W -equation is 'non-primitive' in both schemes.

TABLE 2. Governing equations of the Richardson and Eliassen models.

Structure of governing equation		Relation to primitive equations†	
Change of	Determined by	Richardson	Eliassen
P in vertical	R	1	1
W in vertical	U, V (*)	1, 2, 3	1, 2
R with time	R, U, V, W	2	3
U, V with time	P, U, V, W	4	4

* Also R and P in Richardson's equation.

† See list in text.

The homology between the two models is most evident from the structure of the governing equations. Each of these equations expresses the simple variation with elevation or time of one of the five variables, in terms of the contemporaneous spatial distribution of one or more of the variables. In Table 2 the first and second equations are *diagnostic*—they do not explicitly involve variation with time—whereas the other three equations are *prognostic*. Starting from the observed fields of R, U, V the first step in the forecast is to obtain the concurrent P diagnostically from the first equation, then the concurrent W diagnostically from the second. With the information thus acquired, the variables R, U, V are extrapolated forward in time (usually by one hour or less) by means of the three prognostic equations. The same alternation between diagnostic and prognostic steps evidently can be performed once again, starting from the new values of R, U, V ; repetition of this procedure a sufficient number of times will produce the desired forecast.

Eliassen used pressure as a vertical coordinate, whereas the National Meteorological Center uses a 'sigma' coordinate, which is a linear function of pressure designed so that the ground and the tropopause are coordinate surfaces. (The sigma-coordinate concept was introduced

by Phillips.) This variant of Eliassen's model requires one additional diagnostic variable—the pressure—and two additional prognostic variables—proportional to the masses of unit columns of troposphere and stratosphere. Although these additional variables disturb the simplicity of the foregoing comparison, I believe it is nevertheless correct to say that the NMC model and Richardson's have virtually the same homology as Eliassen's and Richardson's.

The use of pressure as a vertical coordinate has played an important role in the modern theory of numerical weather prediction. The horizontal force due to the gradient of hydrostatic pressure can be expressed very simply in isobaric coordinates, and the all-important vorticity equation also is simpler in these coordinates. Moreover, and this is their main virtue: isobaric coordinates lead to an especially simple and convenient form of the diagnostic equation for vertical velocity (the W -equation of the foregoing discussion). When height is used as the vertical coordinate, the W -equation is more complicated; indeed, the derivation of this equation is the principal substantive contribution of Richardson's book to dynamic meteorology. Although the idea seems evident to us today, we must credit Richardson with a major achievement in recognizing that, since the vertical velocity is a diagnostic quantity under quasi-static conditions, a corresponding diagnostic equation is essential for a complete, algorithmic system of numerical prediction—a "lattice-reproducing process." In his numerical example Richardson used the form given as (1) on page 178; but he advances (18) on page 118 as preferable. Eliassen has proposed for the latter the name *Richardson's equation*.¹²

Why did Richardson use height rather than pressure as vertical coordinate? I think the answer is simply that he did not discover the 'isobaric' form of the W -equation, which in in fact is merely the equation for mass conservation. The first published statement of this important equation is in a little-known paper by Ertel (1943). Later it was given by Van Mieghem (1947) and simultaneously by Sutcliffe (1947), who attributed it to O. Godart in an unpublished memorandum of 1942. Independently, it was derived by Eliassen (1949) in a monograph which gave the first complete set of prediction equations in isobaric coordinates and paved the way for further developments along these lines. Eliassen emphasized the fact that the isobaric form of the vertical-velocity equation is more convenient than Richardson's, but drew attention to the historical significance of Richardson's equation.

Richardson skirted the question of isobaric coordinates in Chapter 3, where he discusses the division of the atmosphere into horizontal layers (pp. 16–17):

In making a conventional division of the atmosphere into horizontal layers the following considerations have to be borne in mind. It is desirable to have one conventional dividing surface at or near the natural boundary between the stratosphere and troposphere, at an average height of 10.5

km . . . over Europe. Secondly, that to represent the convergence of currents at the bottom of a cyclone and the divergence at the top . . . , the troposphere must be divided into at least two layers. Thirdly, that the lowest kilometre is distinguished from all the others by the disturbance due to the ground. Thus it appears desirable to divide the atmosphere into not less than 4 layers. If the layers are of equal or approximately equal mass the treatment of many parts of the subject is greatly simplified, e.g. radiation, atmospheric mixing, etc. To facilitate comparison with V. Bjerknes' charts and tables, I have chosen 5 strata divided at approximately 2, 4, 6, and 8 decibars.

This being granted, there are various ways in which the divisions may be taken:

(1) Divisions at the instantaneous pressures of 2, 4, 6, 8 decibars. This is Bjerknes' system, except that he takes 10 sheets, not 5. The heights of the isobaric surfaces become the dependent variables in place of the pressures. This system readily yields elegant approximations. But it entails the inconveniences of deformable coordinates, for it is equivalent to taking . . . [pressure] as an independent variable in place of . . . [height]. The corresponding alterations in the equations can be carried out by means of the following set of substitutions.

. . . [There follows a statement of the relations between derivatives with pressure constant and the corresponding derivatives with height constant.]

The result of these substitutions is to produce a large number of terms. The additional terms are small, but they are not always negligible in comparison with the errors of observations. As observations improve they are likely to become more significant. On this account I have preferred to use instead the following system:

(2) The divisions between the five conventional strata are taken at fixed heights above mean sea-level, so chosen as to correspond to the mean heights of the 2, 4, 6 and 8 decibar surfaces. These mean positions . . . are at about 2.0, 4.2, 7.2, 11.8 kilometres over Europe.

I have quoted this extract at some length because it shows the cautious sifting of alternatives which is characteristic of Richardson's work. The reference, under (1), to "elegant approximations" is intriguing, but I think there can be little doubt that had he discovered the isobaric *W*-equation, he would have displayed it and probably used it.

Richardson's division of the atmosphere into five layers ("conventional strata") can be compared with the division into six layers in the NMC model (see Fig. 2). The stratosphere consists of two layers in the NMC model (where it is surmounted by an inert, isentropic layer), and of only one layer (assumed isothermal) in Richardson's model. However, Richardson's treatment of the stratosphere differs significantly from that of the other layers. Indeed, Chapter 6 is devoted exclusively to a detailed discussion of the problem; the conclusions are (pp. 147-148):

An effort has been made to treat all the air above 11.8 km as a single conventional stratum in the sense that the momenta, pressures, and densities in a column should each be represented by a single number. To do this, all quantities have to be integrated with respect to height. The integrals of . . . [pressure, density, entropy] came out simply because the temperature is independent of height. But integrals involving velocity can only be obtained from the relation at any level of velocity to pressure. The strict treatment of this

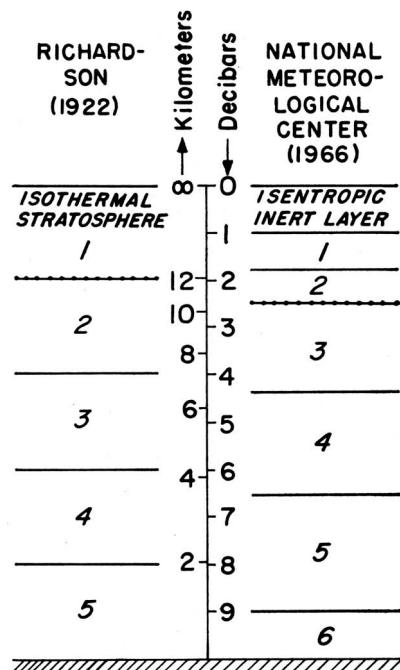


FIG. 2. Comparison between vertical structure of Richardson's model and that of the model currently used by the National Meteorological Center. The dotted line indicates the tropopause.

relation by analysis is too difficult, and so the geostrophic approximation has been introduced. This is probably good enough for transforming the dynamical equation, but when applied to finding the temperature change it yields results which are unlikely.

If on further consideration the single stratum has to be abandoned, another plan is ready. Divide the stratosphere into conventional strata. For all of these except the uppermost the general processes of Ch. 4, Ch. 5 will apply. The mass of this one having been made small, any errors committed in treating it will be of little consequence near the earth where we live. . . .

This excerpt brings out the fact that in each of the layers of his model, Richardson performs an analytic, vertical integration of each of the governing equations (except the *W*-equation) between the interfaces that bound the layer, and thereby arrives at equations in which the dependent variables are averages through the layers. In fact, both Richardson and NMC use *P* and *W* at interfaces, and *U*, *V* and *R* in layers. (Richardson uses *P* also in layers.)

The troposphere has four layers in both models; its upper limit is about 200 mb in Richardson's model, about 250 mb in the NMC model. The thickness of the surface boundary layer is about 200 mb in Richardson's model, and in the NMC model it is 100 mb. In Chapter 4 the special problems of this layer are examined meticulously (pp. 65-94), and at the end of this discussion Richardson says (p. 92):

For the sake of economy in arithmetic, an effort has been made to use a thick lower stratum extending to a height of 2

km above sea-level. Whether this plan will be successful or not depends on whether four empirical quantities . . . [related to vertical flux of momentum, heat and water vapor] can be expressed with sufficient accuracy as functions of the data available in the numerical process. There is some hope of it, but in order to settle the question many more observations, and reductions of existing observations, are required.

If this plan should fail, then an alternative is to be found in taking thinner strata near the surface. If for example we had divisions at heights of about 50 m, 200 m, 800 m, all our processes would be much more exact. . . . These thinner layers would need to rise and fall with the height of the land above the sea, and consequently the dynamical equations in them would need to be furnished with extra terms depending on the slope of the ground. As the land sometimes rises above 2000 metres, these surfaces would project above . . . [the 800-mb level]. In avoiding that difficulty we might end by making all the conventional strata rise and fall with the height of the land.

Here are the ingredients of 'sigma' coordinates!

Richardson attached much importance also to the horizontal and temporal distribution of variables, which he designated the "arrangement of points and instants" (Chapter 7). The considerations that he adduces to establish this arrangement are explained as follows (p. 149):

It will be convenient to have a brief distinctive name for the arrangement here to be discussed. For this purpose we may borrow from crystallography the term "lattice."

The approximate representation of a differential coefficient by a ratio of finite differences is notably more exact when the differences are centered. . . . If A , B and u be any three variables, the ideal arrangement would be such that:

(i) Wherever B has to be equated to $\delta A / \delta u$, then B should be tabulated at points on the u -scale half-way between the points where A is tabulated.

(ii) Wherever two variables have to combine in an expression, not involving their differential coefficients, they should be tabulated at the same points and instants.

(iii) When A is a function of $\delta A / \delta u$, special difficulties arise. . . .

Unfortunately it is not possible to satisfy conditions (i) and (ii), in their entirety, for the given differential equations. The best we can do is to satisfy (i) and (ii) for the largest terms, and to leave the rest to be centered by interpolation.

The arrangement adopted is obtained by applying these principles to the linearized forms of the three prognostic (U , V , and R) equations. The result is a distribution of points in a "chessboard" pattern formed by meridians and parallels, with P , W , and R defined at the center of each black square and U , V at the center of each white square. The size of individual "chequers" in the pattern is governed by (p. 16):

. . . four considerations: (1) the scale of variation of atmospheric disturbances, (2) the errors due to replacing infinitesimals by finite differences, (3) the accuracy which is necessary in order to satisfy public requirements, (4) the cost, which increases with the number of points in space and time that have to be dealt with.

He finds that the density of telegraphic weather stations in the existing distribution within the British

Isles is 32 stations in about $56 \times (100 \text{ km})^2$, equivalent to one station in a square of side 132 km. In view of the disparity in station densities over land and sea ("abundance of observations on the former does not compensate for scarcity of observations on the latter"), he concludes that (p. 19):

A satisfactory arrangement would appear to be to divide the surface into chequers by parallels of latitude separated by 200 km and by meridians spaced uniformly at the rate of 128 to the whole equator. The chequer is then nearly a square of $200 \times 200 \text{ km}$ in latitude 50° . At the equator even, it is not too elongated, as it measures there 313.09 km east $\times 200 \text{ km}$ north. In latitude 63° the chequer is about 142 km from east to west. As $\frac{142}{200} = \sqrt{1/2}$ this would be the latitude for the first omission of alternate meridians. . . .

The choice of 128 (=?) meridians on the equator is made with a view to dealing conveniently with convergence of the meridians, by a scheme in which it is "proposed to begin at the equator with 128 meridians, to omit alternate ones in latitude 63° or thereabouts, and again to halve the number at successive stages until only four are left close to the pole." (p. 155). The "joints" involved in this scheme are treated at the end of Chapter 7, where there is the remark (p. 155):

If we had to deal only with local polar meteorology the most satisfactory plan would probably be to neglect the curvature of the earth and to arrange pressure and momentum points on a "chessboard" formed by straight lines intersecting at right angles. But there does not seem to be any way of making a smooth joint between the rectilinear chequers which suit the poles, and the chequers formed by meridians and parallels of latitude which suit the rest of the globe.

For the reason mentioned above, the spacing between meridians thus recommended for subpolar regions is $360^\circ / 128 = 2^\circ 48' 45''$, but our author adds plaintively (p. 19), "Unfortunately this was not thought of, until after the 3° difference of longitude had been used in the example of Ch. 9."

Richardson's chessboard arrangement can be exhibited most easily by means of the "introductory example" of Chapter 2, designed to illustrate the framework of his prediction method in a much simplified case. (I find this one of the most interesting parts of the book!) Richardson derives the governing equations of this case by linearizing the prognostic equations and then integrating them with respect to height from sea level to "a height so great that the density there is negligible." (p. 4.) The result (Table 3) is a model with three

TABLE 3. Structure of the governing equations in Chapter 2.

Change of	Determined by
P with time	U, V
U with time	P, V
V with time	P, U

prognostic equations formally identical to Laplace's tidal equations. (The vertical integration eliminates W ; and R is replaced hydrostatically by P . A model of this type now is called *barotropic*.) The chessboard arrangement (in his terminology, the "lattice") for this model is shown in Fig. 3. The characteristic features of the arrangement are that each square contains only some—not all—of the dependent variables, and that it conforms to the rules (i) and (ii) (quoted above) for the use of central differences. Pressure is defined only on the black squares, velocity (or for Richardson, momentum) only on the white squares. (In his main forecast procedure W and R also are defined, on black squares only.) This arrangement is designed to use the minimum amount of information needed to form what he calls a "lattice-reproducing process" (now usually called an algorithm)—that is (p. 156): "Wherever in the lattice a pressure was given, there the numerical process must yield a pressure. And so for all the other meteorological elements." The lattice of Fig. 3 has been used frequently in recent years (in some cases with extension to the third dimension), but not always with due acknowledgement to Richardson. For this reason I have proposed to name Fig. 3 (with or without extensions to height and time) the *Richardson lattice*.

In a very brief discussion of the "arrangement of instants" (p. 150) Richardson comes to the conclusion that it seems best to tabulate all quantities at each instant; but here, uncharacteristically, he is too hasty. In Fig. 3 quantities in the left side of each square are a half step out of phase (in time) with quantities in the right side. Thus, at a particular instant the only quantities that appear are those in the left (or right) side; a half step later, only those in the right (or left) side appear. This gives a "lattice-reproducing process" that conforms with the rules for centering, and is the unique extension of Richardson's lattice into the time dimension.¹³

Richardson observes (p. 150) that ". . . progress in time is made by what may be called the "step-over" method, in which, in order to secure proper centering, we multiply . . . [the rate of change] of any quantity at the instant t by . . . [the time step], and add this product to the value of the quantity . . . [a half step earlier], in order to find the new value . . . [a half step later]." In choosing the size of the time step, Richardson says simply (p. 16), "With regard to time intervals, the existing practice of making observations for telegraphic purposes every 12 hours, or sometimes every 6 hours, is again our safest guide." In the numerical example worked out in detail in Chapter 9 he uses a step of six hours. We know now that this step is much too large: if used to produce a forecast of, say, 48 or 72 hours, the result would be hopelessly distorted because of a malfunction of the "lattice-reproducing process." This derangement—the severity of which depends exponentially upon the number of time steps taken—is called *computational instability*. It is purely mathematical in origin

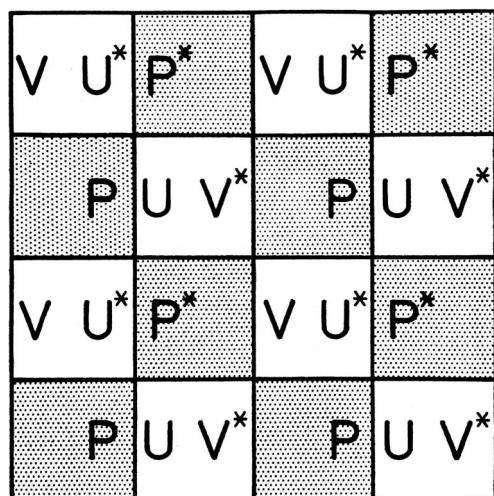


FIG. 3. The Richardson lattice: pressure on black squares, momenta on white squares. The left and right sides of each square are a half step out of phase in time. Quantities without the asterisk form an elementary Richardson lattice, which is "lattice-reproducing" if Coriolis forces are excluded. Those with the asterisk form a similar elementary lattice which is conjugate to the other one in the sense that taken together, the two are "lattice-reproducing" if Coriolis forces are included.

and is not the result of any impropriety in formulating the governing differential equations.

In the numerical example worked out in Chapter 9, the pressure change at the ground at the one point where it was computed came out to be 145 mb in six hours. This is completely unrealistic, when it is considered that the value applies to a region of dimensions about 200 km \times 200 km. It is sometimes said that Richardson's forecast failed because his time step was too large. This is not an accurate or fair appraisal; indeed, it is misleading because Richardson took only one time step—that is, he calculated only initial rates of change from the given data. (To do even this much took him six weeks!) Since computational instability makes its appearance only after at least several time steps are made, this cannot be the source of his trouble. Moreover, I have the opinion that if he had taken several steps (as would have been possible with the aid of an electronic computer), and thereby observed computational instability in action, he would promptly have made a diagnosis and prescribed a remedy. For it should be remembered that numerical analysis was one of Richardson's *fortes*, and that his interest and accomplishments in this field were a prime motivation for undertaking the work on weather prediction. Indeed, on p. 153 in a simple example of numerical integration (given for a purpose unrelated to the present subject) he takes five steps and is very close to detecting a computational instability, for he observes: "There is a curious oscillation in time about the errors of the step-over method, the amplitude increasing as it goes."

However, this speculation is beside the point. Richardson's forecast failed not because of computational instability but for entirely different reasons: specifically, because of the nature of the initial wind data which he used. I shall return to this point later.

5. Richardson's dynamics

In the preceding section, I have discussed mainly those aspects of Richardson's book that are concerned with problems arising from the use of finite differences in place of derivatives. Although not more than 15 pages (mainly Chapters 3 and 7) specifically deal with this subject, it is clear that Richardson regarded the numerical processes as a characteristic feature of the work, and his own summary (pp. 1-3) stresses them more than any other topic. The book cannot, however, be judged on this basis alone, so I consider now what it tells us about Richardson's view of atmospheric dynamics, set forth mainly in Chapter 4 (where the momentum-tendency equations are formulated), Chapter 5 (on the equation for vertical velocity), and Chapter 6.

In Chapter 6 a special treatment is devised for the stratosphere, the uppermost layer in Richardson's model. The tropospheric layers are relatively thin and thus permit the use of ordinary finite differences, "But in the stratum, which has its base at 11.8 km and extends upwards to at least 40 km, the ratio of pressure is so great that the aforesaid approximations are all open to criticism and must be reexamined." (p. 125.) This reexamination proceeds from the special assumption that the stratosphere is and remains vertically (but not necessarily horizontally) isothermal. The temperature of each column may change with time, however. An additional assumption that the initial wind in the stratosphere is geostrophic, makes it possible to infer the integrated stratosphere momentum from the distribution of temperature at the base of stratosphere. Much of the effort in this rather difficult chapter is spent in deducing the consequences of these assumptions about temperature and wind in the stratosphere.¹⁴

A large portion of Chapter 4 ("The Fundamental Equations") is taken up by a discourse on turbulence, the most recurrent theme of Richardson's writings in meteorology. Among the papers listed by Gold (1954) there are four published in 1919 and 1920, including the famous paper "The supply of energy from and to atmospheric eddies." Since Richardson refers to each of these in Chapter 4, it is a reasonable inference that they are the "excrescences" which in the Preface he says were removed in 1919 for separate publication. In spite of these deletions, the subject of atmospheric turbulence is one of the book's dominant motifs. However, it cannot be said that Richardson's elaborate calculations of eddy flux of momentum and heat had an important bearing on the outcome of his forecast.

Richardson's formulation of the prediction equations is uncompromising: virtually no term is omitted no matter how small its effect. This can be seen most directly by examining the computing forms (pp. 188-210). Despite the fact that about one-third of the book and more than half the computing forms are devoted to thermodynamic and hydrologic calculations, the results of these calculations have only a minor influence on the hydrodynamic calculations. The direct link between these two categories is through the heat-source term in the vertical-velocity equation, namely the third term on the right side of Eq. (1), page 178. The contribution of this term to the vertical velocity can be seen from Computing Form P XVI (p. 203): in the lower layers this contribution is about 2 per cent, in the upper layers it is less than 1 per cent.

It is difficult to resist the conclusion that in using a substantial part of the book for complex thermodynamic and hydrologic considerations, Richardson has given us a tale "full of sound and fury, signifying nothing." Yet, it is a tale told, most assuredly, *not* "by an idiot," so we are compelled to look for a different interpretation. My own view is that, notwithstanding his evident dedication to the *pratique* of weather forecasting, Richardson did not look upon his book as being merely a blueprint for attainment of an operational scheme. In a broader sense a dual aim was to show how the diffuse body of knowledge that formed the dynamic meteorology of his day could be brought to bear upon the general problem of numerical weather prediction. Some insight to his attitude is found in the Preface where in charming, Richardsonian manner he says (third paragraph):

The scheme is complicated because the atmosphere is complicated. But it has been reduced to a set of computing forms. These are ready . . . to assist anyone who wishes to make partial experimental forecasts from such incomplete observational data as are now available. In such a way it is thought that our knowledge of meteorology might be tested and widened, and concurrently the set of forms might be revised and simplified. 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.

(A dream come true?)¹⁵

I believe that this wider view of the book is part of the truth. Yet, the advantages of historical perspective require us to observe some defects in the manner in which Richardson carried out the task he set for himself. His reluctance to accept even minor approximations, a defect which is diffused throughout the work, seems to stem from an attitude revealed at the outset in the ingenuous remark (p. 22), "As the arithmetical method allows us to take account of the terms which are usually neglected, many of these terms have been included." We might argue that the inclusion of small

terms is not harmful, having in mind the evident truth of the converse, that the exclusion of *large* terms is harmful. However, the indiscriminate inclusion of small terms is symptomatic of a graver defect: it indicates that the analysis is not guided by the use of a self-consistent model.

The concept of a 'model' has been important in the modern development of numerical weather prediction. In the context that concerns us here, all models have one attribute in common, namely a set of mathematical equations and formulas which, taken together, can be considered an explicit characterization of the model. Within this broad use of the word there are several shades of meaning. For example, when we speak of a homogeneous incompressible fluid we have in mind an idealization of a real substance, like water, and in particular a set of four hydrodynamical equations which determine the motion of such an ideal fluid. On the other hand, when we speak of a 'quasi-static' model, we mean to emphasize that the hydrostatic equation is used as an approximate statement of the vertical component of the equations of motion. The model is thus restricted but nevertheless is capable of describing a certain class of phenomena, on which we wish to focus attention. In the first example—that of a homogeneous incompressible fluid—we also limit the range of phenomena capable of description (for example, sound waves are excluded). Indeed, such a limitation is implicit in every model, and is the real purpose underlying the use of a model. However, in the first example the limitation is achieved by idealizing the physical properties of the working substance (geometrical arrangements might also be idealized), whereas in the second example it is achieved by an approximation, made on more distinctly phenomenological grounds after the structural aspects of the model have been formulated. The words 'structural' and 'phenomenological' can be used to distinguish between these two aspects of a model.

Finally, we also speak of the 'numerical' aspects of a model. What is usually meant by this is a set of equations approximately equivalent to the original equations of the model and designed specifically for numerical calculation. Typically (in weather prediction), the equivalent 'numerical' equations are obtained by means of finite-difference approximations to derivatives.

The prediction equations used by Richardson in his book are the original momentum equations for an ideal gas, modified by the quasi-static approximation. This constitutes what is now called a 'primitive, hydrostatic' (or quasi-static) model. The modern development of numerical weather prediction followed quite a different route. It can be said to have begun with the publication of papers by Charney (1948) and concurrently by Eliassen (1949). I have already mentioned the latter. Charney's paper opened the door to phenomenological modeling on a systematic basis; in particular, Charney

gave the equations for what is now called the 'quasi-geostrophic' model, in barotropic and in baroclinic form. In this model gravity waves (as well as sound waves) are excluded, by means of the quasi-geostrophic approximation, and thus, attention is focused on the large-scale, slow-moving features of atmospheric motion which appear to be significant for description of the day-to-day changes of circulation as seen on upper-air charts. Richardson's model differs fundamentally from the quasi-geostrophic model because it does not exclude gravity waves. It is the most direct way of formulating the prediction problem because (except for the vertical-velocity equation) it takes the hydrodynamical equations in their 'primitive' form. By contrast, the formulation of the quasi-geostrophic model is more devious, and could hardly have been achieved without a clear understanding of the mechanics of large-scale circulation systems.

In a sense it is paradoxical that Richardson's forecast was a failure whereas the first forecast from a quasi-geostrophic model was not—paradoxical because in principle it is the primitive-equation model that is capable of giving the more exact rendition of atmospheric motions in general. However, the quasi-geostrophic model was the first successful model for numerical prediction precisely because of its more restricted range of applicability, or rather because it was limited to the main features of the motions predicted, to the exclusion of all else. Today we know how to deal with the primitive-equation model for numerical weather prediction: we know that a key to success is in the proper modification of initial data, so that the forecast will not get off to a bad start by generating unrealistically large gravity waves. This is what happened to Richardson's forecast.

The quasi-geostrophic model is a culmination of many related lines of development in dynamic meteorology which emerged after the worldwide extension of the upper-air observation network that took place in the early 1930's. Foremost among these precursors are the work of J. Bjerknes and Rossby in the late 1930's in elucidating the dynamics of 'upper' waves, and that of Rossby in revealing the dominant role of the vorticity equation as governor of the dynamics of quasi-geostrophic flow; the work of Sutcliffe on 'development' (e.g., cyclogenesis) also is an important part of this background. Thus, from an historical perspective, since Richardson wrote his book decades before these germinal studies, it is understandable that he did not anticipate the quasi-geostrophic model.¹⁶

I have suggested that the restrictions implicit in the quasi-geostrophic model contributed to the success of this model. Another restrictive aspect of the quasi-geostrophic model first used for weather prediction is that it was a barotropic, rather than baroclinic model. Such a model does not permit horizontal variations of temperature or vertical variations of wind velocity, and thus is not capable of depicting the full, three-dimensional

structure of the circulation of the atmosphere. To anyone familiar with this structure as seen on daily upper-air charts, it may seem that such a model is not likely to be useful. However, through the work of J. Bjerknes, Holmboe, Rossby, Sutcliffe, and others it became evident that the field of pressure in mid-troposphere, at the 'level' of zero divergence of horizontal velocity, behaves to a large extent as if it were the pressure field of a barotropic model. A dynamical explanation of this fact, embodied in the concept and equations of an "equivalent-barotropic" model, was formulated by Charney (1949).

Although Richardson's model was baroclinic, and thus aimed at dealing with the atmosphere on a three-dimensional basis, he did briefly discuss a barotropic model, in the introductory example of Chapter 2. He introduces this example by saying "Before attending to the complexities of the actual atmosphere and their treatment by this numerical method, it may be well to exhibit the working of a much simplified case." He then describes the structural aspects of the model in this way (p. 4):

Suppose now that there is no precipitation, clouds or water vapor, neither solar nor terrestrial radiation, no eddies, and no mountains or land, but an atmosphere in which we can ignore or summarize variations with height moving upon a globe covered by sea. Further to simplify the problem, let us neglect all the quadratic terms in the dynamical equations. Then, in order to summarize the vertical velocity and the density, let us perform an integration with respect to height upon the horizontal dynamical equations and upon the equation of continuity of mass. If the limits of integration are sea-level, and a height so great that the density there is negligible, we thus arrive at a set of equations similar to those used by Laplace in his discussion of Tides on a Rotating Globe . . .

In the preceding section I discussed these equations from the standpoint of finite differences. Here I want to call attention to the fact that, although this barotropic model is 'primitive' and 'hydrostatic' and thus has the same pitfalls (in regard to initial data) as the more general, baroclinic model that is the main desideratum of the book, nevertheless, in the introductory example Richardson uses *geostrophic* initial data and thereby obtains what, in principle, is a 'meteorological' forecast. He introduces the initial data as follows (p. 5):

Now to represent the initial observations of pressure we are at liberty to write down any arbitrary set of numbers, at the points of the map where . . . [pressure] is required, only with this qualification: that if the assumed pressure gradients be unnaturally steep, the consequent changes will be perplexingly violent . . .

When the pressure distribution has been chosen, we have next to represent the initial observations of momentum-per-area by writing down numbers in the alternate chequers. These numbers might have been chosen independently of the pressure, and in fact quite arbitrarily, with a qualification similar to that mentioned above. But it has been thought to be more interesting to sacrifice the arbitrariness in order to test our familiar idea, the geostrophic wind, by assuming it initially and watching the ensuing changes.

In this example the initial pressure field is specified by a simple formula in terms of latitude and longitude, which gives one cell of high pressure and one of low pressure in the Northern Hemisphere and symmetrically, an identical pattern in the Southern Hemisphere (Fig. 4). The maximum departure of pressure from the mean is 38.5 mb (located at about 55° latitude), and the maximum geostrophic wind is about 19 m sec⁻¹ (located, rather unrealistically, at the equator).

In a memoir "The single-layer problem in the atmosphere and the height-integral of pressure," four years after publication of the book, Richardson and Munday (1926) returned to the question of the efficacy of the barotropic model for weather prediction. There he poses the problem as follows (pp. 17-18):

Does the atmosphere move in such a way as to permit us to describe its state in each vertical by a single number for temperature, by another for pressure, by a third for water-content, and by two more for momentum, and yet, in spite of this sparsity of data, allow us to obtain a meaningful description of the cyclones of temperature latitudes? If this is possible we shall say that the atmosphere can be treated as a "single layer."

The fact that tidal theory has been applied with success to the atmosphere shows that a single dynamical layer suffices for that special class of "long" motions. Whether it will serve for any other types of motion is problematic.

Those who make forecasts based on maps of surface conditions only, may be said to answer the above question in the affirmative. In the Bjerknes schematic cyclone, on the contrary, two layers are essential. The object of the present enquiry is to extract the answer from the records of sounding balloons. . . .

The conclusion of this enquiry is (p. 33): "We have found nothing in the observations to justify the hope that the atmosphere could be treated as a single dynamical layer." Thus, Richardson turned away from the barotropic model, which 25 years later was used successfully in the opening phase of operational numerical weather prediction. He could hardly have known that this model is best suited for prediction at *mid-troposphere* levels rather than at sea level.

As a final comment about dynamical aspects of the book, I shall express the opinion that a major defect in Richardson's approach to weather prediction is his disregard of perturbation theory as a means of clarifying the problems of dynamic meteorology through analysis of atmospheric wave motion. Although Richardson's book came slightly before the great developments which took place along these lines at the hands of V. Bjerknes and his Norwegian school, nevertheless it is strange that little of perturbation theory is visible in the book, or indeed in his other meteorological work. If this is a characteristic feature of his scientific style, perhaps some of the major defects in his prediction scheme were inevitable. However, without a conditioning against perturbation theory the whole conception of the book might not have germinated, and some of his other major contributions to meteorology might never have emerged!

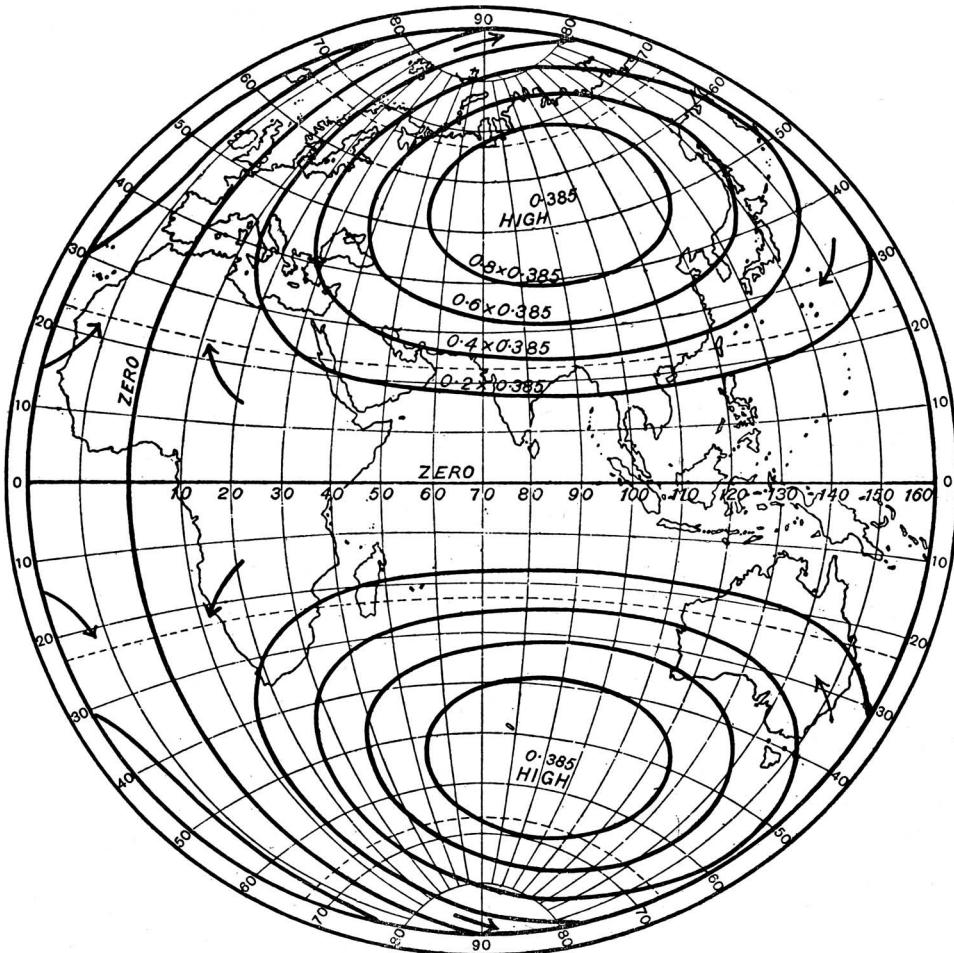


FIG. 4. The initial pressure field of Richardson's introductory example (reproduced from the book, p. 6). The pattern has hemispheric wave number 1 and maximum pressure departure 38.5 mb.

6. The initial data and forecast

Richardson wrote his book at a time when the daily surface weather map had long been issued for many regions of the globe. However, his computing scheme demanded three-dimensional synoptic data, and for this the primary source was a compilation issued regularly by the International Commission for Scientific Aeronautics,¹⁷ under the presidency of Hugo Hergesell, Director of the Meteorological Institute of Strassburg (see Shaw, 1926, pp. 167, 224). The purpose of these compilations was to bring together all data from upper-air soundings made each month on international days (normally one day, frequently several consecutive days) by countries participating in the work of the Commission.¹⁸

The date chosen by Richardson for initial data is 20 May 1910 (07 GMT). To obtain the initial "momenta-per-unit-area" for the five layers of his model he proceeded as follows (p. 182):

. . . The first process was to undo the computing already done . . . by the observing stations, by reconverting the winds

to components and the heights to pressures. The component velocities . . . were then plotted against the pressures. The divisions between the conventional strata, at 2.0, 4.2, 7.2 and 11.8 km were then marked off on the pressure scale, and areas on the diagram were measured with an Amsler planimeter so as to obtain . . . [the pressure integral of each component] between the limits of the strata. . . . The mean velocities . . . [thus implied] were compared with the mean resultant velocities and directions published by V. Bjerknes for his 10 standard sheets, on this occasion, and in general there was good agreement. In the data for Pavia and Strassburg, however, there were some discrepancies for which I could not account.

In the stratosphere Richardson assumed that the velocity components are linear functions of height, and thus extrapolated each component to zero pressure. The mean components for this layer were taken then as the extrapolated components at the height 18.1 km, which was his estimate of the mean height of the center of mass of the atmosphere above 11.8 km (that is, of his stratosphere).

For initial pressures, Richardson did not directly use the Hergesell reports, but instead turned to the elaborate syntheses of maps from these reports that were undertaken by V. Bjerknes and issued as the first series of publications of the Geophysical Institute at the University of Leipzig during Bjerknes' tenure there.¹⁹ This series was a landmark in the long-range program begun by Bjerknes with support from the Carnegie Institution of Washington, the first phase of which culminated in the great *Dynamic Meteorology and Hydrography*. For Richardson's purpose the essential feature of the Leipzig publication was a set of maps of the contours of ten isobaric surfaces at 100-mb intervals from 100 to 1000 mb, which he used as follows (pp. 181-2):

The initial pressures are tabulated at the ground and at exactly 2.0, 4.2, 7.2, 11.8 kilometres above m.s.l. [approximately the 800-, 600-, 400-, 200-mb levels]. They are read from V. Bjerknes' maps for the instant in question. These maps give the "dynamic height" of the isobaric surfaces, so that various conversions were necessary [because Richardson did not use pressure as vertical coordinate]. In the first place 2.0, 4.2, 7.2, 11.8 km are equivalent to 1.959, 4.113, 7.048, 11.543 "dynamic kilometres" when $g = 980.00 \text{ cm sec}^{-2}$ at sea-level. The pressures corresponding to these "dynamic heights" were obtained from the maps with the aid of V. Bjerknes' table "10M." Then a small correction has to be

applied for the variation of gravity. This was obtained from tables 2M, 4M and 10M . . .

To find the pressure at the ground, the height of the ground was first assigned by reference to V. Bjerknes' maps of idealized topography. . . . The pressure at sea-level was next read from V. Bjerknes' map for the particular instant, and was then corrected to the height of the ground by means of the usual tables (*Observer's Handbook*). In the case of very high land the surface pressure was obtained from the maps of the heights of the 800 mb and 900 mb surfaces, with the aid of table 10M.

The reference to "table 10M" is to Bjerknes and Sandström (1910, pp. 12B-16B).²⁰

During the period 18-20 May 1910, which includes the epoch of Richardson's forecast, the weather over northern Europe was dominated by a blocking high-pressure ridge which protruded southward from polar regions across Scandinavia. Southern Europe was characterized by relatively low pressure in association with a region of frontolysis centered over Switzerland; another frontolytic region developed south of Iceland as a precursor to northward extension of the Azores high. The following description was available to Richardson in Hergesell's Strassburg *Veröffentlichungen* (translation):

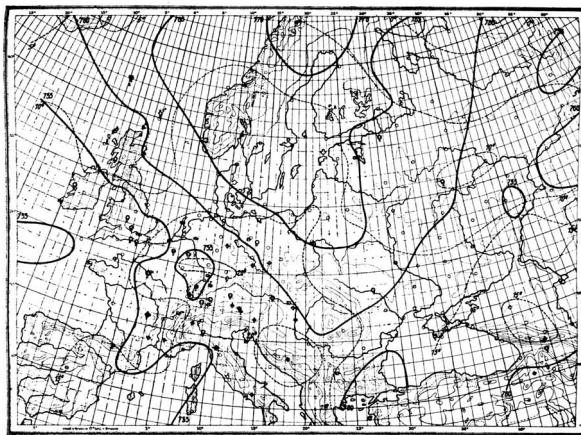
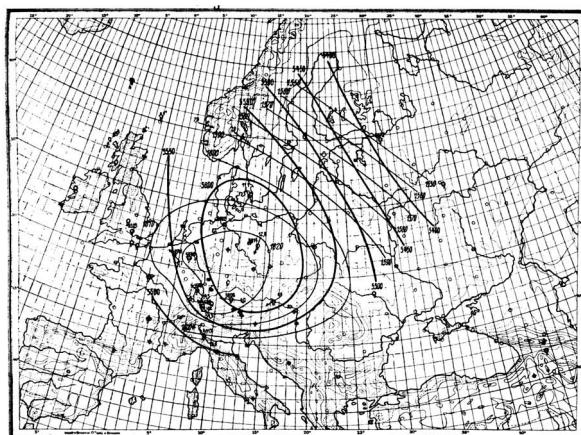
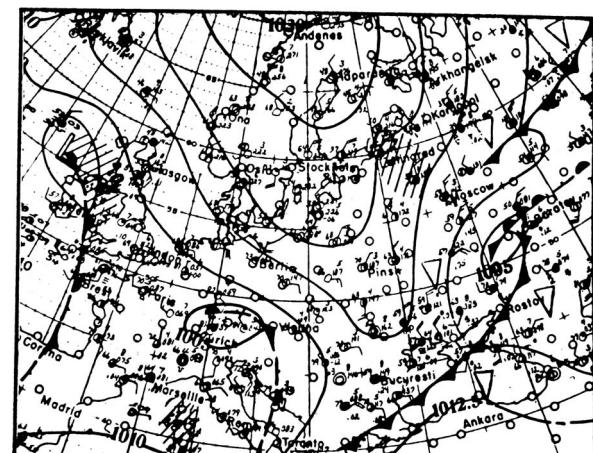


FIG. 5. Upper left: the 500-mb chart at 07 GMT 20 May 1910 in Bjerknes' Leipzig *Veröffentlichungen* (original scale 1:10⁶); thick lines are contours of elevation of the 500-mb surface in dynamic meters, thin lines are contours of thickness between 500 mb and 400 mb.

Lower left: the corresponding sea-level chart; continuous lines are isobars in millimeters of mercury, broken lines are isotherms in degrees Celsius.

Lower right: sea-level isobars (millibars) at 13 GMT 20 May 1910, from *Historical Weather Maps, Daily Synoptic Series, Northern Hemisphere Sea Level*.



Weather situation: During the ascent days [18–20 May 1910] the pressure distribution was very irregular, and frequent thunderstorms were the result, particularly in western and middle Europe. A cyclone moved northward from the Bay of Biscay, while a weak minimum shifted westward from the Adriatic with gradual increase in intensity and a maximum over Scandinavia gradually increased in strength.

In Fig. 5 are reproduced two of the Bjerknes maps (500 mb and sea level) for 07 GMT 20 May 1910, and the corresponding portion of the Northern-Hemisphere sea-level map found in the *Historical Weather Maps* series, valid six hours later.

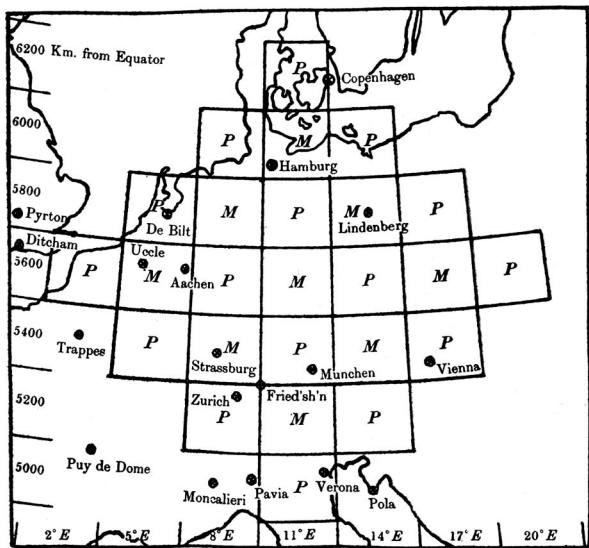


FIG. 6. Reproduced from Richardson's book (p. 184): "Map of points for pressure (P) and momentum (M) used in the example of Ch. 9. Note: these points are placed at the centres of the chequers, and to the centres also the latitude and longitude refer. Each chequer measures 3° from west to east and 200 km from south to north."

The region for which Richardson compiled initial data, and the corresponding chessboard pattern are shown in Fig. 6. He made a forecast at two points: the M-point in the center of the region (11°E , 5600 km from equator) and the P-point directly south of this. In Table 4 are assembled the pressure and momentum data tabulated by Richardson (p. 185) as initial data for his forecast. (Stratosphere temperatures also are included in his tabulation, and water content of each of the four layers of troposphere at P-points.) The arrangement of squares corresponds to that of Fig. 6 (some squares omitted), and the two squares for which a forecast was made are enclosed by a heavy border. In P-squares the quantities tabulated are pressure (millibars) at the ground (bottom number) and at each of the standard heights 2.0, 4.2, 7.2, 11.8 km MSL of the model. In M-squares are the eastward (left) and northward (right) "momenta-per-unit-area" for the stratosphere (top number) and for each of the four tropospheric layers of the

model. The unit here is $10^4 \text{ gm cm}^{-1} \text{ sec}^{-1}$, which fortuitously makes the tabulated numbers roughly the same numerically as the mean wind in knots. The italicized number at the bottom of each square is the average elevation of the ground (in meters).

Let us turn now to the forecast that Richardson made from these data. I have already pointed out that he did not go beyond calculation of the initial tendencies—the initial rates of change with time—from which, by simple extrapolation, he inferred a six-hour forecast. Table 5 is a summary of the six-hour changes of momentum at the one square for which momentum tendency was calculated. In each block of the table the arrangement of numbers is the same as in Table 4: on the left the eastward component of momentum (change) in each of the five layers (numbered as in Fig. 2), on the right the northward component; unit: $10^4 \text{ gm cm}^{-1} \text{ sec}^{-1}$ (roughly the same numerically as the mean wind in knots). The first block of the table gives the total six-hour changes of momentum (from the bottom lines of forms M III, IV pp. 209, 210), and the other three blocks show how each of these totals is formed from three segments of the prognostic momentum equations.

The most conspicuous defect in the first block of Table 5 is in layer 1 (the stratosphere), where the magnitude of the numbers is much larger than tolerable for a 'meteorological' forecast. Indeed, each of the numbers in this block should be not more than about one-fifth of the corresponding number in the second block. The numbers in the second block are not conspicuously unreasonable except in layer 1 where they seem too large. The real trouble lies in the third block, and to some extent in the fourth (where some of the numbers are too large, especially in layers 2 and 3). Since the numbers in the first block are the sums of the corresponding numbers in the other three blocks, and since those in the fourth block ("Other terms") are small in comparison with the pressure-gradient and Coriolis-force terms (except in layers 2 and 3), the only way in which the total change can be a small fraction of the pressure-gradient term is for the Coriolis-force term to be nearly equal to the pressure-gradient term—that is, for the wind to be quasi-geostrophic. Richardson's reliance upon the *observed* wind as initial data, unmodified by any consideration of quasi-geostrophic balance, foreclosed the possibility of a successful forecast.

The forecast of pressure change shows more dramatically the fatal consequences of using unmodified observed winds as initial data. This forecast is analyzed in Table 6, which I have constructed from Form P XIII (p. 200, reproduced here as Fig. 7). To interpret Table 6, remark that the difference in pressure between the bottom and top of a particular layer is proportional to the mass of the layer per unit horizontal area (gravity being the constant of proportionality). In Form P XIII Richardson calculates the rate of change of mass in each layer as a sum of eastward, northward, and vertical mass

TABLE 4. The initial pressure and momentum data used by Richardson (see text for further explanation).

km north of equator	longitude 5E	longitude 8E	longitude 11E	longitude 14E	longitude 17E
6000			- 6.5 0.8 12.7 -10.4 8.1 - 2.5 - 8.1 0 -19.8 8.4 0		
5800		- 7.0 - 6.2 -11.4 - 9.1 -16.0	204.7 409.0 608.6 798.3 988.3	-16.0 4.0 - 6.0 - 6.0 -21.0	
		150	200	100	
5600	- 3.0 -11.0 -24.5 30.3 -22.3 15.8 - 9.1 8.7 - 1.8 1.5	204.7 408.3 606.7 795.0 983.4	- 5.6 - 1.8 -14.6 - 6.2 - 9.5 2.9 - 5.2 5.8 -11.0 5.5	204.9 409.1 608.7 797.9 976.3	-10.0 - 3.2 0 -26.0 - 5.5 -13.5 - 2.5 4.8 -19.0 16.0
	200	200	400	300	300
5400	203.0 404.9 604.4 792.8 974.4	2.7 -32.8 -13.6 - 3.3 4.8	205.0 409.0 607.9 796.0 962.6	0 -16.6 - 9.5 - 1.9 - 6.5	204.4 408.2 606.8 797.8 988.2
	200	400	400	400	200
5200		203.9 406.2 605.0 794.9 874.6 [?]	- 5.0 8.0 -28.0 4.1 -17.5 15.0 -10.5 8.0 -15.5 4.0	204.3 407.5 606.5 796.7 845.8 [?]	
		120	180	150	
5000			203.4 403.4 603.1 795.7 997.2		
			100		

TABLE 5. Richardson's forecast of six-hour changes of eastward and northward components of momentum (unit: $10^4 \text{ gm cm}^{-1} \text{ sec}^{-1}$) in each layer. Layer 1 is the stratosphere; see text for further explanation

Layer	Total change	Pressure-gradient term	Coriolis-force term	Other terms
1	-73.0 -33.7	-67.6 -52.1	- 4.3 13.6	-1.1 4.8
2	-19.6 23.8	-10.6 3.7	-15.1 35.4	6.1 -15.3
3	- 8.9 13.8	-20.3 - 5.0	7.1 23.1	4.3 - 4.3
4	-15.3 - 4.3	-27.8 -16.6	14.0 12.5	-1.5 - 0.2
5	-17.9 6.3	-32.0 -22.7	13.4 26.8	0.7 2.2

TABLE 6. Richardson's forecast of 6-hr changes of pressure (unit: 1 mb), expressed as a change in the pressure thickness of each layer. Layer 1 is the stratosphere; see text for further explanation

Layer	Total change	Eastward	Northward	Vertical	Horizontal
1	48.3 (48.3)	12.9	52.9	-17.5	65.8
2	28.7 (77.0)	-77.5	53.8	52.4	-23.7
3	26.2 (103.2)	-19.7	67.5	-21.6	47.8
4	23.3 (126.5)	- 6.8	14.2	15.9	7.4
5	18.6 (145.1)	54.2	-6.4	-29.2	47.8
Sums	145.1	-36.9	182.0	0	145.1

COMPUTING FORM P XIII. Divergence of horizontal momentum-per-area. Increase of pressure

The equation is typified by: $-\frac{\partial R_{\text{ss}}}{\partial t} = \frac{\partial M_{\text{ss}}}{\partial e} + \frac{\partial M_{\text{ss}}}{\partial n} - M_{\text{ss}} \frac{\tan \phi}{a} + m_{\text{ss}} - m_{\text{ss}}^* + \frac{2}{a} M_{\text{ss}}$. (See Ch. 4/2 #5.)

* In the equation for the lowest stratum the corresponding term $-m_{\text{ss}}$ does not appear

Longitude 11° East $\delta e = 441 \times 10^6$			Latitude 5400 km North $\delta n = 400 \times 10^6$			Instant 1910 May 20 th G.M.T. Interval, δt 6 hours $a^{-1} \cdot \tan \phi = 1.78 \times 10^{-9}$ $a = 6.36 \times 10^8$					
Row:-				previous 3 columns	previous column		Form P XVI	Form P XVI	equation above	previous column	previous column
λ	$\frac{\delta M_x}{\delta e}$	$\frac{\delta M_x}{\delta n}$	$-\frac{M_x \tan \phi}{a}$	$\text{div}'_{xx} M$	$-g\delta t \text{div}'_{xx} M$		m_x	$\frac{2M_x}{a}$	$-\frac{\partial R}{\partial t}$	$+\frac{\partial R}{\partial t} \delta t$	$g \frac{\partial R}{\partial t} \delta t$
	$10^{-6} \times$	$10^{-6} \times$	$10^{-6} \times$	$10^{-6} \times$	$100 \times$		$10^{-6} \times$	$10^{-6} \times$		$100 \times$	$100 \times$
h_1	-61	-245	-6	-312	656		0		-229	49.5	483
h_2	367	-257	2	112	-236		-83	0.06	-136	29.4	287
h_3	93	-303	-16	-226	478		165	0.11	-124	26.8	262
h_4	32	-55	-12	-35	74		63	0.07	-110	23.8	233
h_5	-256	88	-8	-226	479		138	0.03	-88	19.0	186.
h_6	NOTE: $\text{div}'_{xx} M$ is a contraction for $\frac{\delta M_x}{\delta e} + \frac{\delta M_x}{\delta n} - M_x \frac{\tan \phi}{a}$ $= \frac{\partial p}{\partial t} \delta t$					SUM = 1451					1451
											check by $\Sigma -g\delta t \text{div}'_{xx} M$

FIG. 7. Computing Form P XIII. (Reproduction of p. 200 of the book. CGS units are used throughout.) This is the fateful form on which Richardson computed a 6-hr pressure change of 145.1 mb at the ground.

convergence within the layer. If these numbers are multiplied by gravity and by the 6-hr time increment adopted for the forecast, we obtain the corresponding three contributions to the 6-hr change of pressure difference (bottom pressure minus top pressure) across the layer. The latter are given in the second block of Table 6. Their sum within each layer—the total 6-hr change of pressure difference—is the number on the left in the first block of the table.

Two other columns in Table 6 are included to aid in interpretation of the results. The numbers in the last column are simply the sums, within each layer, of the corresponding numbers in the "Eastward" and "Northward" columns. They express the contributions to pressure change by horizontal convergence of mass. Finally, in the first block the parenthetical numbers on the right give vertical partial sums of those on the left, the second number on the right being the sum of the first two on the left, the third the sum of the first three, and so on. Since the stratosphere (layer 1) extends effectively to zero pressure, these numbers evidently are the 6-hr changes of pressure at the bottom of each layer. The last one is Richardson's forecast of 6-hr change of pressure at the ground: 145.1 mb! The actual change was more than a hundred times smaller.

The cause of this error is revealed by Table 6. For the sake of illustration, suppose that a correct forecast would have given a surface pressure change of 1.45 mb in 6 hr,

that is, exactly 100 times smaller than that of Table 6. Now fix attention on the last column of the table, and note that the change of surface pressure is the sum of horizontal mass convergence in the five layers. (Vertical mass convergence can affect pressure change *above* the ground, but not *at* the ground.) One way to reduce this sum by a factor of 100 is to reduce the horizontal convergence of mass in each layer by about a factor of 10, and then to adjust the resulting values somewhat so that their sum is about a factor of 10 smaller than the individual values. The latter adjustment produces the necessary 'compensation' effect well-known empirically. The former adjustment is possible if there is a similar compensation which results when the "Eastward" and "Northward" contributions to "Horizontal" are added; this too corresponds to a well-known empirical principle. There is in fact a tendency for the "Eastward" and "Northward" contributions in Table 6 to compensate in each layer (except the stratosphere), but the compensation is very incomplete. As for vertical compensation in the "Horizontal" column, this is inconsequential. (It would be much improved if the stratosphere's contribution were given the opposite sign!)

It is apparent that Richardson's forecast of surface pressure change lacks a compensation factor of about 100. Without a more exhaustive analysis, I can not say with certainty how much of this compensation must be assigned to the vertical distribution of horizontal mass

convergence and how much to the individual values of convergence at each level. Probably, most of the needed compensation should be in the vertical distribution, because individual values of horizontal mass convergence (Table 6, last column) are not unreasonable as to order of magnitude. Thus, the corresponding values of velocity convergence in each layer are (Form P XVI, p. 203):

Layer	1	2	3	4	5
Velocity convergence	1.49	-0.57	1.11	0.17	1.47
(Unit: 10^{-5} sec $^{-1}$)					

and these are not excessively large. Similarly, the vertical velocities obtained by Richardson are not unreasonable as to order of magnitude (Form P XVI, p. 203):

Level (km)	11.8	7.2	4.0	2.0	0.4
Vertical velocity	-2.465	2.833	0.800	1.420	-0.200
(Unit: 1 cm sec $^{-1}$)					

although their distribution may be incorrect. Thus, the evidence indicates *prima facie* that the "Horizontal" numbers in Table 6 are mainly in need of vertical compensation, to produce a reasonable forecast of pressure change at the ground. There must also be better compensation between "Horizontal" and "Vertical" in each layer, in order to have good forecasts of pressure change above the ground. To summarize, we can say that if the entries in Table 6 are to give a reasonable forecast, a rather delicate state of compensation must prevail between them. Nowhere in the book does the author indicate that he is aware of this crucial requirement.

Richardson begins to analyze the failure of his forecast on p. 212:

The striking errors in the "forecast," which has been obtained by means of the computing forms, may be traced back to the large apparent convergence of wind. It may be asked whether this spurious convergence arises from the errors of observations with balloons, or from the finite horizontal differences being too large, or thirdly from the process by which the winds at points, arranged in a rectangular pattern, are interpolated between the observing stations. . . .

He examines the third possibility in some detail, and concludes that this cannot be the most significant error.

In the brief but very interesting Chapter 10 ("Smoothing the initial data") Richardson carries his thoughts about the forecast errors one stage farther (p. 214):

We are not concerned to know all about the weather, nor even to trace the entangled detail of the path of every air-particle. A judicious selection is necessary for our peace of mind. For some such reason it is customary, at stations which report wind by telegraph, to replace the instantaneous velocity by a mean value over about ten minutes. An extension of this process must be contemplated, for there is a good deal of evidence to show that the wind is full of small "secondary cyclones" or other whirls having the most various diameters. The arithmetical process can only take account individually of such whirls as have diameters greater than the distance between the centres of red chequers in our

co-ordinate chessboard, and this length has been taken provisionally as 400 km.

If we smooth out these whirls we shall have to make amends by introducing suitable eddy-diffusivities. So far meteorologists do not appear to have attended to eddy-diffusivities of this kind. . . .

He considers five ways to do the smoothing: (i) by taking space means of the initial data, (ii) by taking time means, (iii) by smoothing (or eliminating entirely) the divergence of the initial winds, (iv) by a similar operation to smooth the initial vorticity, (v) smoothing during the forecast. What he means by (v) is (p. 216):

While beginning the forecast with the unsmoothed velocities we might temporarily introduce into the dynamical equations terms representing a considerable fictitious viscosity. These would have the effect of smoothing out irregular motions whether waves of compression or whirls. As is well known, the shorter the wave length or the smaller the diameter of the whirl, the more rapidly would the corresponding motion be damped out.

This type of smoothing is often used by modern practitioners. Similar effects are also obtained by means of specially designed finite-difference operations, without explicitly introducing a fictitious viscosity.

Smoothing of type (iii) is, of all five ways, the most relevant to his forecast errors (p. 215):

The irregularity in the observations which has forced itself on our attention is the large value of . . . [wind divergence]. It may be possible by slight adjustment of . . . [wind velocity] to remove large values of . . . [wind divergence] especially if the latter are scattered at random and if they vary, as is to be anticipated, symmetrically around a mean near zero.

To do this he adds to the observed wind an irrotational field whose divergence is (say) the negative of that of the observed wind. This leaves a modified wind which is strictly nondivergent. (Taking (iii) and (iv) together, what he proposes is equivalent to the Stokes-Helmholtz decomposition of an arbitrary vector field into irrotational and nondivergent parts. The smoothing is to be performed on these parts separately, rather than on the observed winds.)

It is known now that, to obtain a reasonable forecast of typical large-scale, slow-moving circulation patterns it is necessary to start from a wind field that is either non-divergent or in which the vorticity greatly predominates over whatever divergence is present. Often this is done by eliminating the initial divergence entirely, as Richardson suggested. However, contrary to Richardson's conception the initial, nondivergent wind field is obtained not from observed winds (or at least not mainly from them), but from the observed pressure field by using the geostrophic wind, with some modification to allow for nonlinear effects. Further, it has been found necessary to bring to zero not only the initial divergence but also the initial rate of change of the divergence. All of these feats are accomplished in one stroke by solving

a nonlinear differential equation now called the *balance equation*.

The various steps involved in the development of our ability to cope with Richardson's initial-data problem have been taken gradually during the past 15 years. In theory and in execution they are by no means simple or obvious, and are hardly likely to have evolved without the illumination provided by the quasi-geostrophic model. It is not surprising that Richardson was thwarted in his encounter with this problem!

7. Bjerknes, Margules, and Dines

The perspective of Richardson's book that I have attempted to give would not be adequate without some comments about the conceptual framework in which this work was created.

In the first paragraph of Chapter 1 (quoted in Section 2, above) Richardson refers to a paper on finite differences—his first major work and a governing influence in his subsequent research. In that paper (Richardson, 1910) he lays the foundations for solving certain types of physical problems by numerical methods by adopting finite-difference equations as approximations for partial-differential equations, and by devising an iterative method of successive approximation to the solution of the finite-difference equations—the precursor of important lines of development in this now widely-cultivated field. The spirit of the “relaxation” method, an outgrowth which came into prominence about 20 years later, is portrayed vividly in the following paragraph from this paper (pp. 325–6):

The method of successive approximation to the surface . . . [that represents the solution sought] reminds one of the manufacture of plane metallic surfaces. The initial form of the surface is arbitrary in both cases. The essential things in both cases are a method of testing the work at any stage, a tool with which to alter the surface and judgment in using it. Methods of testing the arithmetic have been described . . . above. Our tools [consisting of successive approximation] and the Fourier method of removing principal modes of vibration may be compared to automatic grinding machines. The use of the handscraping tool corresponds to the adjustment of the numbers according to the judgment of the operator. This is always permissible.

Richardson's interest in approximate methods for solving differential equations was one of the prime motivations for the book, as he himself affirms in the final paragraph of the Preface. This interest originated through a brief term of employment (1906–07) with the National Peat Industries, Ltd, when he studied the flow of water through peat. He retained it throughout his career, and in 1950 published as one of his last papers “A purification method for computing the latent columns of numerical matrices and some integrals of differential equations” which may be regarded as a sequel to the paper of 1910.

Another major influence is identified by Richardson in the Preface:

The extensive researches of V. Bjerknes and his School are pervaded by the idea of using the differential equations for all that they are worth. I read his volumes on *Statics and Kinematics* . . . soon after beginning the present study, and they have exercised a considerable influence throughout it; especially, for example, in the adoption of conventional strata, in the preference for momentum-per-volume rather than of velocity, in the statical treatment of the vertical column, and in the forced vertical motion at the ground. But whereas Prof. Bjerknes employs mostly graphs, I have thought it better to proceed by way of numerical tables. The reason for this is that a previous comparison . . . of the two methods, in dealing with differential equations, had convinced me that the arithmetical procedure is the more exact and the more powerful in coping with otherwise awkward equations. Graphical methods are sometimes elegant when the problem involves irregularly curved boundaries. But the atmospheric boundary, at the earth, nearly coincides with one of the coordinate surfaces, so that graphs would have no advantage over arithmetic in that respect.

In 1913 Richardson was appointed Superintendent of the Meteorological and Magnetic Observatory at Eskdalemuir, an isolated location in the Scottish border country, where he began serious work on numerical weather prediction. In the preceding year V. Bjerknes (1862–1951) was appointed first professor of geophysics at the University of Leipzig, and director of the new Geophysical Institute. There, with the aid of Hesselberg, Sverdrup, Solberg and J. Bjerknes he laid the foundation for a distinguished school and initiated two important series of publications—one of which I have cited above as a source used by Richardson for initial data. On January 8, 1913, Bjerknes delivered an inspiring inaugural lecture for the formal occasion of his installation as professor in the new chair of geophysics. The peroration of this lecture is especially germane here. After reference to the pioneers Ferrel, Guldberg and Mohn, Helmholtz, Hertz and Von Bezold, it begins (Bjerknes, 1914):

All these works antedate the founding of modern aerology. But now that complete observations from an extensive portion of the free air are being published in a regular series, a mighty problem looms before us and we can no longer disregard it. We must apply the equations of theoretical physics not to ideal cases only, but to the actual existing atmospheric conditions as they are revealed by modern observations. These equations contain the laws according to which subsequent atmospheric conditions develop from those that precede them. It is for us to discover a method of practically utilizing the knowledge contained in the equations. From the conditions revealed by the observations we must learn to compute those that will follow. The problem of accurate pre-calculation that was solved for astronomy centuries ago must now be attacked in all earnest for meteorology.

The problem is of huge dimensions. Its solution can only be the result of a long development. An individual investigator will not advance very far, even with his greatest exertions. However, I am convinced that it is not too soon to consider this problem as the objective of our researches. One does not always aim only at what he expects soon to attain. The effort to steer straight toward a distant, possibly unattainable point, serves, nevertheless, to fix one's course. So, in the present case, the far-distant goal will give an invaluable plan of work and research.



Vilhelm F. K. Bjerknes, 1862–1951

Bjerknes concludes with these words:

Before closing I must touch upon an objection which is brought against our work. Our problem is, of course, essentially that of predicting future weather. "But," says our critic, "How can this be of any use? The calculations must require a preposterously long time. Under the most favorable conditions it will take the learned gentlemen perhaps three months to calculate the weather that nature will bring about in three hours. What satisfaction is there in being able to calculate to-morrow's weather if it takes us a year to do it?"

To this I can only reply: I hardly hope to advance even so far as this. I shall be more than happy if I can carry on the work so far that I am able to predict the weather from day to day after many years of calculation. If only the calculations shall agree with the facts, the scientific victory will be won. Meteorology would then have become an exact science, a true physics of the atmosphere. When that point is reached, then the practical results will soon develop.

It may require many years to bore a tunnel through a mountain. Many a laborer may not live to see the cut finished. Nevertheless this will not prevent later comers from riding through the tunnel at express-train speed.

Bjerknes' dream was fulfilled less than 40 years after these prophetic words were written, when the first dynamical forecast was made, in March 1950, on the ENIAC computer in Aberdeen, Md. Then it took the "learned gentlemen" about 24 hours to make a 24-hour forecast; today, the equivalent calculation can be made in about one minute!

Bjerknes' visionary program, so eloquently portrayed by him in Leipzig in 1913, was first enunciated ten years earlier (near the end of his "Stockholm period" when he was professor of mechanics and mathematical physics at the University of Stockholm) in a paper (Bjerknes, 1904) which opens with the declaration:²¹

If it is true, as every scientist believes, that subsequent atmospheric states develop from the preceding ones according to physical law, then it is apparent that the necessary and sufficient conditions for the rational solution of forecasting problems are the following:

1. A sufficiently accurate knowledge of the state of the atmosphere at the initial time.

2. A sufficiently accurate knowledge of the laws according to which one state of the atmosphere develops from another.

It is interesting to note how Bjerknes states his preference for graphical methods:

An exact analytical integration of the system of equations is out of the question. Even the computation of the motion of three mass-points, which influence each other according to a law as simple as that of Newton, exceeds the limits of today's mathematical analysis. Naturally there is no hope of understanding the motion of all the points of the atmosphere, which have far more complicated reactions upon one another. Moreover, the exact analytical solution, even if we could write it down, would not give the result which we need. For to be practical and useful the solution has to have a readily seen, synoptic form and has to omit the countless details which would appear in every exact solution. The prognosis need only deal, therefore, with averages over sizeable distances and time intervals; for example, from degree of meridian to degree of meridian and from hour to hour, but not from millimeter to millimeter or from second to second.

We therefore forego any thought of analytical methods of integration and, instead, pose the problem of weather prediction in the following practical form:

Based upon the observations that have been made, the initial state of the atmosphere is represented by a number of charts which give the distribution of the seven variables from level to level in the atmosphere. With these charts as the starting point, new charts of a similar kind are to be drawn which represent the new state from hour to hour.

For the solution of the problem in this form, graphical or mixed graphical and numerical methods are appropriate, which methods must be derived either from the partial differential equations or from the dynamical-physical principles which are the basis of these equations. There is no reason to doubt, beforehand, that these methods can be worked out. Everything will depend upon whether we can successfully divide, in a suitable way, a total problem of insurmountable difficulty into a number of partial problems of which none is too difficult.

The foundations of graphical analysis and calculation were laid in the second volume of *Dynamic Meteorology and Hydrography* (Bjerknes et al., 1911), and were applied with great vigor in the ensuing years to the first—diagnostic—phase of the Bjerknes program.²² These efforts have had a profound influence on the development of synoptic meteorology. Their first fruit appeared in the Leipzig *Veröffentlichungen* (Erste Serie) and especially in the flowering (1918–1920) of the Bergen School, founded by Bjerknes after his departure from Leipzig in 1917, when the war made it virtually impossible for him to continue his program there. Fifteen years later Bjerknes wrote (Bjerknes et al., 1933, p. 785) (translation):

The main task of the Institute [in Leipzig] was, however, preparation of the synoptic representation of atmospheric

states, which was published in *Serie I* of the Veröffentlichungen of the Institute. The international aerological ascents organized by Hergesell formed the basis for application of the diagnostic methods that had been developed. Especially by means of diagnoses as complete as possible for consecutive series, we hoped to clarify atmospheric processes in order to pave the way for application of dynamic and thermodynamic prognostic methods. By this means the nature of cyclones should ultimately emerge: whether they are or not, indeed, like waves.

In later years the great Norwegian tradition of graphical analysis and calculation was carried on by Petterssen and others, who took the first steps in the second—prognostic—phase of the Bjerknes program. In a historical sense it may be said that this phase of the program culminated in the work of Fjörtoft (1952), on the basis for graphical integration of the barotropic vorticity equation.

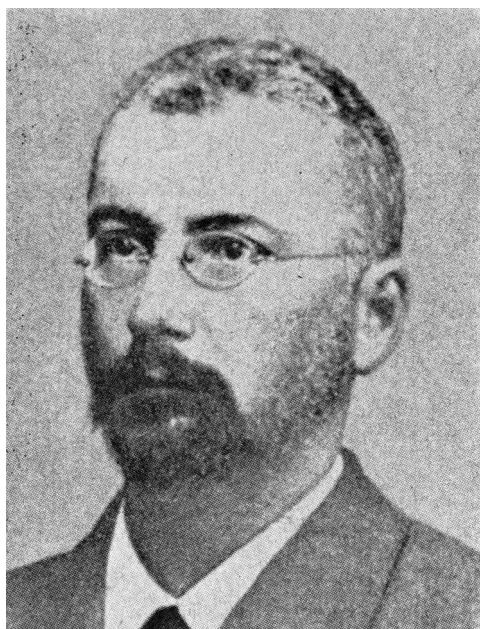
Whereas the work of V. Bjerknes had a predominant influence on Richardson's book, that of another contemporary—namely, Exner—was notable for the contrary reason. Felix M. Exner (1876–1930), professor of geophysics at the University of Vienna, was Hann's successor as director of the Zentralanstalt für Meteorologie und Geodynamik, the Austrian weather service. His *Dynamische Meteorologie*—characterized by Shaw (1931, p. 72) as “the work of an acknowledged master of mathematical method in its application to meteorology”—was the first text book on this subject in the modern sense. The first edition was published in 1917,²³ about midway in Richardson's eight-year period of labor on his own work; the second edition, much enlarged, appeared in 1925.

There is no text-book in the English language which is strictly comparable with Exner's. The dynamical methods followed by Exner, Margules, and others of the Austrian school of meteorologists have not been very widely used in England or the United States, and as a result, English textbooks are either descriptive or physical, rather than mathematical. Thus Exner's book has met a widely felt need among meteorologists, and is one of the few books of which we can say with complete honesty that it is indispensable to any serious student.

This appreciation was written by Brunt (1930).

Richardson makes four references to Exner's book (first edition): one is to the literature citations in Exner's §§ 38–41 on friction and turbulence, the other three are to § 70 where Exner gives his theory of advective pressure change (§§ 75–76 of the second edition). Two of the latter merely concern the use of an exponential form of the pressure; the other is a rather oblique reference to Exner's prognostic method: a serious attempt to develop dynamical methods of weather prediction, in a series of papers published by Exner in the years 1906–1910. In his review of Richardson's book Exner recalled this attempt, but made no claim for its efficacy as compared with Richardson's method.

In relation to Richardson's book the real significance of Exner's *Dynamische Meteorologie* is that it gives the first comprehensive account of Margules' numerous



Max Margules, 1856–1920

fundamental contributions to meteorology. In the foreword to the first edition Exner (1917, p. v) observes that although his book was written just after the appearance of Bjerknes' *Dynamische Meteorologie und Hydrographie* (the German edition, published in 1912–1913), his aim—in contrast to that of Bjerknes—is to give a comprehensive view of theoretical knowledge in the field of meteorology, and a survey of the important literature in this field. Further (translation):

Whereas in the work of Bjerknes and his school the main emphasis is placed on the forces of motion in horizontal planes, the present book brings out especially the role of temperature and its distribution in the atmosphere. This view goes back mainly to the work of Margules, which runs conspicuously through the book like a red thread. It seems to be of such basic significance that its dissemination in wider circles would alone be a satisfactory reward for my labor.

Max Margules (1856–1920) surely must share with Bjerknes the distinction of being one of the founders of modern meteorology. With that, however, the similarity between the two men ends abruptly. As is well known, wherever he established himself academically Bjerknes created a site of intense intellectual activity (as though he were himself a pulsating sphere and his outward-radiating influence the corresponding “field of force”). He is remembered as “the incomparable team leader, the enthusiastic and inspired stimulator, the brilliant lecturer—and the good and kind man with a variety of cultural interests” (Bergeron *et al.*, 1962).

Exner (1920), in a moving obituary, said “Margules war der einsamste Mensch, den ich gekannt habe.” (the loneliest man I have known). Introverted to the point of eccentricity, Margules worked in intellectual isolation and never collaborated with anyone. His formal

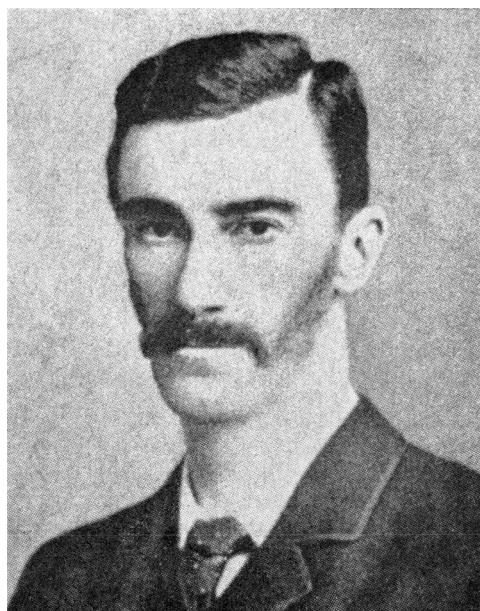
association with meteorology took place entirely in Vienna where from 1882 to 1906 he was an assistant in the Zentralanstalt (from 1890 its secretary). He came into meteorology with a background in physics and chemistry. In fact, he was so preoccupied with chemical researches (he set up a laboratory in his living quarters) that it was not until 1890, at the age of 34, that he published his first work in meteorology, the first of his now-famous papers on atmospheric oscillations. In 1906 at the early age of 50 he retired from meteorological service, turned his back on meteorology, and by means of a small pension settled down near Vienna to devote the rest of his life to his chemical researches.

In an obituary, Gold (1920) said "The present writer was saddened to see him there [in retirement] in 1909 entirely divorced from the subject of which he had made himself a master. Meteorology lost him some 15 years ago, and is for ever the poorer for a loss which one feels might and ought to have been prevented."

With the onset of the war and deterioration of the European economy, Margules' pension became inadequate to allow him to continue his work, and in the aftermath of the war was insufficient even for his bare subsistence as a bachelor. (His pension was 400 crowns a month, equivalent in 1920 to about \$2!) In 1919 the Austrian meteorological society awarded him the Hann silver medal; characteristically, he declined the honorarium that accompanied it. Despite repeated offers of assistance from Exner and others, Margules inexorably and stoically accepted his fate and—incredible though it may be—died of starvation in 1920.

Of the 16 papers listed by Exner (1920) as the most important of Margules' meteorological work, only one need concern us here: "Über die Beziehung zwischen Barometerschwankungen und Kontinuitätsgleichung" (Margules, 1904). In this five-page paper Margules derives the pressure-tendency equation and from it, shows that a horizontal gradient of the vertically-averaged wind as small as 4 cm sec^{-1} in 100 km can give a surface-pressure change of more than 1 mb hr^{-1} . Here is the germ of the compensation principle, and a clear-cut means of understanding (albeit not circumventing) the failure of Richardson's forecast. Margules' paper is summarized very clearly and completely by Exner (1917, §31) in the first edition of *Dynamische Meteorologie*; but Richardson—although he cites Exner's book, as noted above—does not give any indication that he is aware of the relevance or even of the existence of this paper.²⁴ Had he taken account of its implications, he surely would have dealt with his initial data in a different way, or perhaps have had second thoughts about his model (see Note 16).

William H. Dines (1855–1927), more than any other single person, is responsible for the notable contributions of Great Britain to the early exploration of the upper air.²⁵ From 1914 to 1922 Dines' home and 'observatory' were at Benson, Oxfordshire, where in 1919–20 he was joined by Richardson. In the Preface to



William Henry Dines, 1855–1927

the book, Richardson thanks Dines "for his interest in some early arithmetical experiments" and for his discussion of "hypotheses" later at Benson. There, perhaps at Dines' suggestion, Richardson added the introductory example (Chapter 2), at the end of which (p. 15) he says he is indebted to Dines "for having read and criticised the manuscript of this chapter." The very substantial treatment of radiation in the book probably owes much to the collaboration with Dines which took place on this subject at Benson. This was the principal subject to which Dines had turned in the closing years of his scientific career. A major paper "Atmospheric and terrestrial radiation" by Dines in 1920 is based upon the method of calculation given by Richardson in his book.

Dines was a highly versatile scientist and was not content to leave to others the interpretation of the upper-air data he collected. His first major publication in this direction appeared in 1911: "The vertical temperature distribution in the atmosphere over England, and some remarks on the general and local circulation." (The papers to which I refer in this paragraph are contained in the selection published by the Royal Meteorological Society (Dines, 1931).) In this paper clear evidence is presented for the fundamental principle of thermal compensation (cold troposphere, warm stratosphere and *vice versa*) in cyclones and anticyclones, which was anticipated to some extent by Hann and others, but which often is associated with the name of Dines. The principle of dynamic compensation (convergence into the lower layers of a cyclone, divergence aloft) also is used in this paper, in an attempt to arrive at a consistent picture of the characteristic structure of low- and high-pressure systems in mid-latitudes. A year later Dines published "Further contributions to the investigation of the upper air. Total and partial correlation coeffi-

cients between sundry variables of the upper air," the first of numerous papers in which the data were subjected to more refined analysis by statistical methods, culminating in 1919 in "The characteristics of the free atmosphere." Of the latter paper his son L. H. G. Dines said: "It shows the remarkable manner in which he had in the space of a comparatively few years removed the reproach that England was a backward country in the prosecution of upper air research." (See Dines, 1931, p. 119.)

The copious references in Richardson's book to Dines's work, and the close personal association between the two men in the years 1919–20, make abundantly clear that—in contrast to the evidence in regard to Margules—Richardson was well informed as to the main features of Dines's investigations of the upper air and their possible relevance to his own work on weather prediction. Moreover, in many papers during the period 1910–20 Dines set forth and refined his views of thermal and dynamic compensation. However, in order to be applicable to Richardson's problem these ideas had to be joined to Margules' pressure-tendency equation, and this is a step that Richardson did not take. Indeed, it is a step that was not made decisively until the work of J. Bjerknes and Sutcliffe, more than 20 years later.

8. Epilogue

It is commonplace to say that a great idea does not bear fruit unless advanced at a time set to favor its entry upon the stage of history. So it was with Richardson's book, which failed to have a significant influence on the progress of meteorology in the decades that followed. In fact, interest in the book is more widespread today than it was in the quarter century 1923–1948 when it might have led to a new branch of meteorological research as a prospectus of things to come.

There can be little doubt that the failure of Richardson's ideas to germinate must be ascribed chiefly to the fact that in 1922 there existed neither an adequate network for upper-air observations nor a high-speed automatic computer.²⁶ Many years later, after both of these developments had emerged, Richardson (1949) himself gave a retrospective view:

. . . This research was begun in 1913 as an exercise on finite differences suggested by Philos. Trans. A., Vol. 210, pp. 307–357; but it became the unifying idea to which all my other meteorological work was attached. The book shows how all the then known atmospheric processes can be fitted together into a lattice-reproducing numerical system. In 1922 numerical prediction appeared to be impossible for four reasons: (i) The slowness of arithmetic. Modern electronic machines have provided the requisite speed. (ii) Winds above clouds were unexplored. They are now regularly observed by radio-sondes and could be by spheres shot upwards. (iii) Inadequate statistical information about eddies, about reflectivity, about water in clouds, and about ozone. Much more is now known about each of these. (iv) The random location of observatories. They are still not arranged in the appropriate pattern, which is like a chessboard, where pressure should be observed above the centre of each black chequer, and wind above the centre of each white chequer.

Notwithstanding these obvious prerequisites for a practical implementation of Richardson's program, we should not forget the forecast of 145.1 mb change of surface pressure in six hours, which must have engendered incredulity in the minds of his colleagues (and doubtless also was a source of disillusionment for Richardson himself). As for the professional forecaster, who we can imagine was predisposed to be skeptical of a scheme that was as drastically unconventional as it was difficult to comprehend or apply, Richardson's forecast served to justify adherence to traditional methods, and thus to harden the breach between theory and practice. It is difficult to resist the conclusion that this single result, 145 mb in 6 hr, which Richardson himself described as a "glaring error," was the hinge which closed the door to further speculation about numerical weather prediction for 25 years, until the electronic computer made re-examination of the problem inevitable.

These facts must be recorded as the framework of our analysis, but we must look beyond them for deeper insight into the historical processes at work. I have already described the contemporary reviews from which we can see that Richardson's book created widespread interest. However, this interest appears to have been ephemeral, and did not lead to significant consequences in the scientific literature. To illustrate this point I shall refer to six major works in which a discussion of Richardson's forecasting scheme would have found a logical place. All were published in the decade 1925–1935 and each has become a classic of meteorological science. Proceeding chronologically, we have first Exner's *Dynamische Meteorologie*, the second edition of which appeared in 1925. There are several references by Exner to Richardson's work on turbulence, but concerning the book on weather prediction he says, in the foreword (Exner, 1925, p. v) (translation):

. . . we can distinguish today between essentially three directions in dynamic meteorology. The one set forth here, which comes from the Austrian and Bjerknes schools, has already been characterized; parallel to this there exists a line of work in England, more concerned with the details of air motions in connection with the study of turbulence, which in part had originated there. The work of L. F. Richardson, *Weather Prediction by Numerical Process*, represents this direction to a large extent. I have not succeeded in incorporating this valuable but difficult work in the new edition: the general formulation of the problem is too different. As a third direction I should like to characterize that which continues the completely theoretical approach of V. Bjerknes' Leipzig period and today is represented especially in Russia.

I believe that Exner was sympathetic to Richardson's work and sensed its significance, but was unwilling to cope with its complexities.

The first modern textbook of theoretical meteorology written in America was Humphreys' *Physics of the Air*, first edition 1920, second 1929, third 1940. In the second edition Richardson's name appears once in a casual way; there is no reference to the book on weather prediction. In sharp contrast is Shaw's treatment of Richardson's book in the monumental *Manual of Meteorology*, no-

tably in the third volume 1930 and in the fourth 1931. Shaw was unquestionably the principal advocate of Richardson's meteorological work. He supported it from the beginning, and in the years 1919–20 when Richardson was at Benson Observatory in charge "of experiments in the computation of the sequence of weather by numerical process." The numerous references to the book on weather prediction that appear in Volumes III and IV of Shaw's *Manual* plainly indicate a genuine desire to do justice to this infrangible material. However, Shaw's bent of mind was not toward theory, and he was then nearing the age of eighty. Thus it is not surprising that we do not find in the *Manual* a connected account of Richardson's method.

Shaw's sympathetic yet realistic attitude toward the numerical approach to forecasting is set forth very well in his earlier book *Forecasting Weather*. In a four-page chapter titled "Weather prediction by numerical process"—which first appeared in the third edition published just a year after Richardson's book—Shaw very briefly describes the salient features of Richardson's method and concludes with the following paragraphs (Shaw, 1923). (I quote these partly because of the incisive simile in the third paragraph, which modern practitioners of numerical forecasting might well ponder!)

The process as thus described in its present form is not likely to be adopted by official forecasters, and we cannot offer it to the reader as Richardson's "Ready Reckoner" for forecasts. It has been playfully said that on this method it would take a year to forecast to-morrow's weather from to-day. But the effort to bring the processes of weather under numerical computation is by no means wasted. In the course of mapping out the computation, as in no other way, the dynamics and physics of many of the processes of weather are made clear, and a very large amount of information about the atmosphere, difficult to acquire, is contained in the book.

The introduction of physical and mechanical computation into the study of the atmosphere remains an urgent requirement, and perhaps the best course is to devise some form of grouping of facts into systems which will economise the labour of computation.

If visitors from Mars, unfamiliar with engines and, at the same time, familiar with Cartesian co-ordinates, happened upon the engine room of some big ship, they might endeavour to unravel the mystery of that great development of power by dividing the space occupied by the machinery into points arranged according to co-ordinate axes. If they did so it would take a vast amount of computation to arrive at an idea of the constituent parts of the machinery which would certainly be disclosed to them in time if they examined enough points. In the end they might, of course, know more about it than the hasty visitor who recognised a turbine or a cylinder or a shaft; but as working knowledge what they knew would be less useful.

It is possible to carry the process of minute analysis too far. There is a story of a conversation between Maxwell and Tyndall which may or may not be in print. Talking of the ultimate molecular constitution of matter Tyndall pointed out to Maxwell how curious it was that the pulling force depended on the mutual attraction of separate molecules, and said that the traces by which a horse drew a cart were not really continuous, but were held together by attraction at a distance. "Yes," said Maxwell, "but when you are in the cart it is very comforting to know that the traces are there." In like manner, the grouping of the phenomena of the atmo-

sphere into entities, which are sufficiently persistent to be dealt with as working units in selected situations and which can be recognized in maps, remains as a possible alternative to a generalised system which is equally applicable to every meteorological situation and at all times. The application of Mr. Richardson's method to such selected types should prove very illuminating.

The published evidence in regard to V. Bjerknes' attitude toward Richardson's book is meager. The logical place for an assessment of what Richardson had done was in *Physikalische Hydrodynamik*, published in 1933, but there we find that Richardson's book is mentioned only in the bibliography, historically annotated by V. Bjerknes. In describing the work of the Bergen school, and its emphasis on air-mass analysis, Bjerknes comments that (Bjerknes *et al.*, 1933, p. 787) (translation):

This was, however, no less than the realization of the program for weather prediction set forth in 1904, based upon the laws of mechanics and physics . . . , except that a daily weather service must substitute rapid approximations for detailed calculations. Now everything is so well worked out that in time the rough estimates will be replaced by ever more precise calculations; and that is especially so since in the meantime an extraordinarily remarkable work by L. F. Richardson has appeared in this field.

In spite of this indication of a favorable attitude toward Richardson's work, Bjerknes himself seems to have turned away from a direct confrontation with the program of 1904, and apparently did not respond vigorously to the bold step that Richardson had taken toward the fulfillment of that program. Thus, the Norwegian school did not attempt to build upon Richardson's work—a fact that seems to me to be significant historically, because more than any other man of the time, Bjerknes was in a position to stimulate others to follow the path opened up by Richardson. On the other hand, Bjerknes and his collaborators in Bergen were preoccupied with fashioning the new tools of air-mass and frontal analysis, which in the decades that followed brought about the greatest advances in weather forecasting since the advent of the telegraph. There can be no doubt that by aiming in that direction, Bjerknes made a much greater contribution to forecasting than would have been possible had he single-mindedly pursued his program of 1904.²⁷

The *Dynamische Meteorologie* by Koschmieder (1933) mentions Richardson's work only in the brief concluding section. After referring to three categories of atmospheric dynamics (horizontal equilibrium flow, perturbation theory, and line or space integrals) Koschmieder says (p. 357, translation):

A fourth, important group is missing: integrals over time, which from an initial state yield the spatial distribution of the meteorological elements at a later time. This fundamental problem of dynamic meteorology, probably first recognized clearly by V. Bjerknes . . . [the 1904 program], still . . . encounters insurmountable difficulties. It has been tackled for the first time by L. F. Richardson The success of the single case treated by him is in no satisfactory proportion to the computational effort expended. Never-

theless, further work must be joined to this fundamental investigation, whether one is content with differential expressions for singular points, as Angervo . . . and similarly Wagemann . . . have proposed recently, or whether one seeks mechanical analogies—a route that is not at all unpromising.
...

Finally I must mention Brunt's *Physical and Dynamical Meteorology*, one of the most authoritative and widely-read English-language works of its kind. The first edition of Brunt's treatise appeared in 1934, and contains several references to Richardson's work on turbulence. The book on weather prediction is mentioned once, in an appended list of supplementary references; but it is not cited anywhere in the second edition of 1939. In 1934, under Brunt's editorship the Royal Meteorological Society published a collection of papers (from the *Quarterly Journal*) under the title "Some problems of modern meteorology." The Introduction, by Brunt, does not mention Richardson and could logically have done so. In the 16 papers of this collection, the only one that directly deals with short-range forecasting is by Douglas, who says (Douglas, 1931, p. 252): "Mention should be made of Richardson's gallant effort to work out a scheme designed to place weather forecasting on an exact basis. Unfortunately the attempt must be classed as a splendid failure, owing to the complexity of the whole problem and particularly the difficulty of dealing with small irregularities."

The influential works I have mentioned eschewed any serious confrontation with Richardson's method of weather prediction, but it must also be said that Richardson himself took few steps to arouse the interest of his contemporaries by sustaining the attack which he had initiated so boldly. To understand this reticence on Richardson's part, we must recall that for pacifist reasons Richardson resigned from the Meteorological Office in 1920, when that office came under the administration of the Air Ministry. His subsequent positions, in charge of the physics department at Westminster Training College (1920–29) and as Principal of Paisley Technical College (1929–40) doubtless hindered his access to the mainstream of meteorological activities. Why did he not take a University position, from which he could have been more effective scientifically? In this connection I quote again from the note by Richardson's wife:⁴

Lewis Richardson's stand as a C. O. in the first world war disqualified him, according to the then-prevalent ideas, from University teaching. He just accepted this, never "explaining" his decision unless definitely asked. He would never "make excuses" for himself. He never became a University Professor. In 1940, when he was 59, a Professorship was offered him, the Governors being willing to wait a whole month for his decision. Again a difficult choice had to be made. He refused the post with very real regret, for he had planned to do research into the causes and results of wars of all kinds during the past 130 years—a research which only a Psychologist and Mathematician and Statistician could possibly do—and a man with the open mind and painstaking accuracy of a true scientist. . . . To this difficult, thankless and exhausting task he devoted all his skill and all his strength for the last 13 years of his life."

As Ashford (1949) has said, concerning this second phase of Richardson's career, ". . . meteorology was a temptation to be resisted, so that his thoughts could be concentrated on his service to world peace."

Thus, after 1930 Richardson dedicated his life to a mathematical study of the causes of war. The nature of this dedication, which stemmed from strong moral conviction, is illuminated by the following paragraph from the obituary by Gold (1954), which contains extracts from Richardson's own biographic notes (made at age 71):

Naturally the Society of Friends was a persistent influence, 'with its solemn emphasis on public and private duty . . . its condemnation of war pulled me away from the many warlike applications of physics.' His earliest paper relevant to the avoidance of war was written when he was with the motor ambulance convoy in France. 'There was no learned society to which I dared to offer so unconventional a work (*Mathematical Psychology of War*). Therefore I had 300 copies made by multigraph, at a cost of about £35, and gave them nearly all away. It was little noticed. Some of my friends thought it funny. But for me it was quite serious, and was the beginning of the investigations on the causes of wars which now occupy me in my retirement.'

It would be out of place to comment in any detail here about the substance or scope of Richardson's studies on war, nor am I qualified to do so. (The interested reader should consult the comprehensive analysis by Rapoport (1957), or the book review by Sutton (1961).) However, I shall call attention to some interesting similarities in the style of this work as compared with that of the book on weather prediction.

In spite of the abstract, mathematical qualities of the book on weather prediction, there can be no doubt that Richardson resolutely aimed this work toward direct, practical application. The Computing Forms alone testify eloquently to this fact. Doubtless his concern for a practical outcome was partly an expression of the Quaker emphasis on 'public duty' (above quotation), for it is evident also in his work on the causes of war. The book *Arms and Insecurity*, published posthumously, was edited by Rashevsky and Trucco who make the following observations (Richardson, 1960, p. ix):

. . . How strongly Richardson felt about the immediate practical importance of his work is illustrated by the following story, for the authenticity of which the editor cannot vouch but which he heard from a responsible source and has every reason to believe to be true.

In 1939, when the clouds of the Second World War were ominously gathering over Europe, an American journal received for publication a paper from Lewis F. Richardson. The paper contained the essentials of chapters ii and iii of the present book, as well as fractions of other chapters. In his letter of transmittal Richardson urged that the paper be published immediately because its publication might avert an impending war. The editors of the journal not only did not rush the paper but rejected it.

Nobody will of course entertain even for a moment the thought that the Second World War would not have occurred had the paper been published in time. However, the story, whether true or not, illustrates Richardson's strong belief that scientific knowledge may prevent disasters to

humanity. We think that in this basic belief he was right.

Of the book on weather prediction Dr. Chapman (1922) said "The enterprise contemplated in this book is of almost quixotic boldness." This aptly characterizes the audacity with which Richardson set forth on a venture so bold that some of his contemporaries understandably tended to look upon it as a tilt at windmills. Similar attributes appear in his work on war. Rashevsky and Trucco quote Boulding, who put the matter in this way, in reference to *Arms and Insecurity* (Richardson, 1960, p. vii):

. . . He is a man inspired with a vision of orderly relations in a field where disorder is usually regarded as supreme. Not content with the vision only, he seeks to bend what empirical data he can lay his hands on into a quantitative verification of the relations which he postulates. He does this with dash and courage, but naturally, in view of the grave deficiencies in the data, with only a limited success. The important thing however is that it should have been tried; future generations will be able to run where he stumbled.

To Boulding's assessment they add (pp. viii–ix):

Suppose for a moment, just for the sake of illustration, that not only the socioeconomic data used by Richardson were found to be inadequate, but that entirely new factors and assumptions would have to be introduced in his theory and that his basic equations would have to be changed beyond recognition. Would this affect the value of Richardson's work? The answer is an emphatic NO. The value of this work lies not in the particular formulation of his theory but in the fact that Richardson shows how the problems of the causes of war can be subject to mathematical treatment and to rigorous mathematical thought. . . . Richardson's equations will be changed by future investigators, some of his conclusions will be abandoned, but his work will remain forever as the first step made in the right direction, namely, the putting of the study of war on a rigorous basis of mathematical reasoning. Whatever the shortcomings of this book, it will have to be studied by every investigator who delves into the causes and origins of war. This work is a starting point for the development of a new branch of sociology.

The reader will recognize that these comments are remarkably pertinent to the book on weather prediction!

In his own biographical notes, quoted by Gold (1954), Richardson says that the tendency of his mental machine *almost* to run of its own accord made possible

. . . an intentionally guided dreaming. If the machine ran *entirely* of its own accord, one would have no control, and the dream would be out of touch with reality . . . It is the "almost" condition that is advantageous for creative thinking. . . . In some ways it is a nuisance; for example I am a bad listener because I am distracted by thoughts; and I was a bad motor-driver because at times I saw my dream instead of the traffic.

Concerning his inclination for solitude a son recalls (Richardson, 1957) that "In preparing his entry for *Who's Who*, my father asked the family if he could answer the question about hobbies and sports with the one word 'solitude.'" Richardson himself (quoted by Gold, 1954) tells us

. . . I hardly know what loneliness feels like: when solitary I am usually serene; when in a crowd, I am often embarrassed. . . . After I had become a professional scientist and when I met obvious leaders, my usual tendency was to shy away from them. . . . Gregarious readers will probably think such a standoffish temperament is a deplorable fault and an unmitigated disadvantage. It certainly does prevent its possessor from collecting disciples and founding a "school." Let me, however, point out that the same solitary temperament enables him to persist in unpopular researches such as those on the causes of wars.

Thus we see in the work of Lewis F. Richardson the imprint of a brilliant and visionary innovator who perceived and mapped the outlines of hitherto uncharted territories in meteorology and in sociology, who brought to each of these unsung labors the same intense zeal for service to mankind as was characteristic of his personal life, and who had neither the professional position nor the traits of personality nor the attentive ear of history that are the means by which other men can influence strongly the contemporary directions of intellectual activity. Sheppard (1953) aptly recalls Wordsworth on Newton: ". . . a mind for ever/Voyaging through strange seas of Thought, alone."

I shall close this essay with a glimpse of Richardson through the eyes of two who knew him intimately: again from the tribute to her husband written by Mrs. Dorothy Richardson:⁴

One of Lewis's sayings was "Our job in life is to make things better and easier for those who follow us. What happens to ourselves afterwards is not our concern." In nearly fifty years I never saw him other than "strong and very courageous," following the light as he saw it and reaching out a helping hand to all who came within his influence. A fellow undergraduate at King's College, Cambridge, writes, "Lewis was a rock and flew his colours with superb, audacious gallantry." This is a perfect picture of him in his youth. In later years the "audaciousness" became an understanding gentleness, but the "superb gallantry" increased.

And finally, from a biographical note by Richardson's son Stephen A. Richardson (1957):

The quality of my father which for me is outstanding was his courage to stand by his convictions in his actions and in his research. His research into the causes of war received little understanding or recognition and some ridicule until the last few years of his life. Sustained pursuit of a vision under these conditions is not easy. Nevertheless, he searched on for the goal he envisioned with tenacity and audacity. His isolation was increased because he moved freely across the boundaries of traditional disciplines.

Notes

1. Apart from contemporary reviews, a paper by Vernić (1948), written in the Croatian language, is to my knowledge the only attempt at a critical analysis of the book. I do not have a written translation of this paper, but was fortunate in receiving an oral translation from Mrs. Vita Wackerling. Vernić's appraisal is distinctly adverse but on the whole quite sound. He does not seem to recognize the importance of the vertical-velocity equation; but writing in 1948 he had none of the advantages

of hindsight with respect to the modern development just then beginning.

2. The only other complaint I have is a minor one which concerns pagination of the preliminaries. Since the new introduction is placed ahead of the original preface, pagination of the preface is not the same in the reprint as in the original edition. This creates some inconsistencies with the index, which could have been avoided had the original pagination of preliminaries been retained (e.g., by showing the new pagination as drop folios).

3. Many errata are recorded in the 'revision' file, but the most important are in the two published lists. A few that did not find their way to those lists are: p. 11, upper left corner of table, *for Coeff. 0 read Coeff. of*; p. 46, *for 5 microns read 3 microns*; p. 177, in heading of Ch. 8/2/20, *for volume read area*.

4. I am indebted to Professor Henry Stommel for bringing to my attention the tribute by Mrs. Dorothy Richardson, which appeared in the Paisley and Renfrewshire Gazette, 3 October 1953. Professor Stommel has also given me an account of his collaboration with Richardson in the joint paper published in 1948, which marked Richardson's return to geophysics in the twilight of his career:

My visit to him in Kilmun came about in a fairly simple direct manner. I was spending several months in England at the Imperial College trying to pick up enough knowledge of relaxation methods under some of Southwell's assistants so that I could do a numerical theory of the semi-diurnal tide of the ocean in a realistic shape ocean basin. It came to nothing. Near the end of the visit—early in January 1948—I wrote a letter to Richardson telling him about my interest in the ocean and rather naively asking if I could visit him. I admired him, and was curious about him as a person. I have rather a strong dislike for wars, and I gathered that he did too, and you know there are really very few people in the world who really feel very strongly about such things so that it is very difficult to discuss the matter with most people. Richardson wrote back saying please do come for a visit, and to stay at his house, and that we might do an experiment—and could I obtain a number of golf balls? Shortly thereafter a second letter came from him saying "never mind the golf balls—they sink." I had no idea what the experiment was going to be, but on my arrival at his house he took me out in the rain on the hill behind and we dug parsnips for the diffusion experiment. The optical measuring gear we built in his home shop which occupied much of the first floor. He suffered from a rather bad palsy but nevertheless was quite a competent handyman at building devices. Incidentally his homelife seemed quite serene and happy, and I stayed for several days, went bicycle riding around the country with him and his children, and never felt pressed to entertain nor felt to be an intruder. The second floor of the house was also largely taken up by his interests: there was a wonderful large study in the front of the house at the head of the stairs such as you might imagine Darwin, or Goethe, or some other leisurely genius of bygone days with expansive interests might possess.

5. In addition to the subventions of £100 + £50 that Richardson mentions in the Preface, the author himself contributed £50. The text gave much scope to the publisher, Cambridge University Press, to display its renowned craftsmanship, and called forth some impressive

feats of typesetting. The original edition, of which 750 copies were printed, was priced at 30 shillings (then about \$7.50) and of course is out of print. Sales of the book—to quote from a letter received from Cambridge University Press—"were certainly not spectacular," owing partly perhaps to the fact noted by an anonymous reviewer writing in the *Methodist Recorder* (February 9, 1922), that "Arrangements have been made by the Meteorological Office to forward a copy of the book to each of the National Observatories."

6. The computing forms were available separately "to assist anyone who wishes to make . . . experimental forecasts" (Preface): *Forms whereon to write the numerical calculations described in "Weather Prediction by Numerical Process,"* by Lewis F. Richardson. Cambridge, University Press, 1922, 23 forms. (Publication of these forms was subsidized entirely by Richardson.)

7. The reader may wish to know the ages of the reviewers whom I shall cite, at the time of publication of the book, 1922: Exner 46, McAdie 59, Woolard 23, Chapman 34, Jeffreys 31, Whipple 46, Shaw 68. Richardson's age was 41.

8. Dr. Woolard (recently retired as Director of the Nautical Almanac Office, N. S. Naval Observatory) has given me the opinion that "There was little patience or sympathy at that time with attempts to construct mathematical theories of weather phenomena, or to apply physical and mathematical methods to forecasting."

9. In addition to the four from which I have selected excerpts for quotation, I am aware of only one other English review (apart from newspaper notices): by Nelson K. Johnson (1922).

10. I am indebted to Dr. Jeffreys for the following recollection:

I knew Richardson quite well. One of my memories is of crossing the Atlantic in 1924 to the British Association Meeting in Toronto, on the old Caronia. He was in a party of meteorologists, who made measures of temperature up the mast and various other things. One day L. F. did not turn up to breakfast and I went along to see him. He was prostrate in his berth, with just enough strength to pull the plugs out of a resistance box to read a platinum resistance thermometer.

11. In the 'revision' file (see Foreword) are letters received by Richardson from Shaw, Dines and Simpson, to each of whom he had sent a complimentary copy of the book. These letters are an interesting supplement to the formal reviews quoted in the text above. Shaw wrote (February 2, 1922):

Dear Richardson

Let me congratulate you on the appearance of the book and thank you for sending me a copy. I hope you won't feel at a loose end now that the work is actually out and cannot be improved: but I expect you will discover in it many buds which can be developed into branches and I hope that you will find time and opportunity and, I think I must add, money for their cultivation. It is a very notable addition to British meteorological literature. I hope it will convince our Physical and Mathematical friends that the atmosphere does give one cause to think.

Yours very sincerely
Napier Shaw

Dines wrote, in part (February 5, 1922):

Dear Mr Richardson

Many thanks for the book of which you have so kindly sent me a copy. As you know I read it in the proof form save those parts in which the mathematics are beyond me but I am very glad to have it as a connected whole and I am reading it again with much interest. I think it will be a standard work on advanced meteorology for some time to come.

Yours sincerely
W. H. Dines

Simpson, then Director of the Meteorological Office wrote, in part (February 6, 1922):

Dear Richardson,

I am extremely grateful to you for sending me a copy of your book "Weather Prediction by Numerical Process." It is much easier now to see the importance of your work than it was when I saw it in manuscript. I cannot say that I have read it through, but you have done an extremely useful piece of work in formulating in mathematical language our meteorological problems and in writing down the equations necessary to tackle them. That we are not likely to predict the weather by numerical process for a long time to come does not detract from the value of your work.

Yours sincerely,
G. C. Simpson

12. The pivotal significance of the W -equation is depicted by Richardson in two undated manuscript sheets (headed "Review of W. P. by N. P.") inserted before the half-title in the 'revision' file.

The problem as it presented itself to me may be explained by a simile. Imagine that you receive the parts of a machine. There are wheels, levers, casings and a hundred and one pieces that you do not recognise. Many of them are beautifully made and finished. A few are rough castings. You believe that they fit together into a machine, but you do not know what shape the machine is and the parts are not labelled. These parts symbolize the existing pieces of meteorological theory, as found in the literature. I collect those that appear to be essential to the machine in Ch. 4.

Now, to resume the simile, you begin trying what will fit onto what and so you build up a machine. Then comes the question of fuel to drive it—the observations. Then you try to get it to go round—and that reminds me of starting a motor car at -15°C . You turn and turn the handle until your back aches and nothing happens. Then on a renewed effort the engine makes a noise like "hou" and turns once around. "Continuez" says the Frenchman, "elle a dit oui." That is the stage we have got to, the wheels have gone once round, and that prompts us to continue.

Withal it seems a fantastic structure. That enormous vertical velocity equation which corresponds to the connecting rod for transmitting the power from the cylinders to the wheels. It is the sort of connecting rod that Heath Robinson would delight to draw. And yet any connecting rod, even an ungainly one, is better than no connecting rod at all. And I am afraid there are to be found theories which omit this necessary link.

It is ironic that Richardson's vertical-velocity equation did not find its way into the modern development of numerical weather prediction. The reason for this is that pressure, rather than height, was used as vertical coordinate throughout the modern development. However, within the past two years Dr. A. Kasahara and Dr. W. Washington at the National Center for Atmospheric

Research (Boulder, Colorado) have constructed a numerical model of the general circulation on the basis of Richardson's equation. Their model is running successfully on the NCAR computer.

13. This aspect of the Richardson lattice can be understood more clearly by omitting the Coriolis-force terms from the U , V equations (a contribution by V to change of U and by U to change of V). Then a "lattice-reproducing process" can be built from either of two elementary lattices each of which consists of only half the variables shown in Fig. 3, namely those without the asterisk or those with the asterisk. This makes it clear that the original arrangement can be regarded as consisting of two elementary lattices coupled by Coriolis force. Moreover, in an elementary Richardson lattice each square of the chessboard can be occupied in one of eight ways: by U , V , P or a blank, in either the left or right side; therefore, a given configuration of cells in the two horizontal dimensions and in time can be occupied by eight independent elementary lattices. These can be grouped as four coupled pairs, so the same cells can be occupied by four independent Richardson lattices when Coriolis forces are included. The existence of such independent lattices was pointed out by Richardson (p. 9): "If we had begun with . . . [P , U , V] all three tabulated in every square, the distribution might have been regarded as two interpenetrating chessboard patterns. In the subsequent steps these interpenetrating systems would have been propagated quite independently of each other." (He is thinking only of the two space dimensions—not the time dimension—hence visualizes two rather than four independent lattices.)

14. In the 'revision' file there is a letter from F. J. W. Whipple dated November 23, 1922 which begins:

Dear Richardson

I have succeeded in writing out a fairly straightforward proof of a relation between northward drift and temperature changes in the stratosphere. I am sorry to say that in so doing I have come across what seems to me to be a slip in your work so that my equation contains a term which you omitted. . . .

To this Richardson replied (November 29, 1922):

My dear Whipple

Many thanks for your letter of 23rd. I regret very much that errors have occurred, but it is good in every way that they are found out. With regard to your points, I see that I must plead guilty to having omitted the term $\gamma_p \partial \theta / \partial h \partial t$ from the first member of Ch. 6/7/2 #10. . . .

Whipple explicitly gives the consequences to this error, but I find no evidence that Richardson himself attended to it.

15. This seems an appropriate place for a bit of Richardsonian whimsy found (inserted between text pages 94 and 95) among the manuscript materials in the 'revision' file. It illuminates his assertion that "The scheme is complicated because the atmosphere is complicated."

When we were schoolboys and the answer to an arithmetical problem came out as a whole number, we felt sure that we had got it right. The idea that simplicity implies rightness is not confined to schoolboys.

Einstein has somewhere remarked that he was guided towards his discoveries partly by the notion that the important laws of physics were really simple. R. H. Fowler has been heard to remark that, of two formulae, the more elegant is the more likely to be true. Dirac very recently sought an explanation alternative to that of spin in the electron because he felt that Nature could not have arranged it in so complicated a way (R. S. Meeting 1928 Feb 9).

These mathematicians have been brilliantly successful in dealing with mass-points and point-charges. If they would condescend to attend to meteorology the subject might be greatly enriched. But I suspect that they would have to abandon the idea that truth is really simple.

16. In the 'revision' file there is a manuscript sheet so remarkable that it is reproduced here in facsimile (Fig. 8). This sheet is inserted after page 22 of the printed text, where Richardson discusses his choice of prediction equations. Before commenting on the manuscript sheet, I shall quote briefly from pp. 21-22:

One of the first questions which had to be decided was whether to eliminate any of the dependent variables before proceeding to the numerical process. Now if a variable be eliminated between two differential equations the resulting equation is usually more complicated, so that the saving in arithmetical toil due to the absence of a variable, is partly or entirely compensated by the increase in toil due to the complication. There is also a clear advantage in keeping to the familiar variables which are observed, to the avoidance of stream-functions and other quantities which cannot be observed. Therefore I decided to do without analytical preparation of this kind except in two cases:

(i) To eliminate temperature between the characteristic equation . . . [the ideal-gas law] and any other equations in which . . . [temperature] occurs. This introduces no complications, because the characteristic equation is not differential.

(ii) To solve for the vertical velocity. This is necessary, because sufficient observations of the vertical velocity are not available, nor likely to become so. The solution can be obtained because the vertical equilibrium can be treated as a static one, that is to say . . . [the vertical acceleration] can be neglected. It is done in Ch. 5.

These considerations led, of course, to the primitive, hydrostatic equations: it is in the light of them that we must interpret the manuscript sheet on which Richardson noted what he considered "the most important change to be made in the second edition."

The significance of Fig. 8 is two-fold: first, it shows that after publication of the book, Richardson came to realize more clearly that observed winds must not be used in the pressure-tendency equation; second, it shows that he had been thinking along lines remarkably similar to those that later led Charney and others to the quasi-geostrophic prediction equations. The evidence for this latter point is contained under "II," where he says "It is . . . [the velocity divergence] that we have to eliminate" and where at the end, in reference to choice of the two scalars that must be retained for specifying the three-dimensional velocity, he says ". . . [the vertical component of vorticity], and . . . [the vertical velocity] would be an interesting pair . . ." It may seem to us incredible that Richardson did not pursue these ideas, but we must remember that after 1929 he set aside all serious work in meteorology in order to dedicate himself to his mathematical studies of war.

Regrettably, the sheet reproduced in Fig. 8 is not dated; but attached to printed page 1 in the file is a copy of the following letter from Richardson to Dr. C. L. Godske, which proves Fig. 8 to have been written before the date of this letter, September 25, 1936:

Dear Dr Godske

1936 Sept 25

In preparation for your visit tonight I have put in your bedroom:

(i) A copy of my book in the form of loose sheets. Please accept this for annotation.

(ii) A bundle containing 65 computing forms. I shall be pleased to give you as many of these as you like to carry.

(iii) A red binder containing my annotated copy of W. P. by N. P. with a view to a second edition. I am not willing to part with any of the papers in this binder; but you are welcome to copy anything you like out of it. The parts that, I suggest, are worth copying include:—

(a) Remarks written by me on the printed sheets.

(b) A manuscript page immediately preceding printed page 23.

There are two other necessary publications which I shall have to leave you to find at Oslo:—

(α) "A search for the law of Atm. Diffusion" in Hergesell Festschrift of Beiträge z. Physik der Freien Atm. (1929).

(β) "Diffusion as a Compensation for Smoothing." R. Met Soc. Mem Vol III No 30 (1930).

It is important to read (α) and (β) together.

Yours sincerely
L. F. Richardson.

Under (iii) in this letter, Richardson refers to what I have called the 'revision' file, and under (b) to what is reproduced here in Fig. 8.

Before I had seen the 'revision' file, I had asked Professor Godske to comment on his recollection of the contacts between Richardson and V. Bjerknes. In his letter, which follows, Professor Godske gives a delightful account of his visit with Richardson, which took place on the occasion of the Sixth General Assembly of the International Union of Geodesy and Geophysics held in Edinburgh, September 1936:

Lewis F. Richardson spent, I don't remember which year, some time in Bergen and was in contact with the Bjerknes group. Having been assistant of V. Bjerknes for many years, I have often heard B. speak of R. as a (impractical) man of genius. As far as I remember B. had a sincere sympathy for R., and I had the feeling that B.—like myself—considered it a great shame that "the big Empire" had no better use for R. than as a lecturer at a school.

I should like to tell you about my only meeting with L. F. At a conference 1936 in Edinburgh, V. B. presented me to L. F. as "a young man very interested in your work." L. F. invited me at once to visit him after the Congress and to spend a day and evening with him. I will never forget him, his kind wife—and his black dog (see below). L. F. was just a "vicar of Wakefield" type, the ideal of a ruddy whitehaired English country clergyman. The discussions were at the same time inspiring and depressing—depressing because I had the feeling that R. had lost contact with science, inspiring because of the many ideas, and the man himself.

And the dog! L. F. as a quaker would not serve under the Air Ministry. Fridtjof Nansen, who had recently died by overwork, was his great ideal. Consequently, the name of his dog (a fine setter) was "Nansen." "What did Nansen do for the League of Nations?" said L. F. and looked gravely at the dog. "Nansen" dropped down as if dead! You can imagine that I was between tears and laughter by this "homage à

Perhaps the most important change to be made in the second edition is that the equation of continuity of mass must be eliminated. That is to say $\frac{\partial \rho}{\partial t}$ will not be found by substituting m_E , m_N , in this equation, but in some other way.

Possible ways:—

I On p. 22 I eliminate θ between ~~$\rho = b\rho\theta$~~ and every other equation in which θ occurs.

It would surely be better to leave θ in and to eliminate ρ , for θ is observed while ρ isn't.

However that wouldn't affect the ~~real trouble which~~ is that $\operatorname{div} \mathbf{v}$ cannot be observed accurately.

II It is $\operatorname{div} \mathbf{v}$ that we have to eliminate

Is it possible to express the dynamical equations so that $\operatorname{div} \mathbf{v}$ appears in them, but \mathbf{v} does not?

From page (30) (1d)

$$-\nabla \psi - \frac{1}{\rho} \nabla \rho = \frac{\partial \mathbf{v}}{\partial t} + \nabla \frac{\mathbf{v}^2}{2} - [\mathbf{v} \cdot \operatorname{curl} \mathbf{v}] + 2[\boldsymbol{\omega} \cdot \mathbf{v}]$$

As \mathbf{v} requires for its specification 3 scalars, and as we propose to eliminate ~~one scalar~~ ^($\operatorname{div} \mathbf{v}$), we must still retain two scalars specifying the velocity. As to the choice of the remaining two we must be guided by the equations [$\operatorname{curl}_N \mathbf{v}$, and v_N would be an interesting pair, but we must take what we can get]

FIG. 8. Facsimile of an important manuscript sheet from Richardson's 'revision' file.

F. N." I think, however, that Nansen, a friend of children and animals would have enjoyed this compliment much more than the dead sculpture and pictures produced after his death.

17. This Commission was formed by the International Meteorological Organization in 1896. After the first world war it was reconstituted as the International Commission for the Exploration of the Upper Air, and after the second war as the Commission for Aerology of the World Meteorological Organization.

18. The bibliographic description of the issue used by Richardson is: *Veröffentlichungen der Internationalen Kommission für Wissenschaftliche Luftschiffahrt*, herausgegeben von Prof. Dr. H. Hergesell. Jahrgang 1910, Heft 5: *Beobachtungen mit bemannten, unbemannten Ballons und Drachen sowie auf Berg- und Wolkenstationen vom 18.-20. Mai 1910*. Strassburg, DuMont Schauberg, 1912.

19. The bibliographic description of this material is: *Veröffentlichungen des Geophysikalischen Instituts der Universität Leipzig*, herausgegeben von dessen Direktor V. Bjerknes. Erster Serie: *Synoptische Darstellungen atmosphärischer Zustände*. Jahrgang 1910, Heft 3: *Zustände der Atmosphäre über Europa am 18., 19., und 20. Mai 1910*. Leipzig, n.p., 1914.

20. In Richardson's book there is a curious ambiguity as to the source of the initial pressure data, because of omission of explicit reference to the Leipzig *Veröffentlichungen*. Gold (1954, p. 222) states incorrectly that the source is *Dynamic Meteorology and Hydrography*.

21. Translation by Yale Mintz in "A collection of papers related to the 1966 NMC primitive-equation model," Environmental Science Services Administration, Weather Bureau, Western Region Technical Memorandum 9, 1966, 46 pp.

22. The two volumes of *Dynamic Meteorology and Hydrography* were published by the Carnegie Institution of Washington, which in fact supported the work of V. Bjerknes and his collaborators for more than 40 years. This support began in May 1906 with a grant of \$1,200 to Bjerknes and Sandström (see p. 212 of the Yearbook for 1906 of the Carnegie Institution). It came about as a result of a lecture entitled "The application of the principles of hydrodynamics and thermodynamics to weather prediction," delivered by Bjerknes before the Washington Academy of Sciences, December 18, 1905, in Washington, D. C. The arrangements for this lecture were made by Arthur L. Day (soon thereafter named director of the Geophysical Laboratory of the Carnegie Institution), who had met Bjerknes in Stockholm in the summer of 1905. Bjerknes' presence in the United States was the result of his having been engaged in the autumn of that year in a series of lectures at Columbia University, on hydrodynamic fields of force (Bjerknes, 1906)—the first of the Adams Fund lectures, given in subsequent years by other outstanding physicists of the day. This invitation probably was initiated by Michael I. Pupin, Professor of Electromechanics at Columbia University.

23. Exner notes in the foreword that the war delayed the printing of his book, the complete manuscript of which was sent to the publisher in 1915. Richardson's manuscript, ready in 1919, was similarly delayed "by the legacy of the war" (Preface). I have already mentioned that the war caused Bjerknes to abandon the Leipzig school (details are recalled vividly by Bjerknes on p. 786 of *Physikalische Hydrodynamik*); and I shall note its tragic consequences for Margules. These are somber reminders of the grim and pervasive influence of the first world war.

24. Richardson gave citations to the two papers by Margules that are reprinted in Abbe's third collection of translations (published in 1910), namely, "The mechanical equivalent . . ." and "On the energy of storms," which appeared respectively in 1901 and 1904.

In his second collection, published in 1893, Abbe included Margules' paper of 1890, the first of the papers on atmospheric oscillations. Inexplicably, Exner (1917, p. 298) suggests that Lamb, in his paper of 1910 "On atmospheric oscillations," was unaware of Margules' work of 1890; but, in fact, Lamb does cite this work in a footnote on p. 557 of that paper. Moreover, Lamb cites Margules' work as early as 1895, in the second edition of *Hydrodynamics* (p. 495), where he gives Abbe's second collection as the source. The third edition (p. 520), published in 1906, gives also the original source; a German translation of this edition was published in 1907. However, Lamb did overlook Margules' papers of 1892–93, an oversight brought to his attention by Dr. Chapman and rectified in the fifth edition (p. 530), published in 1924.

25. The lead in this field had been taken before 1900 in the United States by Rotch (founder of Blue Hill Observatory), in France by Teisserenc de Bort (founder of the observatory at Trappes), and in Germany by Assmann. At the turn of the century Dines, in collaboration with Shaw, provided the impetus needed to enlist support from official councils; he built almost every part of the necessary equipment with his own hands, and in 1902 established what may be considered the first British upper-air stations, near Crinan, on the west coast of Scotland, and at his home in Oxshott.

26. It is a curious quirk of history that the automatic computer was conceived exactly a hundred years before Richardson began to speculate about numerical weather prediction. Its inventor Charles Babbage (1791–1871) was a misanthropic and erratic genius who spent much of his life in largely fruitless attempts to win support from various British governments for construction of his "engines." Had he succeeded, Richardson might have begun his own work under entirely more auspicious circumstances.

27. Professor J. Bjerknes very kindly has given me some comments which place the foregoing discussion in clearer perspective:

I think I can say for certain that my father did consider Richardson's work as the real first step toward the fulfillment of the "1904 program." But he also saw it as a demonstration

of the next to insurmountable difficulties looming ahead for numerical forecasting. As an aging man of 70, at the time when *Physikalische Hydrodynamik* was written, he must then have realized that he had no chance of making a "constructive" breakthrough along Richardson's line and therefore left the task for the younger generation.

The emphasis in *Physikalische Hydrodynamik* consequently was put on "The Life Cycle of Cyclones" (a title which by the way had been suggested by Richardson during his visit to the Bergen School in 1920). . . .

References

- Ashford, O. M., 1949: Lewis F. Richardson, D.Sc., F.R.S. *Weather*, **4**, 9–10.
- Bergeron, Tor, Olaf Devik and Carl Ludvig Godske, 1962: Vilhelm Bjerknes, March 14, 1862–April 19, 1951. *Geofys. Publikasjoner*, **23** (Bjerknes Centenary Volume), 7–25.
- Bjerknes, V., 1904: Das Problem der Wettervorhersage, betrachtet vom Standpunkte der Mechanik und der Physik. *Meteor. Z.*, **21**, 1–7.
- , 1906: *Fields of force*. New York, Columbia University, 160 pp.
- , 1914: Meteorology as an exact science. *Mon. Wea. Rev.*, **42**, 11–14. (Translation of *Die Meteorologie als exakte Wissenschaft*. Antrittsvorlesung gehalten am 8. Januar 1913 in der Aula der Universität Leipzig, Braunschweig, Vieweg, 1913, 16 pp.)
- , and J. W. Sandström, 1910: *Statics* (Part I of *Dynamic Meteorology and Hydrography*, by V. Bjerknes and collaborators). Washington, D. C., Carnegie Institution of Washington, Publication 88 (Part I), 146 pp. and tables.
- , Th. Hesselberg and O. Devik, 1911: *Kinematics* (Part II of *Dynamic Meteorology and Hydrography*, by V. Bjerknes and collaborators). Washington, D. C., Carnegie Institution of Washington, Publication 88 (Part II), 175 pp. and Atlas of 60 plates.
- , J. Bjerknes, H. Solberg and T. Bergeron, 1933: *Physikalische Hydrodynamik*. Berlin, J. Springer, 797 pp.
- Brunt, David, 1930: [Obituary of] Prof. F. M. Exner. *Nature*, **125**, 419.
- Chapman, S., 1922: [Book review]. *Quart. J. Roy. Meteor. Soc.*, **48**, 282–284.
- Charney, J. G., 1948: On the scale of atmospheric motions. *Geofys. Publikasjoner*, **17**, No. 2, 17 pp.
- , 1949: On a physical basis for numerical prediction of large-scale motions in the atmosphere. *J. Meteor.*, **6**, 371–385.
- , R. Fjörtoft and J. von Neumann, 1950: Numerical integration of the barotropic vorticity equation. *Tellus*, **2**, 237–254.
- Dines, William Henry, 1931: *Collected Scientific Papers*. London, Royal Meteorological Society, 461 pp.
- Douglas, C. K. M., 1931: Some problems of modern meteorology, No. 4. The present position of weather forecasting. *Quart. J. Roy. Meteor. Soc.*, **57**, 245–253.
- Eliassen, Arnt, 1949: The quasi-static equations of motion with pressure as independent variable. *Geofys. Publikasjoner*, **17**, No. 3, 44 pp.
- Ertel, Hans, 1943: Winddivergenz auf Isobarenflächen und Luftdruckänderung. *Meteor. Z.*, **60**, 188–191.
- Exner, Felix M., 1917: *Dynamische Meteorologie*. Leipzig, B. G. Teubner, 308 pp.
- , 1920: [Obituary of] Max Margules. *Meteor. Z.*, **37**, 322–324.
- , 1923: [Book review]. *Meteor. Z.*, **40**, 189–191.
- , 1925: *Dynamische Meteorologie*. Second edition. Vienna, J. Springer, 421 pp.
- Fjörtoft, Ragnar, 1952: On a numerical method of integrating the barotropic vorticity equation. *Tellus*, **3**, 179–194.
- Gold, E., 1920: [Obituary of] Dr. Max Margules. *Nature*, **106**, 286–287.
- , 1954: Lewis Fry Richardson, 1881–1953. *Obituary Notices of Fellows of the Royal Society*, **9**, 217–235.
- Jeffreys, Harold, 1922: [Book review]. *Phil. Mag.*, **44**, 285–286.
- Johnson, Nelson K., 1922: [Book review]. *Mathematical Gazette*, **11**, 125–127.
- Koschmieder, H., 1933: *Dynamische Meteorologie*. Leipzig, Akademische Verlagsgesellschaft, 376 pp.
- Margules, Max, 1904: Über die Beziehung zwischen Barometerschwankungen und Kontinuitätsgleichung. *Festschrift Ludwig Boltzmann*, pp. 585–589, Leipzig, J. A. Barth, 930 pp.
- McAdie, Alexander, 1923: [Book review]. *Geogr. Rev.*, **13**, 324–325.
- Neis, Bernhard, 1956: *Fortschritte in der Meteorologischen Forschung seit 1900*. Frankfurt, Akademische Verlagsgesellschaft, 238 pp.
- Rapoport, Anatol, 1957: Lewis F. Richardson's mathematical theory of war. *Journal of Conflict Resolution*, **1**, 249–299.
- Richardson, L. F., 1910: The approximate arithmetical solution by finite differences of physical problems involving differential equations, with an application to the stresses in a masonry dam. *Phil. Trans. Roy. Soc. London (A)*, **210**, 307–357.
- , 1949: Meteorological publications by L. F. Richardson as they appear to him in October 1948. *Weather*, **4**, 6–9.
- , 1960: *Arms and Insecurity*. Pittsburgh, Boxwood Press, 307 pp.
- , and Russell E. Munday, 1926: The single-layer problem in the atmosphere and the height-integral of pressure. *Mem. Roy. Meteor. Soc.*, **1**, No. 2, 34 pp.
- Richardson, Stephen A., 1957: Lewis Fry Richardson (1881–1953). A personal biography. *J. Conflict Resolution*, **1**, 300–304.
- Shaw, Napier, 1922: [Book review]. *Nature*, **110**, 762–765.
- , 1923: *Forecasting Weather*, third edition. London, Constable & Company, 584 pp.
- , 1926: *Manual of Meteorology*, Volume I: *Meteorology in History*. Cambridge, University Press, 339 pp.
- , 1931: *Manual of Meteorology*, Volume IV: *Meteorological Calculus: Pressure and Wind*. Cambridge, University Press, 359 pp.
- Sheppard, P. A., 1953: [Obituary of] Dr. L. F. Richardson, F.R.S. *Nature*, **4390**, 1127–1128.
- Shuman, Frederick G., and John B. Hovermale, 1967: An operational six-layer primitive-equation model. *J. Appl. Meteor.*, **6**, in press.
- Sutcliffe, R. C., 1947: Contribution to the problem of development. *Quart. J. Roy. Meteor. Soc.*, **73**, 370–383.
- Sutton, O. G., 1961: [Book review]. *Sci. Amer.*, **204**, 193–200.
- Van Mieghem, Jacques, 1947: La divergence horizontale isobare du vent et la tendance barométrique. Institut Royal Météorologique de Belgique, *Miscellanées*, No. 28, 16 pp.
- Vernič, Radovan, 1948: Richardsonova numerička prognoza vremena [Richardson's numerical weather prediction]. Khidrometeorološki Glasnik [Hydrometeorological Bulletin], **1**, 88–98.
- Whipple, F. J. W., 1922: Weather prediction by numerical process: a review of Mr. Richardson's work. *Meteor. Mag.*, **57**, 61–63.
- Woolard, Edgar W., 1922: L. F. Richardson on weather prediction by numerical process. *Mon. Wea. Rev.*, **50**, 72–74.

SIXTH CONFERENCE ON APPLIED METEOROLOGY (AEROSPACE METEOROLOGY), AMS with AIAA

March 28-31, 1966 Los Angeles, California

AMS/AIAA MEMBER PRICE—\$0.75 each

NONMEMBER PRICE—\$1.50 each

- 66-334 Weather Support Problems in the Gemini and Apollo Programs—K. M. Nagler
- 66-335 Wind Variability Over a Complex Surface—J. F. Appleby and T. H. Pries
- 66-336 Meteorological Real-Time System Used in Support of Unguided Rocket Firing—V. Cochran and L. D. Duncan
- 66-337 Solid State Vane and Anemometer—M. H. Norwood, A. E. Cariffe, V. E. Olszewski and R. C. Haskell
- 66-340 Seasonal Wind Patterns at Wallops Island Applied to Launch Operational Problems—J. A. Cochrane and R. M. Henry
- 66-342 A Preview—U. S. Standard Atmosphere Supplements, 1966—N. Sissenwine, M. Dubin and S. Teweles
- 66-344 Temperature Variations in the Tropical Stratosphere and Mesosphere 25 to 80 km—A. E. Cole
- 66-345 The High-Latitude Density Regime at Rocket Altitudes Inferred From Observations in Opposite Hemispheres—R. S. Quiroz
- 66-347 Density Profiles for Saturn IB Design—R. K. Steely
- 66-349 Meteorological Measurement Accuracies for Use in the Design and Operation of Aerospace Vehicles—A. S. Carten, Jr.
- 66-350 The Alleviation of Aerodynamic Loads on Rigid Space Vehicles—M. H. Rheinfurth
- 66-352 Wind Shear for Small Thickness Layers—M. Armendariz and L. J. Rider
- 66-353 Multivariate Statistical Analysis of Wind Sounding Data—C. J. Van Der Maas
- 66-355 Method for Selecting Atmosphere Density Models for Satellite Systems Studies—R. M. Jones
- 66-357 On the Prediction of Satellite Orbit Decay and Impact—L. N. Rowell, C. Gazley and G. Schilling
- 66-358 On the Linearized Atmospheric Contributions to Re-entry Vehicle CEP—F. M. Shinnick, III
- 66-359 Estimating Ballistic Wind and Density From 500 Millibar Data—D. G. Vincent
- 66-360 Meteorological Environment Considerations for All-Weather Land Recovery Operations of Lifting Re-entry Vehicles—J. Zvara
- 66-361 Simultaneous Occurrence and Statistical Distribution of Clouds Over the United States—R. Atlas
- 66-362 High Altitude Clear Air Turbulence (HICAT)—Menace to the Aerial Highway—N. V. Loving
- 66-364 Operational Application of a Universal Turbulence Measuring System—P. MacCready, Jr.
- 66-365 Flight Data Analysis of the Relation Between Atmospheric Temperature Change and Clear Air Turbulence—P. W. Kadlec
- 66-367 Effects of Atmospheric Gust Criteria on Supersonic Inlet Performance—F. W. Barry
- 66-368 Stratospheric Moisture Profiles over Northern California—D. D. Grantham, N. Sissenwine, H. A. Salmela and S. Rohrbough
- 66-369 Effect of Supersonic Aircraft on Cirrus Formation and Climate—H. S. Appleman
- 66-371 The Frequency Distribution of Route Temperatures at Supersonic Flight Levels—I. Gringorten
- 66-372 Satellite Sensing of the Lower Stratospheric Temperature Structure to Support SST Operations—E. S. Merritt and D. Chang
- 66-374 Some Applications of the Laser as an Atmospheric Probe—F. W. Gibson
- 66-375 Micrometeorology and the Clean Air Act—W. R. Jeffries, III
- 66-376 Project Adobe—A Study in Atmospheric Diffusion—G. L. Tucker
- 66-377 Diffusion of Toxic Exhaust Products from Titan III-C Solid Rocket Motors—L. I. Barker
- 66-378 The Predictability of Winds and Virtual Temperature Profiles for Flight and Static Test Operations—K. W. Veigas, D. B. Spiegler, J. T. Ball and J. P. Gerrity, Jr.
- 66-379 Ablation Particulate Dispersion in the Atmosphere—C. V. Hendricks
- 66-380 The Atmospheric Entry Dynamics of Small Particles—R. C. Lee, F. F. Percy, J. E. Francis and R. H. Maynard
- 66-382 The Gun-Launched Meteorological Sounding System—L. Williamson and E. D. Boyer
- 66-383 New Low Cost Meteorological Vehicle System for Temperature and Wind Measurements in the 75,000 to 200,000 ft Altitude Region—B. Bollermann and R. L. Walker
- 66-385 The Measurement of Temperature in the Stratosphere—H. N. Ballard
- 66-386 The Accuracy of Thermistors in the Measurement of Upper Air Temperature—D. C. Thomson and D. P. Keily
- 66-388 A Means for Improving the Accuracy and Extending the Maximum Altitude of Mesospheric Temperature Measurements—R. G. Billings
- 66-389 Solar Observing and Forecasting for Military Operations—C. K. Anderson
- 66-395 The Electron Distribution From 400 to 1200 km—L. Dubach and C. L. Rush
- 66-397 The USAF Meteorological Rocket Network—J. Giraytys and F. Rose
- 66-398 Wind Sensing Capabilities and Rise-Rate Characteristics of Some Ground Launched Rigid Balloon Systems—C. V. Eckstrom
- 66-399 Description of a New Parachute Designed for Use With Meteorological Rockets and a Consideration of Improvements in Meteorological Measurements—H. N. Murrow and C. V. Eckstrom
- 66-403 Upper Atmosphere Winds Measured by Gun Launched Projectiles—C. H. Murphy, G. V. Bull and H. D. Edwards

Orders should be sent to:

Remittance must accompany all mail orders.

AMERICAN METEOROLOGICAL SOCIETY, 45 BEACON ST., BOSTON, MASS. 02108