Minutes of the meeting held at the Institute for Advanced Study on August 5, 1952 on the subject of <u>practical</u> <u>numerical</u> <u>weather</u> <u>forecasting</u>.

Present were:

Institute for Advanced Study
von Neumann (Chairman), Charney, Phillips, Smagorinsky

Weather Bureau
H. Wexler, J. Smagorinsky

Air Weather Service
Petterssen, Lewis

Office of Naval Research
Bodurtha, Hughes

Aerology Branch, Bureau of Aeronautics, Navy Department
Rex

Geophysics Research Division
Craig, Tuart

University of Chicago
G. Platzman

ARDC
Dolezel

n: It is appropriate to begin by stating the reason for the calling of this meeting and its objective. The meeting has been called because the work that has been done here at the Institute is at the stage where some practical information is available concerning operational weather forecasting by numerical methods. The object of the meeting is to determine whether the stage is ripe to prepare for operational forecasting. Let me begin by describing the work that has been done here.

Pre-1950: The work before 1950 consisted entirely of traditional mathematical calculations together with some "hand" calculations all based on linearized equations.

Early-1950: At this time the Eniac at Aberdeen was used to provide some numerical forecasts on a non-linear basis. The amount of computation done can be summarized conveniently by ~~a factor of~~ stating that it contained 1.5×10^6 multiplications. The amount of time spent in preparing for the test and the amount of actual work at Aberdeen was about 5 months. It would have taken 5 hand computer years to have duplicated the Eniac computations. Thus the first use of automatic computing machinery in this field cannot be considered a~~n~~ radical improvement over what could have been done without the use of automatic computing machinery. This is undoubtedly typical of such beginning attempts.

Early-1952: At this time the machine at the Institute began operation in the sense that one could attempt problems of the complexity typical of meteorological problems. Approximately three months have been spent in meteorological work with the machine, amounting to something like 50×10^6 multiplications, or one year of Eniac time at good efficiency. (The same number of forecasts could have been obtained with 1/2 the number of multiplications if the program had been designed solely for speed ~~this problem~~.)

Combining the Eniac and Princeton forecasts, we have made the equivalent of 20 24-hour forecasts for an area covering the continental United States plus 1000 miles to the East and North. Almost all of this work has been with the simple barotropic model, but the next four months should see an extension to a more complicated model.

I would now like to make some inferences concerning practical forecasting based on the above experience we have gained. I assume that practical forecasting should begin with a general baroclinic model, which should provide valid forecasts for a period of at least 36 hours. The preparation of such a forecast can be divided into three steps:

a) input preparation

b) actual computing time

c) output preparation and editing of forecast.

To produce ~~I wish to point out that in~~ a 24-hour forecast we can allow 12 hours for these three steps. I estimate step b), when designed for speed, will take about four hours leaving 8 hours for a) and c), of which most would presumably be used for a). Any reduction of these times would be best spent in reducing the period of the forecast from 36 hours.

The problem of initiating such a program can be divided into two ~~problems~~ parts, an educational problem and a technological problem. There is an educational problem because there are practically no people available at the present time capable of supervising and operating such a program. Synoptic meteorologists who are capable of understanding the physical reasoning behind the numerical forecast are needed to evaluate the forecasts, for example. Mathematicians are needed to formulate the numerical aspects of the computations. During the first several years of the program the meteorological and mathematical aspects probably cannot be separated and personnel familiar with both aspects are needed. An intense educational program could conceivably produce enough people in about three years.

A technological problem exists because the preparation of forecasts on a routine basis will require that some computing machine be ~~available~~ in perfect condition and operable at a given time of day every day of the year. Of the several very fast machines now in existence I do not believe any are capable of running at maximum efficiency for more than 50% of the time. ~~This problem also exists, for example, in the use of machines for air defense and in missile guiding~~ A similar problem is, however also present in several other (military) applications, and should be partially solved in the next several years.

Petterssen: From conversations I have had on this subject with various people in Washington I believe that ~~each~~ since the forecast computing time amounts to something like 8 hours per day, ~~T~~there will be great pressure to share any machine with other ~~programs.~~ agencies.

von Neumann: If a machine is to be available at a specified time every day, I believe that something like 1/3 of the ~~time~~ day should be spent in preventive maintenance and another 1/3 ~~of the time~~ in test runs. Some research computations should also be done in any spare time available. I might also mention that at least in the beginning of such a program there might be advantages to having a machine operated by a private corporation.

Smagorinsky: Do you believe that duplication~~s~~ of the computing work is necessary?

von Neumann: Strict duplication can probably be dispensed with in a machine which can normally be relied upon; other tests being incorporated into the computation program.

Petterssen: Concerning the times of the various operations a), b) and c). It will be easier to get support from the organizations concerned if we can give them the smallest possible figures in this connection.

Charney: The input and output can undoubtedly be cut down drastically eventually. Even at first I believe Professor von Neumann's figures to be quite conservative.

Petterssen: How large an area do you think such forecasts should include?

von Neumann: We considered the United States the largest area at the present time with adequate data. This is probably not sufficient for the best possible 36-hour forecasts. I believe that any request for more extensive data had best follow practical demonstration of the usefulness of numerical forecasting. The memory limitations of the machine are also of importance, placing an upper limit to the data which can be handled by the machine.

Petterssen: While in Stockholm I was shown some forecasts based on tendency calculations made for the area including Western Europe west to about 50° West Longitude. The maximum correlation between the observed and predicted tendencies was in the middle of this region, the correlation decreasing more rapidly to the West than to the East. The center was not located exactly where the data was most ~~intense~~ plentiful. This may mean that the limitation imposed by artifical boundary conditions may be greater than the absence of what we would otherwise consider adequate data.

von Neumann: To return to my previous remark concerning the memory limitations of the machine; the area from Japan east to ~~East~~ eastern Europe is about four times the area we used here at Princeton. Therefore an appreciably larger machine would be needed to forecast for such an area, especially ~~as~~ when more complicated atmospheric models are used. This technical problem might be solved, let's say, within about five years after the program under consideration is started.

Bodurtha-Wexler: The present forecasting methods have an input and output time of about six hours.

von Neumann: The input for the machine has different requirements than those used in present forecasting methods. To begin with I would say that the machine input would take more than six hours but eventually could be cut down to much less than six hours.

Wexler: In talking to various forecasters they have given me the impression that they expect numerical forecasting methods to yield immediate 3 to 4 day forecasts. Evidently they believe that present methods are sufficient for one-day forecasts.

General Opinion: Such expectations should be discouraged at the present time.

Charney: From our experience I would say that the barotropic forecasts are perhaps not as good as the best conventional forecasts, but the indications are that baroclinic forecasts will be much better.

Concerning the training of people for this program, ~~the~~ our group here at the Institute can probably expand ~~so~~ to the extent that representatives from the three services can join our group for indoctrination in our methods.

Petterssen: The Air Weather Service agrees with the aims of this project and has temporarily earmarked three people for training. Should we plan on more at the present time?

von Neumann: Three is about the most which can be accommodated here at one time. Probably one from each group would be the best scheme.

Petterssen: Yes. We had also considered sending people to Stockholm or to get machine training.

Platzman: I would like to suggest a ~~scale~~ scheme to classify the type of people we are interested in:

A) a small group who now possess most of the qualifications, but who require some brushing up.

B) forecasters experienced in synoptic meteorology with some knowledge of meteorological theory.

C) a large group at present more or less ~~unlimited.~~ inexperienced.

The personnel to initiate this program will, of course, come from group A) and those can best be trained at the Institute, I believe. The B) and C) groups should get training at the Universities. At the University of Chicago, for example, we have training suitable for the B) group consisting of a course in advanced forecasting techniques including some numerical methods. In the near future the Universities should begin to introduce numerical forecasting into their basic curricula, for group C)

Wexler: Do you plan to have a specific course in numerical prediction at the University of Chicago this year?

Platzman: We hope to have such a course and also to increase the emphasis on numerical forecasting methods in the advanced forecasting course.

von Neumann: Group C) is very important to the long-range results of this work. I believe best results here would be obtained by attempting to divert prospective physicists and applied mathematicians into meteorology.

Petterssen: At the present time weather forecasting does not attract very many theoretical minded people, but I believe that this method of forecasting will begin to attract them.

Charney: It will be at least three years before a machine is available for this operational group. The time between indoctrination and three years from now should be spent in pre-operational research and development. For example, the deduction of weather from the flow charts, the introduction of frontal structures, types of condensation, and rationalization of communications.

Wexler: I believe this should be done without having the group leave numerical work entirely.

Tuart: Phil Thompson could not be here, but asked me to read this statement for him (see appendix). (To be included in final copy.)

Charney: I do not believe that a very simple model will be sufficient for operational forecasts.

von Neumann: The problem of deciding at what level to begin forecasts can be decided only by experience which we hope to have within another year.

Tuart: GRD has suggested that Air Weather Service join it in its work with these simple models. We don't suggest taking any one model and concentrating on it, of course.

Wexler: The Weather Bureau also will furnish people for this training program and would be happy to have them come to the Institute for that training.

Rex: The Navy is also prepared to furnish people.

Petterssen: The Air Weather Service will support the GRD program and this program as well.

von Neumann: Concerning the technological problem, has anything been done yet about getting a machine?

exler: The Weather Bureau has asked for $400,000 for a machine. The earliest at which this money would be available would be next summer. Perhaps then would be the best time to raise this question again about what type of machine to get, etc.

Wexler: When should we plan on having these people arrive here?

von Neumann: Would the first of October be appropriate?

Wexler: Should they arrive simultaneously to facilitate orientation?

Charney: We haven't given much thought yet to such details.

Rex: Will people be invited specifically?

von Neumann: Yes, this is the procedure you prefer. However, nothing can be done until I have discussed this with Dr. Oppenheimer, which will be impossible to do until September.

Wexler: Why not plan on having the people arrive on December 1?

General Agreement.

von Neumann: I will plan to send letters to the Weather Bureau, Air Weather Service and Navy before the 10th of September after I have been able to discuss this program with Dr. Oppenheimer.

Wexler: Is housing available for these people?

von Neumann: It may be possible to have housing, but it is not definite that there will be any available. I will put in requests for it.

Letters to be mailed to:

1. Chief, U. S. Weather Bureau, Washington 25, D.C.
2. Chief, Naval Operations, Navy Department, Washington 25, D. C.
3. Chief, Air Weather Service, Washington, D. C.

STATEMENT TO THE CONFERENCE ON NUMERICAL PREDICTION

TO BE HELD AT PRINCETON ON 5 AUGUST 1952

Before coming to a definite conclusion as to the relative merits and disadvantages of two- and three-dimensional dynamical models of the true atmosphere, one should consider not only the increased returns expected from the use of a three-dimensional model, but also the price that must be paid for those returns.

With regard to the former, it has not yet been demonstrated -- nor even strongly suggested by experiment -- that the complete three-dimensional model will produce significantly more accurate predictions than would an equivalent two-dimensional model. Until both possibilities have been thoroughly explored, it would be wasteful of time, effort, and money to adopt a single, fixed viewpoint and to be committed irretrievably to a course of action based on that viewpoint. This is especially true, since whatever improvements are gained by the use of three-dimensional models will be paid for at an enormous increase in cost over that of applying a two-dimensional equivalent.

For the above reason, it is relevant to point out that the use of two-dimensional equivalents is by no means restricted to the "equivalent barotropic" model. It is possible to retain, in a two-dimensional equivalent, all the baroclinic properties of the true three-dimensional model that are known to be essential to the mechanism of cyclone development. It is generally recognized, of course, that it is principally in the lack of such baroclinic effects that the two-dimensional "equivalent-barotropic" model has failed.

I have just completed, within the past week, some theoretical development which leads directly to the invention of an <u>"equivalent-baroclinic"</u>

model. This model is summarized in a pair of independent equations, involving, as independent variables, the horizontal space coordinates and time and, as dependent variables, the temperature and contour height of an isobaric surface at an identifiable "level of equivalence".

Thus far, I have only had time to carry out some simple indirect checks on quantities which can be derived easily from the theory, without solving the equations completely -- e.g. the vertical component of velocity. The vertical motions required by the theory agree very well, as to magnitude, sign, and distribution, with those reconstructed from synoptic data. As a simple and rather sensitive test, I have also worked out the stability condition for small disturbances in a horizontally uniform flow. The stability discriminant is of the same general form as those derived by Fjörtoft and Phillips, the relevant physical parameters being the same and entering in the same way. As it turns out, in fact, this stability condition includes Fjörtoft's as a special case, valid for long wave lengths.

This, in itself, is ample evidence that the stability characteristics of three-dimensional baroclinic flow can be incorporated in a two-dimensional equivalent and, accordingly, that cyclone development due to those baroclinic effects can be predicted by the use of a two-dimensional model.

The complete equations are, of course, non-linear. However, solution of the complete non-linear equations involves nothing more complicated than solving a non-homogeneous equation of the Helmholtz type, in only two dimensions.

In this connection, it is important to note that the solution of two-dimensional equations -- even non-linear equations -- does not require the flexibility nor speed of a very high-speed, general purpose computer. It is a problem which is well within the reach of special purpose machines of standard make and proven reliability. There are, moreover, a number of simple electrical or mechanical systems which are governed by a non-homogeneous Helmholtz equation and which would therefore provide a reliable and inexpensive analogue device for solving the non-linear equations.

For the above reasons, I believe it would be unsound to become definitely committed to any program which is founded on the premise that numerical methods will bring about significant improvements in forecasting practice <u>only</u> through the use of three-dimensional models and, accordingly, that a high speed general purpose computer is the <u>only</u> instrument capable of dealing with the associated computational problems.

In my opinion, lacking any real evidence to the contrary, there is serious doubt that a three-dimensional model will yield much better results than its two-dimensional baroclinic equivalent -- enough better that the enormous effort of solving the three-dimensional equations would be justified on economic grounds.* This is not to say that high speed computers should not be used for research purposes, to solve the three-dimensional equations, and thereby to resolve the questions I have raised. I simply mean to suggest that high speed, general purpose equipment may not be the best choice for operational purposes, as opposed to research purposes.

In any case, adequate account must be taken of the law of diminishing returns.

The above discussion has dealt mainly with considerations which enter into the choice of the "best economical approximation" to the true optimum of method and equipment. Although these considerations should affect even a long range program, the economic compromises are generally less severe as

---

* The curious "two-dimensional" character of the atmosphere has been frequently noted -- by Rossby, Charney, and others, and, of course, by the practicing forecaster.

the length of the program increases. Having plenty of time, so to speak, one can wait for the development of new products and buy only the best.

On the other hand, if time is short, one is forced to buy the best available at the latest moment. With this in mind, I would question whether all of the resources necessary to carry out integrations of the three-dimensional equations on a routine basis -- fully trained meteorologists, automatic transcription and data processing equipment, reliable computing machines, a full staff of engineers and technicians, and experience -- could be assembled under the same roof within two years, without hampering meteorological labours of equal merit.

A much more modest effort, however, would be sufficient to develop "two-dimensional" models for routine use.

The figure of two years quoted above is based on immediate military needs, as distinct from the needs of the populace at large and the broad interests of meteorological science. Therefore, unless it is absolutely certain that the best possible method will be available within two years, I believe that we should immediately start action to develop the best method that we are certain will be available within that period. At the present time, we are only certain that the job of exploiting a two-ddmensional model can be done in such a limited time.

This should not be construed as an argument against undertaking a longer range program for developing the model which, in principle, is the best possible, namely, the three-dimensional model. My recommendation is that two programs should be carried out in parallel: one, a short range program for the development of the best methodavailable within two years, primarily for military purposes; the other, a long range program for the development of the best method <u>possible</u>, for general use.

In closing, I see no reason why two parallel programs could not be made to complement each other, with proper coordination and close contact at the working level. The shorter range program would serve as a valuable proving ground for new ideas and would undoubtedly make some of the mistakes by which others profit later.

Philip D. Thompson
Major, USAF