PANOFSKY, HANS A., 1947-1966

ok 1/2

THE PENNSYLVANIA STATE UNIVERSITY
322 MINERAL INDUSTRIES BUILDING
UNIVERSITY PARK, PENNSYLVANIA 16802

27 September 1966

College of Mineral Industries
Department of Meteorology
Prof. H.A. Panofsky

Area Code 814
865-5222
865-6451

Prof. Jules Charney
Department of Meteorology
M.I.T.
Cambridge, Mass.

Dear Jules:

In accordance with ou̅r telephone conversation I am sending to you the enclosed NCAR report.

Best regards,

Hans

THE PENNSYLVANIA STATE UNIVERSITY
~~STATE COLLEGE, PENNSYLVANIA~~
University Park, Pa.

COLLEGE OF MINERAL INDUSTRIES
DEPARTMENT OF METEOROLOGY

March 1, 1955

Dr. Jule Charney
Institute for Advanced Study
Princeton, New Jersey

Dear Jule:

Yale Mintz tells me that you have some misgivings in regard to the measurement of divergence from winds. I certainly agree with you that divergence cannot be measured that way, between say 700 mb. and 400 mb. However, we are actually determining divergence of the wind integrated from the surface to 700 mb., thus including frictional divergence. We find that many cases exist where the wind speeds up along the streamlines without confluence, or a confluence exists with the speeding up of the wind.

I believe that not only is friction responsible for the fairly large divergence, but also the accelerations in the general region near 850 mb. In fact, we are trying to check the integrated divergence by (1) the adiabatic technique, and (2) the vorticity equation, in which we include a frictional term containing the curl of the surface stress. I am rather prejudiced in believing that the latter contributes greatly to the vertical motion at the top of the friction layer and, therefore at 700 mb. Our measurements at Brookhaven indicate that the wind of ten meters per second produces a stress of about 10 dynes per square centimeter.

Please don't answer this letter unless you object to it. In any case, I hope to see you for a very short time around Easter.

Best regards,

HAP

H. A. Panofsky
Acting Head
Department of Meteorology

HAP:ck

PENN STATE CHANGE OF ADDRESS

Effective February 22, 1955,
the mailing address of The Pennsylvania
State University was changed to

UNIVERSITY PARK, PENNSYLVANIA

All mail intended for the main campus
(formerly sent to State College, Pa.)
should be sent to University Park.

August 6, 1953

Professor Hans A. Panofsky
195 Westerly Parkway
State College, Pennsylvania

Dear Hans:

Enclosed are the results of the time series analysis of the data sent to us on April 11, 1953.

The quantities sent to you have six respectively five significant decimal digits of which at most the first three can be trusted. You will find a short explanation of the results on the bottom of the data sheet.

The code was checked out by special input data and the results should therefore by trustworthy.

I am sorry you had to wait so long to obtain these results and hope that they are still of interest to you. As for the computation of the other data, this is a problem to be discussed with Jule. The machine has been dismantled and is in the process of being moved and I am leaving the Institute in September.

Sincerely yours,

Adolf Nussbaum

AN/cb

Enclosure

THE PENNSYLVANIA STATE UNIVERSITY
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STATE COLLEGE, PENNSYLVANIA

DEPARTMENT OF EARTH SCIENCES
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ADDRESS: MINERAL INDUSTRIES BUILDING
TELEPHONE: STATE COLLEGE 8-8441

17 May 1954

Mr. James W. Cooley
Electronic Computer Project
The Institute for Advanced Study
Princeton, New Jersey

Dear Mr. Cooley:

Many thanks for your data and cards. The results seem to be very good.

Sincerely yours,

Hans P.

Hans A. Panofsky
Acting Chief
Div. of Meteorology

HAP:pwr

THE PENNSYLVANIA STATE UNIVERSITY
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ADDRESS: MINERAL INDUSTRIES BUILDING
TELEPHONE: STATE COLLEGE 8-8441

5 May 1954

Mr. James W. Cooley
Institute for Advanced Study
Electronic Computer Project
Princeton, New Jersey

Dear Mr. Cooley:

All your data looked very nice. Since we are starting to write a final report for our project, could you give us an estimate as to when we could have the remaining spectra? If it would expedite matters a great deal, m = 30 would be sufficient.

Also, would you be so kind to remind Charney to send back Chapter V (he'll know what I am talking about). If he wants to save the postage, he can take it to Los Angeles and give it to me there.

Best regards,

Hans Panofsky

Hans A. Panofsky
Acting Chief
Div. of Meteorology

HAP:pwr

P.S. Would it be possible for you to return the cards for Period WK?

THE PENNSYLVANIA STATE UNIVERSITY
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ADDRESS: MINERAL INDUSTRIES BUILDING
TELEPHONE: STATE COLLEGE 8-8441

March 10, 1954

Dr. Jule Charney
Institute for Advanced Study
Princeton University
Princeton, New Jersey

Dear Jule:

Thank you very much for letting Cooley work on the turbulence spectra.

I hope to be in Princeton the Monday after Easter, April 19. Hope to talk to you for a little while about various things. In particular, I have completed Chapter V of the Navy text which deals with numerical forecasting. I am very anxious that you or Gilchrist read this chapter before I throw it to the wolves, before I come to Princeton, if possible.

Best regards,

Hans

H. A. Panofsky
Acting Chief
Division of Meteorology

HAP:ck

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ADDRESS: MINERAL INDUSTRIES BUILDING
TELEPHONE: STATE COLLEGE 8441

23 December 1953

Dr. Jule Charney
Institute for Advanced Study
Princeton, New Jersey

Dear Jule:

Happy New Year ! I am sending you under separate cover Chapter I of a manual on dynamic meteorology for the Navy. If you should read it (which I doubt that you will) you will find some of the things rather childish. However, I would appreciate if somebody in your outfit could look at it in case I have made any serious errors. Eventually, I would like to send you Chapter V which deals with numerical forecasting.

If your coders do not have the time to make up a new code for spectrum analysis, could you possibly see to it that the cards which you have available are run off by Nussbaum's code? We know that code works even if it takes too long.

Best regards to Eady, Gilchrist, and Eleanor.

Sincerely yours,

Hans

H. A. Panofsky
Professor

HAP:pwr

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ADDRESS: MINERAL INDUSTRIES BUILDING
TELEPHONE: STATE COLLEGE 8441

December 1, 1953

Dr. Jule Charney
Institute for Advanced Study
Princeton University
Princeton, New Jersey

Dear Jule:

While you had your discussion with Cressman, I tried to rewrite Tukey's formulae in much less elegant notation. It is pretty certain that they are correct, but if Tukey would look them over sometime, it would be greatly appreciated. Also, I explained them to Mr. Lewis who seems to understand the formulae thoroughly. Mr. Cooley was not around when I left. Would you be so kind as to keep an eye on what happens to these spectrum analyses?

You still have several sets of cards which we would very much like you to run if or when it is convenient, in particular, that set containing u's and v's, five minute averages for a period of a week.

~~We~~ I very greatly enjoyed talking to your seminar, and to Eady.

Best regards,

Hans

H. A. Panofsky
Professor
Department of Meteorology

HAP:ck

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ADDRESS: MINERAL INDUSTRIES BUILDING
TELEPHONE: STATE COLLEGE 8441

24 September 1953

Dr. Jule Charney
Institute for Advanced Study
Princeton, New Jersey

Dear Jule:

Thank you very much for taking care of the different matters for us concerning Dr. Sheppard. Would you be so kind to also thank him for taking out time to stop at State College. We enjoyed his visit tremendously and learned a great deal from him.

Sincerely yours,

Hans

H.A. Panofsky

HAP:pwr

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ADDRESS: MINERAL INDUSTRIES BUILDING
TELEPHONE: STATE COLLEGE 8441

17 September 1953

Dr. Jule Charney
Institute for Advanced Study
Princeton, New Jersey

Dear Jule:

After looking up the calendar for this year and my course schedules, it appears that the only day on which I could give a seminar at Princeton would be the day after Thanksgiving. Would you be so kind to let me know whether this is convenient.

Best regards,

Hans

H.A. Panofsky
Assoc. Professor

HAP:pwr

THE PENNSYLVANIA STATE COLLEGE
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ADDRESS: MINERAL INDUSTRIES BUILDING
TELEPHONE: STATE COLLEGE 8441

20 May 1953

Dr. Jule Charney
Institute for Advanced Study
Princeton, New Jersey

Dear Jule:

Many thanks for your letter. In view of your proposed trip, I am changing my plans to be in Princeton June 6, 7, and 8. I hope to see you then for a short time.

Some of the physicists here have made some very pleasing comments about your speech in Washington. You seem to be leading a one man campaign to raise the physicists' opinion of us meteorologists.

Best regards,

Hans

H.A. Panofsky
Assoc. Professor

HAP:pwr

May 15, 1953

Professor Hans Panofsky
Pennsylvania State College
School of Mineral Industries
Mineral Industries Building
State College, Pennsylvania

Dear Hans:

This is in reply to your letter of May 5. I would be glad to see you in Princeton on any date that is convenient to you, but unfortunately I am leaving for the West coast approximately June 11, therefore, I hope it will be possible for you to get up here before then. However, if it is not possible for you, Nussbaum will be here and I hope it will be enough for you to discuss your business with him.

The code has taken longer than we had planned, but it is now finished and we should run the computations within the next few days.

With best regards,

Yours sincerely,

JC/cb

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ADDRESS: MINERAL INDUSTRIES BUILDING
TELEPHONE: STATE COLLEGE 8441

5 May 1953

Dr. Jule Charney
Institute for Advanced Study
Princeton, New Jersey

Dear Jule:

I have sent you separately another set of data, including vertical and horizontal velocities. There are altogether somewhat over 1200 values of each. If this is too many for your present code, just omit the tail end of the observations.

As you know, I had a chance to talk to Tukey by telephone, and he explained his new method to me. The spectrum which he finally obtains after the manipulations indicated still has to be transformed in order to become comparable with the spectra which we have been computing.

To discuss this and some other things I would very much like to talk to you for a little while in June. I had been planning to get to Princeton next on Saturday, June 13. Are you adverse to discussing business on Saturday or Sunday, especially that Saturday and Sunday? If so please let me know and I shall change my plans. I should also like to talk to Mr. Newsbaum.

Best regards to Eleanor and the kids.

Sincerely yours,

Hans

H.A. Panofsky
Assoc. Professor

HAP:pwr

March 27, 1953

Professor H. A. Panofsky
Department of Earth Sciences
Pennsylvania State College
State College, Pennsylvania

Dear Hans:

Thanks for your kind remarks about the meetings. Also thank you again for your own contribution. I found it very interesting and would like to discuss the subject with you again. I certainly do think you should continue along the lines you are now following. This field must be developed.

Concerning your visit. We shall be glad to have you come on Tuesday, April 7. We have some meetings on Monday, April 6 and cannot be sure when we will be free. The invitation is also extended to Mr. Miller. Norman Phillips has done the programming of the spectral computation for time series and will be on hand to discuss matters.

Best regards,

Sincerely,

JC/cb

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ADDRESS: MINERAL INDUSTRIES BUILDING
TELEPHONE: STATE COLLEGE 8441

20 March 1953

Dr. Jule Charney
Institute for Advanced Study
Princeton, New Jersey

Dear Jule:

The proposed arrangement for processing our data by the Princeton electronic computer will take quite a bit of detailed discussion, partly between you and me and partly between your coders and me. Would you be so very kind to make arrangements for me to talk to your assistants April 6 or 7.

If possible, I would like to talk to you for perhaps a couple of hours one of these two days. Also, Mr. Miller of our staff would like to see you about the same time concerning his studies which deal with the relationship between surface weather and the free atmosphere field of motion.

I believe that the symposium at Atlantic City went extremely well and I learned a tremendous amount. One of the things I would still like to discuss with you is whether you think there is any sense in our continuing along the lines of objective analysis which we are now doing. Hope to see you soon.

Best regards,

Hans

H.A. Panofsky
Assoc. Professor

HAP:pwr

P.S. Do you think that experience really helps an analyst, or does he sometimes throw out correct observations "by experience"?

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ADDRESS: MINERAL INDUSTRIES BUILDING
TELEPHONE: STATE COLLEGE 8441

17 February 1953

Dr. Jule Charney
Institute for Advanced Study
Princeton, New Jersey

Dear Jule:

Before I stick my neck out too far at the March meeting I wonder if you have any comments on the following type of semiobjective analysis: Our school has an analogue computer which will make a two-dimensional Fourier analysis from observations at equidistant grid points. We are planning to take some subjectively-analyzed weather maps, take off data at grid points and compute the first 3 or 4 harmonics. We hope that these few harmonics will not depend very strongly on the original analysis, provided the number of coefficients of the functions is not larger than the number of observations. I have no good theoretical reason for this opinion and wonder whether you have any opinion on the matter.

In any case the experiment may not be finished in time for inclusion in the talk at Atlantic City, in which case I would simply review the philosophy of making an objective analysis and summarize some of the work done by Haurwitz and us.

Incidentally, am I required to write a formal paper and if so would you need it before the meeting?

Best regards to Eleanor.

Sincerely yours,

Hans

H.A. Panofsky
Assoc. Professor

HAP:pwr

Prof. H. ~~[illegible]~~ Panofsky

Dear Hans,

There isn't much I can add to what you already know ~~about our work~~ and what Bolin told you at Woods Hole ~~[illegible]~~ except that we are now making a ~~concerted~~ direct attack on the three-dimensional problem via the quasi-geostrophic equations. ~~A careful~~ An analysis of the amount of real information contained in upper air data has convinced us that six bits (six binary digits) gives all the information ~~given~~ contained in an upper air temperature or pressure recording. This means that we can store p and $\frac{\partial p}{\partial t}$ or z and $\frac{\partial z}{\partial t}$ or ψ and $\frac{\partial \psi}{\partial t}$ $(\psi = c_p T + gz)$, depending on whether z, p, or θ is used as the vertical coordinate, for about 1000 grid points. We are now contemplating an integration for a lattice of dimension 12×12 horizontal $\times$ 8 vertical $= 1152$ points. The equation

$$L\left(\frac{\partial\psi}{\partial t}\right) \equiv \left[\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} - \frac{f(f+\zeta)}{\frac{\partial^2\psi}{\partial\theta^2}}\frac{\partial^2}{\partial\theta^2} - \frac{c_v}{R}\frac{f(f+\zeta)}{\frac{\partial\psi}{\partial\theta}}\frac{\partial}{\partial\theta}\right]\frac{\partial\psi}{\partial t} =$$

$$= \frac{\partial(f+\zeta,\psi)}{\partial(x,y)} - \frac{(f+\zeta)}{\frac{\partial^2\psi}{\partial\theta^2}}\frac{\partial\left(\frac{\partial^2\psi}{\partial\theta^2},\psi\right)}{\partial(x,y)} - \frac{c_v}{R}\frac{(f+\zeta)}{\frac{\partial\psi}{\partial\theta}}\frac{\partial\left(\frac{\partial\psi}{\partial\theta},\psi\right)}{\partial(x,y)}$$

will be solved for $\frac{\partial\psi}{\partial t}$ by a ~~somewhat~~ rather elaborate relaxation method. ~~for $\frac{\partial\psi}{\partial t}$~~

There are a number of problems connected with the integration of a technical-mathematical character. For the time being we are going to have to be applied mathematicians whether we like it or not.

We hope to run the problem before the end of the year, but are keeping our fingers crossed.

We are also ~~pursuing~~ conducting a number of side investigations which, ~~however~~, need not be mentioned as they do not have a direct ~~effect~~ bearing on the numerical forecast problem.

October 30, 1951

Professor H. Panofsky
Mineral Industries Building
Pennsylvania State College
State College, Pennsylvania

Dear Hans,

There isn't much I can add to what you already know and what Bolin told you at Woods Hole except that we are now making a direct attack on the three-dimensional problem via the quasi-geostrophic equations. An analysis of the amount of real information contained in upper air data has convinced us that six bits (six binary digits) gives all the information contained in an upper air temperature or pressure recording. This means that we can store

depending on whether z, p or is used as the vertical coordinate, for about 1000 grid points. We are now contemplating an integration for a lattice of dimension 12 x 12 horizontal x 8 vertical = 1152 points. The equation

will be solved for by a rather elaborate relaxation method. There are a number of problems connected with the integration of a technical-mathematical character. For the time being we are going to have to be applied mathematicians whether we like it or not. We hope to run the problem before the end of the year, but are keeping our fingers crossed.

We are also conducting a number of side investigations which need not be mentioned as they do not have a direct bearing on the numerical forecast problem.

I hope this is what you want and I wish you success in your lecture.

Best regards,

THE PENNSYLVANIA STATE COLLEGE
SCHOOL OF MINERAL INDUSTRIES
STATE COLLEGE, PENNSYLVANIA

DEPARTMENT OF EARTH SCIENCES
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ADDRESS: MINERAL INDUSTRIES BUILDING
TELEPHONE: STATE COLLEGE 8441

Dr Jule Charney
Institute for Advanced Study
Princeton, N.J.

Dear Jule,

on November 12, I am supposed to deliver a Sigma Xi lecture on "Weather Forecasting as a Physical Science". In this lecture, I am mainly going to talk about what you have been doing and are trying to do.

However, I am not familiar with what has been going on since last summer, when Bolin was my office mate at Woods Hole.

I wonder, whether could send me any publications since the Tellus article (if there are such). If not, could you tell me roughly what you have been doing? Also, in what state of completion is the Maniac?

It is really too bad that the Sigma Xi section here has been concentrating on local speakers, for I am sure that you would be able to give a better talk on this subject. However, I must admit that I like to talk.

Please give my best regards to Eleanor (or however else she spells herself) and have a good time

Hans

144
8
1152

NEW YORK UNIVERSITY
COLLEGE OF ENGINEERING
UNIVERSITY HEIGHTS, NEW YORK 53, N. Y.

DEPARTMENT OF METEOROLOGY

[redacted]
TELEPHONE: LUdlow 4-0700

Dear Jules,

When I was in Princeton I forgot to ask you for the reference of a paper you are or have been publishing in a European journal (Tellus?) about the relative magnitude of the terms in the equations of motion, the parts of divergence etc. - I should like to quote you - even if the article has not yet been published

Please answer this letter to WHOI

Regards

Hans

8 March 1949

Professor Hans Panofsky
Department of Meteorology
New York University
New York 53, New York

Dear Hans,

Thanks for your offer to replot the 500 mb maps. I am returning to you all the data you collected for us so that you can have the data plotted for the following dates.

January 12, 1946, 0300 Z
January 12, 1946, 1500 Z
January 13, 1946, 0300 Z.

I am sorry you had difficulty with my expense account, and although I have filled out a travel report and am returning it I am willing to drop the whole matter if it will cause you further trouble.

Regards,

Jule Charney

JC:AK

NEW YORK UNIVERSITY
COLLEGE OF ENGINEERING
UNIVERSITY HEIGHTS, NEW YORK 53, N. Y.

DEPARTMENT OF METEOROLOGY

[redacted]
TELEPHONE: LUDLOW 4-0700

4 March 1949

Dr. Jule Charney
Institute for Advanced Study
Princeton, New Jersey

Dear Jule:

I am sorry to delay you any further for the refund for your travel expenses. However, for sums larger than what can be paid out of petty cash the execution of the enclosed form is required. I am sorry that I did not send you the form before but I did not realize that your expenses had been so high.

By the way, do you want us to do any more with the data?

Regards to your wife, Nicky and X.

HAP

H. A. Panofsky

HAP:WL
Enclosure

NEW YORK UNIVERSITY
COLLEGE OF ENGINEERING
UNIVERSITY HEIGHTS, NEW YORK 53, N. Y.

DEPARTMENT OF METEOROLOGY

[redacted]
TELEPHONE: LUdlow 4-0700

18 January 1949

Dr. Jule Charney
Institute for Advanced Study
Princeton, New Jersey

Dear Jule:

Thanks a lot for the maps. However, we still need the mean maps for February. Could you possibly bring these to the meeting next week? Your maps should also be ready for you then.

Best regards to everybody.

Hans Panofsky

HAP:WL

Hans Panofsky

about Oct. 20, 1948

MRS. HANS A. PANOFSKY
318-A PLAZA ROAD
FAIRLAWN, RADBURN
NEW JERSEY

Dear Jule

This is supposed to be an official letter.

First I should like to ask you whether you would care to give us a seminar on your method of numerical integration on Friday, Nov 5, from 3-4.

Secondly, we are somewhat concerned about a matter of computation, which will come up later. Using "Cartesian" coordinates of Meteorology,

$$\xi = \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y} + \frac{u}{R} \tan\varphi$$

If we define $\vec{V}_{gs}$ by: $f\vec{V}\times\vec{k} = \frac{1}{\rho}\nabla p$,
and substitute, we get finally

$$\zeta_{gs} = \frac{1}{f\rho}\nabla^2 p + \frac{u_{gs}}{R}(\tan\varphi + \cot\varphi)$$

This last term ~~is~~ may be of the <u>same</u> order of magnitude as $\frac{1}{f\rho}\nabla^2 p$ for large ~~sc~~ scale waves, according to our estimate (5×10^{-6} sec^{-1}).

Our estimates show, by the way, that, for the large scale pattern, f is large compare to ζ.

We are going ahead as planned, computing elevations at all 100 mb surfaces.

Best regards

HAP

NEW YORK UNIVERSITY
COLLEGE OF ENGINEERING
UNIVERSITY HEIGHTS, NEW YORK 53, N. Y.

DEPARTMENT OF METEOROLOGY

[redacted]
TELEPHONE: LUDLOW 4-0700

28 May 1948

Dr. Jules Charney
Department of Meteorology
University of California
West Los Angeles, California

Dear Charney:

Would it be possible for you to stop at Flagstaff on your way back long enough to argue with me three hours or more? Write to me at Lowell Observatory there.

Best regards,

HAP:WL

Hans Panofsky

~~Report on~~ objective

Preliminary Report on Objective Weather Map Analysis.

By H. Panofsky.

This report was prepared under contract N6ori-139 between the Office of Naval Research and the Institute for Advanced Study. Valuable help was received and from the Meteorology Department of New York University, which made the author's part time services available. ~~and~~ from the ~~Math. Teller Proj or Comp. Lab~~ computation ~~laboratory~~ of ~~#~~ the National Bureau of Standards.

JULE GREGORY CHARNEY
MC 184 BOX 13, F. 443

PANOFSKY, HANS A., 1947-1966

OK 2/2

H. Panofski

REPORT ON OBJECTIVE ANALYSIS

The purposes of synoptic weather map analysis are roughly these:

1) To summarize the large number of meteorological data by relatively few lines and shaded areas in such a way as to give the most relevant information to the practicing meteorologist or air plane pilot.

2) To enable the theoretical meteorologist to perform certain computations on the data, such as differentiation, interpolation etc.

To summarize discrete characteristics of the weather, such as fog or precipitation, areas are generally shaded; continuous variables are summarized by means of isopleths, if the variables are scalar, or by streamlines if the variables are vectorial. # In addition, the boundaries of air masses are indicated by fronts.

Weather map analysis so far has been a subjective process. Isopleths, streamlines, and fronts are drawn by eye, with considerable use of "judgment". Judgment is necessary to (a) eliminate wrong observations, (b) to weight observations, (c) to smooth random errors of observations, (d) to eliminate local effects which are irrelevant ~~for~~ to the large scale picture, (e) to see that physical laws are not violated, (f) to establish continuity in time and in the third spacial dimension not indicated on the map. In general, different experienced analysts will arrive at similar isopleths or streamlines. Exceptions are found on surface maps in mountainous areas, where the treatment of local factors becomes preponderant, and on very high level charts or in oceanic areas where the data are insufficient to yield a unique analysis.

When shading areas containing precipitation or fog, analysts will generally not differ in the total picture, which is uniquely defined by the observations; they will differ, however, in mountainous areas; they will also differ in the detailed interpretation of the shaded area.

Analysis of fronts presents a different problem. There exist a large number of criteria for the placement of fronts. The different criteria agree only rarely. Hence, the final placement will depend on which criteria an individual analyst places most weight. The difficulty is that the process of weighting ~~of~~ the different factors is often not a rational one.

The term "objective analysis" will be interpreted to mean the process of finding a mathematical equation describing the field of the given observations. Once this equation is given, isopleths or streamlines can be constructed objectively. Interpolation of the field presents no further problem, since this is equivalent to computing values of the function at points between observations.

One advantage of "objective analysis" is that derivatives can be obtained at every point, once the equation which satisfactorily describes the observations is known. Normally, in Meteorology, derivatives are approximated by ratios of finite differences; derivatives determined in such a way are really average values over finite distances. Values of derivatives at specific points are difficult to determine by conventional processes. Still, the equations of hydrodynamics refer to point values of derivatives, and substitution of average values may lead to considerable error, unless allowance for the averaging is made.

At present, of course, "objective" analysis will require an excessive amount of time; however, with a fast electronic computer, this disadvantage probably can be eliminated entirely.

An objective analysis is possible, whenever the "judgment" or "interpretation" involved in the standard, subjective techniques of analysis, can be translated into rational, unambiguous rules. This rationalization of judgment seems sometimes difficult, especially in case of frontal analysis, in mountainous regions or at high elevations.

This first study of map analysis by an objective method was therefore limited to cases where, in the subjective analysis, "judgment" is used largely to smooth out random errors of observation and random effects of turbulence.

The wind field at several levels above the surface was chosen for the first test. Continuity in time and with height was ignored. The method used could, if required, be extended to smooth variation with time and height as well as in the horizontal.

One reason for choosing the wind field for the first test was that functions involving derivatives of the wind field, such as vorticity and divergence, are of special importance in theoretical computation and practical work.

The wind at a given elevation is determined by two scalar quantities, such as direction and speed, or two velocity components. In this case, two Cartesian components of the winds were formed, along the x and the y axis. The y axis was 95W meridian, the x axis at right angles to the y axis, passing through the point; longitude 95W, latitude 30N. These axes were constructed on a Lambert conformal conic projection, which was assumed to be true for the limited areas used in this study. The wind components in the coordinate system so defined will not simply be southerly or westerly. Only near longitude 95W, can these components be interpreted in this simple fashion.

It is assumed that the fields of u and v, the velocity components in the x and y directions, are given by separate polynomials in x and y. Since the errors of observation and the effect of turbulence can be assumed to add components to the wind which are distributed approximately normally, the method which seems the simplest and most promising for the computation of a polynomial which smoothes out these variations is the method of least squares.

Since the random factors in the winds are relatively large, the number of winds from which the polynomials are to be computed should be considerably larger than the number of coefficients in the polynomials.

A situation was chosen where the wind coverage in the Eastern third of the United States was quite complete up to 10,000 feet. The number of winds reported varied from 54 at 3000 to 36 at 9000'. It was decided to fit this wind field (Dec.2, 1946, 22 GMT) by independent third degree polynomials in x and y at the levels 3000', 5000', 7000', and 9000'. A third degree polynomial was selected since it has 10 coefficients, a number considered about right compared to the number of winds in the area.

Let the polynomial at level q be given by:*

$$P_q (x,y) = a_{i\,j} x^i y^j, \; i + j \leq 3 \qquad (1)$$

Then the ten normal equations needed in the solution for the coefficients a_{ij} can be summarized symbolically in the form

$$\sum_{ij} g_{ijkl} \, a_{ij} = h_{kl} \qquad (2)$$

where

$$g_{ijkl} = \sum_s x^{i+k} y^{j+l} \text{ and } h_{kl} = \sum_s p \, x^k y^l \qquad (3)$$

Here index s stands for summation over all stations at which winds were observed. p represents the observations, x and y are the coordinates of the observing stations.

The necessary meteorological data were collected at New York University, and the calculations indicated by equation (3) were carried out at the Bureau of Standards Mathematical Tables Group headed by Dr. Arnold Lowan, under the immediate supervision of Mr. Jack Laderman.

The Princeton Research Laboratories of the Radio Corporation of America have recently completed an electronic linear equation solver. This device was very obligingly put at our disposal, and it was applied to the two first sets of 10 normal equations with 10 unknowns (Equation 1), with the help of Mr. E.A. Goldberg of the

* This notation is the Summation Convention, which is usually adopted in Tensor Analysis.

RCA Laboratories. Since it is an analogy device with a relative precision of about 1 part in 1000 it could not be applied to the equations directly - the determinant g_{ijkl} vanished to that degree of the accuracy. A linear transformation of the coordinates to a new system, the center of which was the centroid of the stations used, and the axes of which were symmetrically located with respect to the stations and the units of length properly normalized, increased the corresponding determinant in the new system of coordinates above the tolerance in question. In this form the equations were solved with the electronic linear equation solver, and an iteration method permitted obtaining any desired degree of precision.

In practice, the transformation and subsequent solution by the electronic computer was used only in the case of the 3000' data; it was found later that the sets of ten equations with the ten unknowns could be solved very economically by the square root method with ordinary computing machines. The time required to solve 10 equations of the form (2) was finally reduced to two and one-half hours including checks, a time short compared to that required in the rest of the computations.

Figure (1) shows the winds smoothed least squares and, for comparison, the observed winds. Close inspection of the two parts of the figure shows that there is no systematic deviation between the two types of analysis. Thus the least square wind field may be regarded as a satisfactory representation of the broad field of flow on this particular date.

Since the wind field in the least square representation is given by a third order polynomial in x and y, isopleths of the divergence and vorticity must be conic sections. Figure (2) shows a comparison of the field of divergence at 3000' as computed from the objective wind analysis with the measurements of divergence by two meteorologists from the observed winds, using subjective techniques. As seen by comparison of the results of the two subjective measurements of divergence, the accuracy of the subjective techniques is very low. The agreement with the objective method is satisfactory.

Figure (3) compares the field of vorticity, evaluated by the objective method, with that determined from the circulations around finite areas. Again the agreement is as good as could be expected.

An entirely different check on the computation of divergence can be made by use of vertical velocities computed independently. If the equation of continuity is integrated with respect to height under the assumption that individual density changes are adiabatic, we obtain:

$$w_{10} - \left(\frac{p_2}{p_{10}}\right)^{\frac{1}{\Gamma}} w_2 = - \int_2^{10} \left(\frac{p}{p_{10}}\right)^{\frac{1}{\Gamma}} \operatorname{div}_2 \vec{V}\, dz \qquad (4)$$

where p is pressure, Γ is the ratio of specific heats $\frac{c_p}{c_v}$, and the limits of integration are given in thousands of feet. The integral can be replaced by a sum of values of divergence at 3000', 5000', 7000', and 9000', weighted by the proper factors. The vertical velocity w_2 at 2000' is presumably small, so that w_{10}, the vertical velocity at 10,000' can be compared with the vertical velocity determined by the "adiabatic"[1] method. The patterns are strikingly similar, with the apparent difference that the zero line of vertical velocity appears further to the west on the map of subjectively measured vertical velocities (Figure 4). This difference, however, is not serious, since the data used in the objective analysis did not reach as far West, and the computations in those regions could not be expected to agree with observation. Another difference is that the observed field shows less North South symmetry than the computed field.

[1] The principles of the adiabatic method for determining vertical velocities are outlined in appendix II.

$w_{10} - (p_2/p_{10})^{\frac{1}{\Gamma}}$

If divergences are measured subjectively at 3000', 5000', 7000', and 9000', and combined, values of vertical velocities at 10,000' are obtained which agree somewhat better with "adiabatic" vertical velocities than the values obtained objectively. For example, there is no forced North South symmetry; the upward motion appears stronger to the North than to the South.

On the other hand, the objective technique permits calculation of vertical velocities over the whole field of observing stations, whereas the area of subjective computation is more limited.

Figure (5) shows a correlation diagram of vertical velocities measured directly as compared to vertical velocities obtained by summations of objectively determined values of divergence. The points on this diagram are well scattered throughout the analyzed area. The correlation is good. However, the objective vertical velocities are, on the average, about twice as large as the vertical velocities measured directly. This is partly explained by the fact that the objective determination yields instantaneous vertical velocities, whereas vertical velocities determined by the adiabatic method represent average vertical motion over a twelve hour period.

It might have been expected that the ~~omissed~~ omission of the 2000' vertical velocity in the objective method would lead to systematic errors in the 10,000' values. Inspection of the surface charts shows that if the 2000' values could be included the discrepancy between the two sets of vertical velocities would have been increased to a slight extent.

As far as can be judged, then, the polynomial fitted to a windfield of limited area represents a satisfactory representation of the general flow.

The next question to be investigated is how adjacent areas in which separate polynomials have been computed can be connected.

In this case a vector field was chosen of "Resultant Winds". A resultant wind is defined by the equation: $\overrightarrow{R_{10}} = \int_s^{10} \overrightarrow{V_1} dz$, where the lower limit of the integral is the surface of the earth, the upper limit is 10,000'. One property of this vector field is that its divergence, multiplied by a correction factor, is nearly equal to the vertical velocity at 10,000' elevation. This can be checked by independent computation.

Accidental errors should have a considerably smaller effect on wind resultants than on winds at individual levels. Also eddies located in particular layers will influence the resultants to a smaller extent. Therefore, the ratio of coefficients in the polynomials to the number of measurements from which these coefficients are to be determined would be greater.

The observations for December 8, 1945, 22 GMT were chosen, since resultant wind vectors $\overrightarrow{R_{10}}$ could be obtained at unusually many observing stations East of 100°W longitude, and since vertical velocities were available for that period. The number of resultants available in the Midwestern and Eastern part of the United States was 57.

The total area was divided into two sections, C and D, by a straight line. The criteria for chosing this line were: (1) that the number of observing stations in each area was approximately the same and (2) that the distribution of observing stations in each area did not show great variation of density; a preliminary study had shown that, if few observations were available in one part of an area and many in the other, the observations in the section containing few data influence the final coefficient in the polynomial to such an extent that a small error in the observations will influence the values of the coefficients in the polynomials significantly.

First, separate third order polynomials were fitted to the x and y components of the resultant vectors in both areas, and the divergences computed. Figure 6 shows the distribution of vertical velocities: $w = -1.15 \operatorname{div} \vec{R}$. As was to be expected, the field is discontinuous along the boundary AA' between the areas C and D. Figure 7 shows the vertical velocities measured by the adiabatic method, for the same period.

The two third order equations in the regions C and D were combined into a single fourth degree equation by the assumption that the coefficients in the original cubics vary linearly at right angles to the line separating areas C and D. If the x' direction is defined as a direction at right angles to this dividing line, the coefficients a_{ij} are then given by an equation of the form:

$$a_{ij} = m_{ij}x' + n_{ij}$$

The m's and n's were computed under the assumption that the values of the a's were equal to the values determined for the individual cubics at the position of the centers of gravity of areas C and D.

Figure 8 shows the distribution of "objective" vertical velocities obtained from the divergence of the fourth degree equation. Apparently the smoothing process had a pronounced effect on the distribution of vertical velocities.

The agreement between the objectively computed vertical velocities with the "adiabatic" values is only fair. Both methods show some downward motion in the Eastern part of the United States, but the adiabatic region of downward motion is located much farther to the North.

Also in the West a discrepancy exists; the "adiabatic" regions of downward motion do not extend as far Eastward as the objectively computed ones.

Next, the possibility of dividing the total area into smaller sections and using lower degree polynomials in each was investigated. Area C was divided into to sub-areas, C_1 and C_2, and area D into D_1 and D_2. Figure 9 shows a map with this subdivision. Again, the lines were chosen in such a way as to yield as nearly a uniform distribution in each sub-area as possible, with nearly the same total number of stations in each. The number of stations reporting wind resultants in each sub-area varied from 13 to 15. Quadratic polynomials were fitted to the field of resultants separately in each sub-area. Thus, the ratio of number of observations to number of coefficients was a little larger than 3:1.

Figure 9 shows the distribution of vertical velocities as computed from the quadratic vector fields in each region. The fields of vertical velocity are, of course, linear and discontinuous at the dividing lines. They give, however, a correct impression of the distribution of vertical velocity, (compare Figure 7).

The quadratic polynomials in the four-subareas were combined by assuming a linear variation of the coefficients a_{ij} of the form:

$$a_{ij} = 0_{ij}\, x + p_{ij}\, y + q \qquad (5)$$

where x and y are the original Cartesian coordinates. Apparently, four sets of coefficients are available to determine the three constants 0, p, and q. They were evaluated by least squares, assuming that the values of the a's at the centers of gravity of the sub-areas previously determined should be fitted by Equation 5 with minimum square residuals. This leads to a cubic resultant field for the whole area, and a quadratic field of vertical velocity. As is apparent from the previous figures, a quadratic field cannot be expected to fit the field of vertical velocity in the whole area accurately. Figure 10 shows this quadratic field of vertical velocity.

~~Effect of error of observation on divergence.~~

Previous experience with subjective methods of measuring divergences of winds or resultant winds indicated that observational errors would have a large effect on the measured value. With subjective techniques it is difficult to estimate this effect quantitatively.

The observations in region D were subjected to random errors estimated to be of the same magnitude as the errors of observation. The frequency of errors of given sizes are summarized in the following table:

Error:	-8	-7	-6	-5	-4	-3	-2	-1	-0	1	2	3	4	5	6	7	8
Frequency:	1	1	1	2	2	2	3	3	3	3	3	2	2	2	1	1	1

The unit of the errors is $(100 \text{ m})^2 \text{ min}^{-1}$. Each possible error was written on one or more cards (depending on the frequency in the above table) and the cards were shuffled. A card was drawn at random and the error on the card was added algebraically to the first observation. The card was put back into the deck, the deck was shuffled again and a new card was drawn. Its error was added to the second observation and so forth, for all observations. The table below shows the vertical velocities computed from the altered observations at certain points compared with the original computations:

STATION	ORIGINAL VERTICAL VELOCITY	WITH ADDED ERROR
Big Springs, Texas	0 cm sec^{-1}	.3 cm sec^{-1}
Kansas City, Mo.	-2.4 "	-2.1 "
St. Louis, Mo.	-1.7 "	-1.0 "
Memphis, Tenn.	.9 "	.8 "
Tallhassee, Fla.	-.6 "	-1.6 "
Jacksonville, Fla.	-.5 "	-1.4 "

This sample seems to indicate that the errors did not influence sign significantly, but that the magnitudes are affected considerably.

In Figure 6 the dashed lines indicate the position of the zero lines as evaluated from the altered observations. Apparently the position of the zero lines is not very greatly affected, although the field of divergence, which was previously elliptic is now hyperbolic. It may then be stated that errors of observations, if their magnitude was estimated correctly here, will only rarely alter the sign, but will often alter the magnitude of the divergence considerably.

APPENDIX I

Let N observations, numbered n = 1,..N, be made; observation no. n being made at the point with the coordinates x_n, y_n, and producing the result f_n. It is desired to find a cubic polynomial

$$P(x,y) = \sum_{\substack{ij=0,1,2,3 \\ i+j \leq 3}} a_{ij} x^i y^j$$

which minimises the mean square deviation

$$\Delta^2 = \frac{1}{N} \sum_{n=1}^{N} \left(P(x_n, y_n) - f_n \right)^2$$

In order to do this form first

$$\bar{X} = \frac{1}{N} \sum_{n=1}^{N} x_n , \quad \bar{Y} = \frac{1}{N} \sum_{n=1}^{N} y_n$$

and then introduce the new variables

$$x_n' = x_n - \bar{x} , \quad y_n' = y_n - \bar{y} \qquad (n = 1, \ldots N)$$

Form next

$$\alpha = \overline{x'^2} = \frac{1}{N} \sum_{n=1}^{N} x_n'^2$$

$$\beta = \overline{x'y'} = \frac{1}{N} \sum_{n=1}^{N} x_n' y_n'$$

$$\gamma = \overline{y'^2} = \frac{1}{N} \sum_{n=1}^{N} y'^2$$

then

$$\nu = \frac{\beta}{\gamma} , \quad \alpha^* = \alpha - \frac{\beta^2}{\gamma}$$

and then introduce the new variables

$$x_n^* = \frac{1}{\sqrt{3\alpha^*}} \left(x_n' - \nu y_n' \right)$$

~~(Note that~~

$$y_n^* = \frac{1}{\sqrt{3\gamma}} y_n' \qquad (n = 1, \ldots, N)$$

Now form

$$A_{ij} = \overline{x^{*i} y^{*j}} = \frac{1}{N} \sum_{n=1}^{N} x_n^{*i} y_n^{*j}$$

for

$$i, j = 0, 1, \ldots 6 \qquad i + j \leq 6$$

Note, as a check, that necessarily

$$A_{00} = 1, \quad A_{10} = A_{01} = A_{11} = 0, \quad A_{20} = A_{02} = \frac{1}{3}$$

Form, furthermore,

$$B_{ij} = x^{*i} y^{*j} f = \frac{1}{N} \sum_{n=1}^{N} x_n^{*i} y_n^{*j} f_n$$

for

$$i, j = 0,1,2,3 \qquad i+j \leqq 3$$

Form the matrix

$$a = \begin{matrix} A_{00} & A_{20} & A_{02} & A_{01} & A_{21} & A_{03} & A_{10} & A_{12} & A_{30} & A_{10} \\ A_{20} & A_{40} & A_{22} & A_{21} & A_{41} & A_{23} & A_{30} & A_{32} & A_{50} & A_{30} \\ A_{02} & A_{22} & A_{04} & A_{03} & A_{23} & A_{05} & A_{12} & A_{14} & A_{32} & A_{13} \\ A_{01} & A_{21} & A_{03} & A_{02} & A_{22} & A_{04} & A_{11} & A_{13} & A_{31} & A_{12} \\ A_{21} & A_{41} & A_{23} & A_{22} & A_{42} & A_{24} & A_{31} & A_{33} & A_{51} & A_{32} \\ A_{03} & A_{23} & A_{05} & A_{04} & A_{24} & A_{06} & A_{13} & A_{15} & A_{33} & A_{14} \\ A_{10} & A_{30} & A_{12} & A_{11} & A_{31} & A_{13} & A_{20} & A_{22} & A_{40} & A_{21} \\ A_{12} & A_{32} & A_{14} & A_{13} & A_{33} & A_{15} & A_{22} & A_{24} & A_{42} & A_{23} \\ A_{30} & A_{50} & A_{32} & A_{31} & A_{51} & A_{33} & A_{40} & A_{42} & A_{60} & A_{41} \\ A_{11} & A_{31} & A_{13} & A_{12} & A_{32} & A_{14} & A_{21} & A_{23} & A_{41} & A_{22} \end{matrix}$$

(Note the somewhat unusual arrangement of the A_{ij}) and the vector

$$(b--- b_{10}) = (B_{00}\ B_{20}\ B_{02}\ B_{01}\ B_{21}\ B_{03}\ B_{10}\ B_{12}\ B_{30}\ B_{11})$$

(Note the somewhat unusual arrangement of the B_{ij}).

The coordinate system which was used in setting up this matrix was chosen in such a manner as to make the positions of the observations stations as random as possible: The mean as well as the dispersion of each coordinate is the same as if each were randomly distributed in the interval -1, 1, and their correlation is 0. If they were randomly and independently distributed, then the matrix a would be

$$a_o = \begin{matrix} 1 & 1/3 & 1/3 & & & & & & & \\ 1/3 & 1/5 & 1/9 & & & & & & & \\ 1/3 & 1/9 & 1/5 & & & & & & & \\ & & & 1/3 & 1/9 & 1/5 & & & & \\ & & & 1/9 & 1/15 & 1/15 & & & & \\ & & & 1/5 & 1/15 & 1/7 & & & & \\ & & & & & & 1/3 & 1/9 & 1/5 & \\ & & & & & & 1/9 & 1/15 & 1/15 & \\ & & & & & & 1/5 & 1/15 & 1/7 & \\ & & & & & & & & & 1/9 \end{matrix}$$

This should be a reasonable approximation to the true a for orientation purposes, but one should not expect to find it adequate for the equations which follow. The 72 empty fields are all occupied by O's.

APPENDIX II

The adiabatic method is based on the following derivation:

$$\frac{dT}{dt} = w\,\frac{dT}{dz} = \frac{\partial T}{\partial t} + V_2 \cdot \nabla_2\, T$$

$6w = 12$

$w = \frac{12}{6} = 2$

where T is the temperature and subscript 2 denotes a two dimensional vector.

Hence,

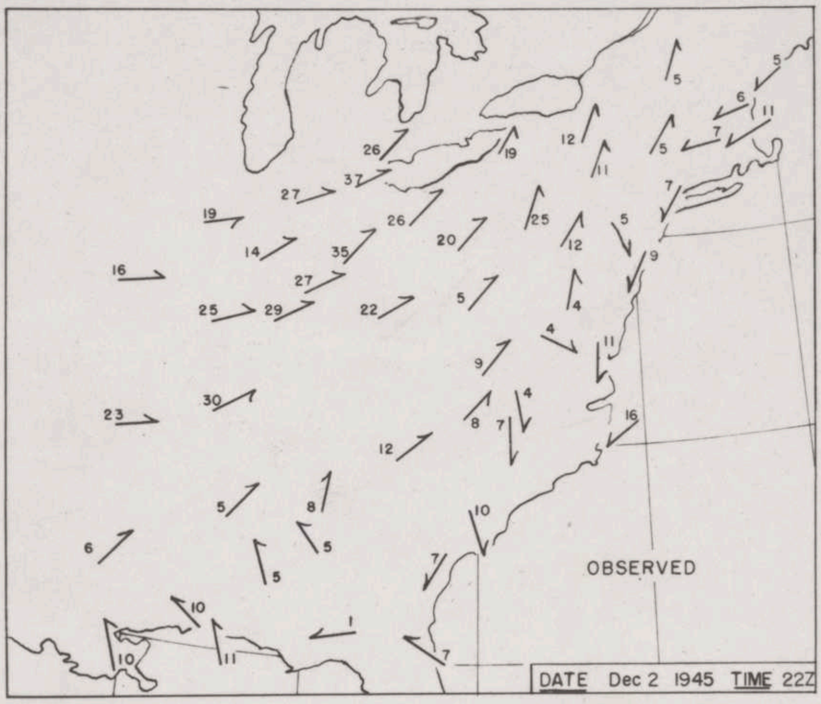
$$w = \frac{\frac{\partial T}{\partial t} + V_2 \cdot \nabla_2 T}{\left(\frac{dT}{dz} - \frac{\partial T}{\partial z}\right)}$$

$\partial T/\partial t + V_2 \cdot \nabla_2 T = w\left(\frac{dT}{dz} - \frac{\partial T}{\partial z}\right)$

$w\left(\frac{dT}{dz} - \frac{\partial T}{\partial z}\right) = \frac{\partial T}{\partial t} + V_2 \cdot \nabla_2 T$

All quantities on the right can be measured, if the individual air motion is assumed adiabatic and $dT/dz = -\ 10°C\ km^{-1}$.

The adiabatic vertical velocities used in this report are based on the "isobaric" technique. From observed geostrophic winds, 12 hour isobaric air trajectories are constructed. The numerator in the expression for the vertical velocities may be interpreted to mean the individual temperature change of a particle following the horizontal motion. Since the difference between horizontal and isobaric trajectories may be neglected, the difference between final and initial temperatures along an isobaric trajectory yields the numerator required. The value of $\frac{\partial T}{\partial z}$ in the denominator is obtained from isopleths of vertical temperature differences on isobaric charts.



WIND SPEEDS GIVEN IN MILES PER HOUR

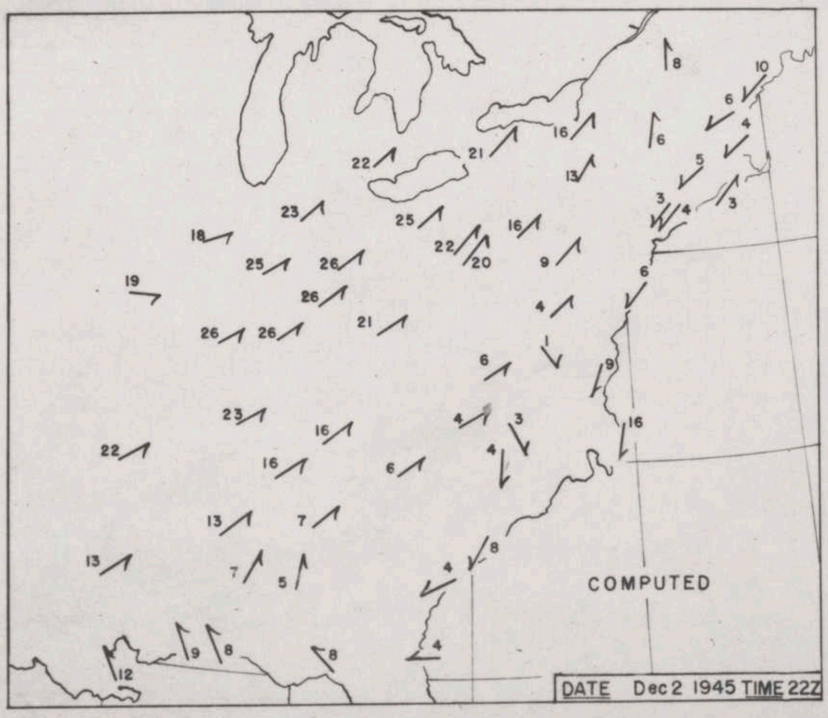


FIG. 1 3000 Ft. WINDS

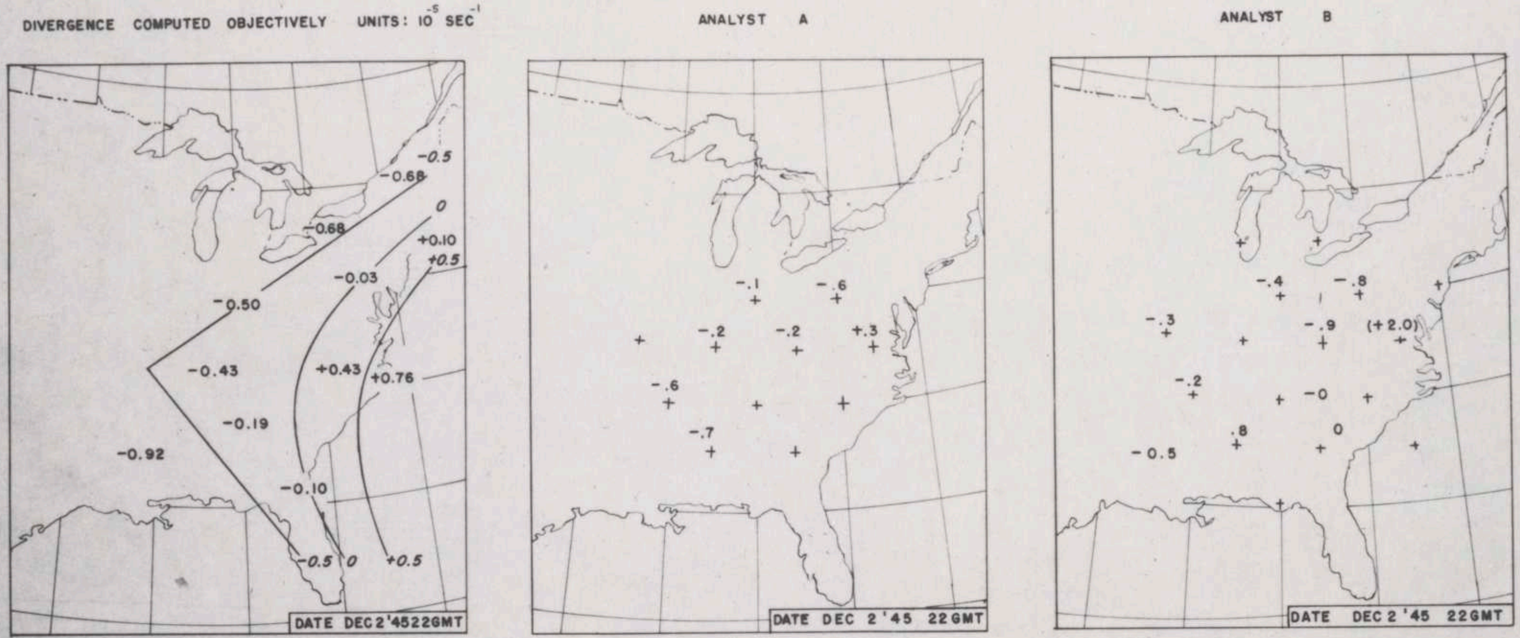


FIG. 2 DIVERGENCE AT 3000 FT.
IN 10^{-5} SEC^{-1}.

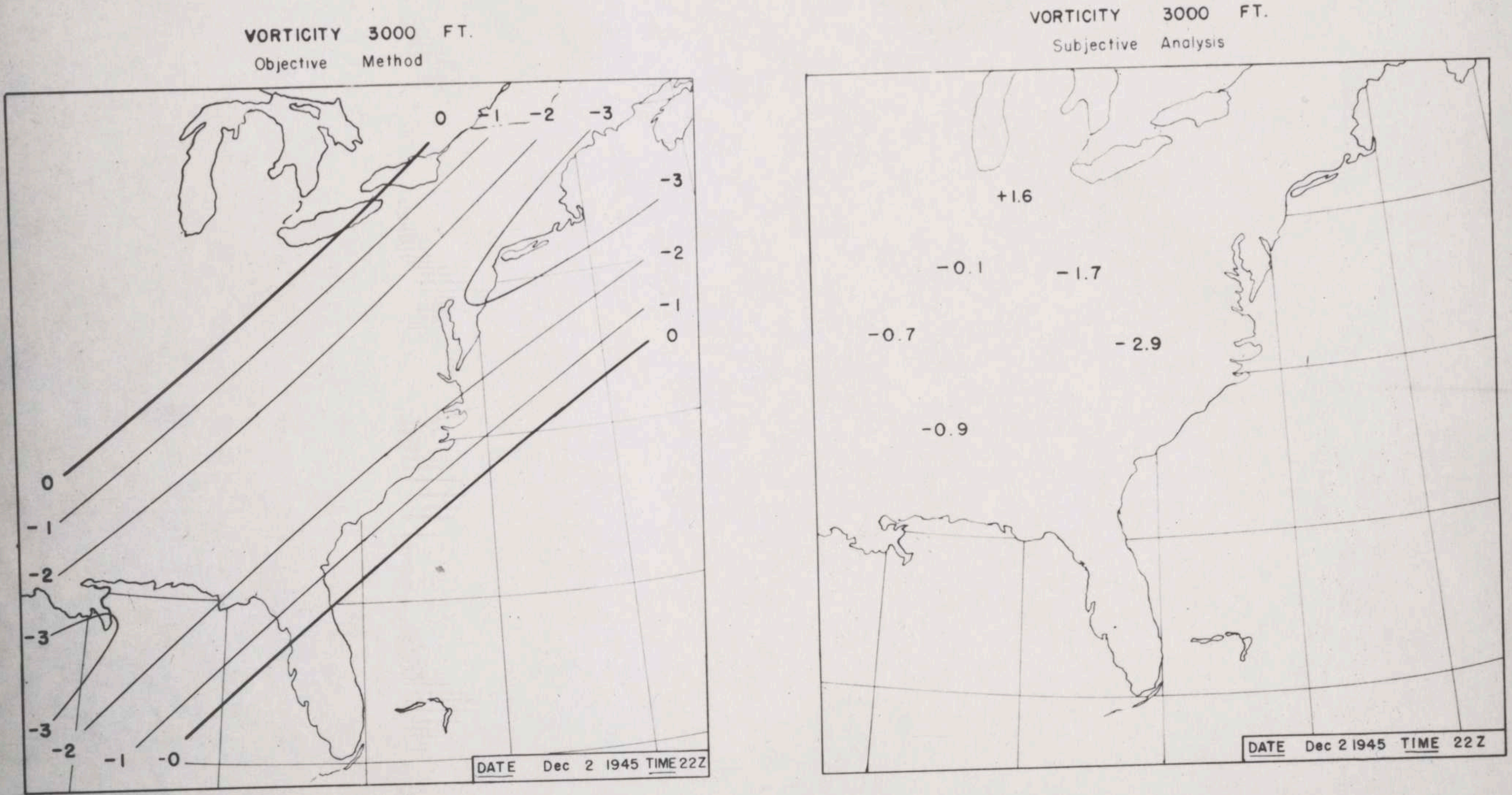


FIG. 3 VORTICITY IN 10^{-5} SEC^{-1}

INSTANTANEOUS VERTICAL VELOCITIES AT 10,000'
OBJECTIVE COMPUTATION

12 HOUR AVERAGE VERTICAL VELOCITY AT 10,000'
SUBJECTIVE MEASUREMENT BY ADIABATIC METHOD

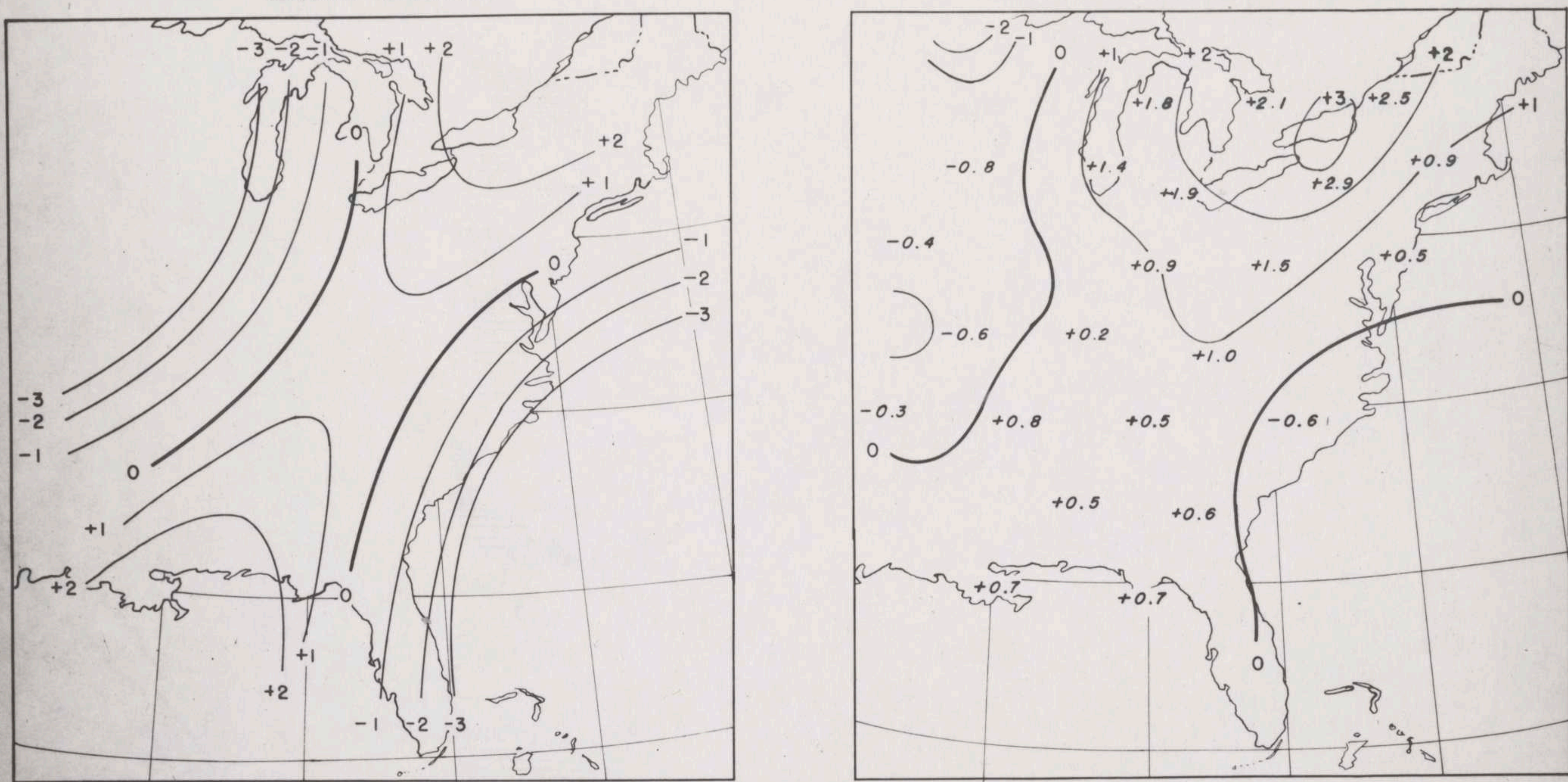


FIG. 4 Vertical Velocities In Cm Sec^{-1}

For Dec 2, 1945, 22 GMT.

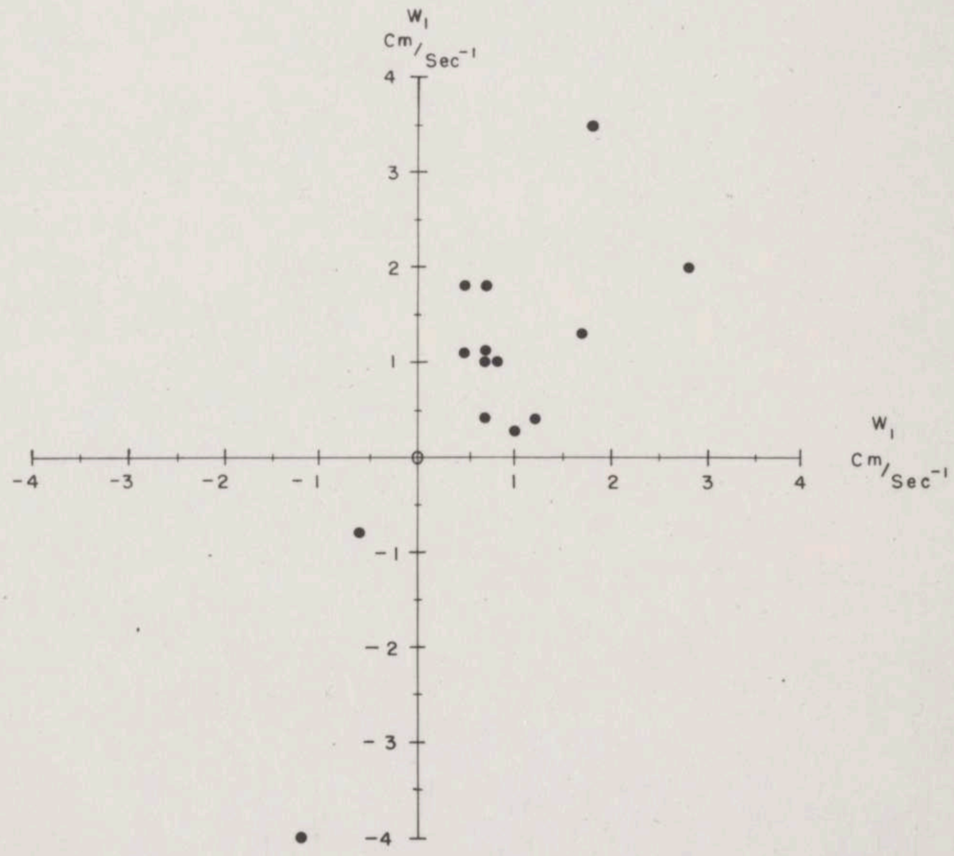


Fig. 5 Vertical Velocities At Selected Points.

ABSCISSA : Subjective. — 12 Hour Average.

ORDINATE : Objective — Instantaneous.

Objective Analysis, Report 1947-48

John von Neumann, Institute for Advanced Study
H. A. Panofsky, New York University.

Introduction

One of the aims of the Meteorological Computer Project at Princeton is the application of the hydrodynamic equations to forecasting.

Initial conditions for this problem would be furnished by the observations at a starting time, t_o. At time t_o it would be required to know the meteorological variables and some of their space derivitives at certain grid points. This could be accomplished by the normal modes of analysis, that is, the subjective drawing of isopleths. The derivatives could be approximated by ratios of finite differences. A disadvantage of this process is that it is subjective; another, that the data must first be plotted.

These disadvantages could be avoided if the meteorological variables could be related objectively to the space co-ordinates by analytic functions. Then the coded weather reports could be translated into initial conditions without human interference.

The problem is then to find an analytic function p(x, y, z) which represents the distribution of the meteorological variable p in three dimensions. In all computations so far, however, the vertical dimension (or pressure) has been held constant; the "objective" analysis completed is thus analogous to the subjective analysis of constant level or constant pressure charts.

Also, the analysis has been restricted to relatively small areas over which third order polynomials fit the data satisfactorily.

A third order polynomial can be written in the form:

$$p(x,y) = \sum_{ij} a_{ij}\, x^i y^j \quad (i+j \leqq 3)$$

where x and y are Carthesian co-ordinates. There are altogether 10 coefficients a_{ij}. The size of the area to which such a polynomial is to be fitted will depend on how much the observations are to be smoothed. A field of 10 observations can be fitted accurately by a third order polynomial, with no smoothing of data. Some smoothing is however desirable in all cases, since all observations are influenced by observational errors and more or less local eddies. The polynomials cannot possibly be expected to fit these eddies, but should rather represent the large scale features of the field to be analyzed.

The amount of smoothing will depend on the nature of the variable analyzed; winds, for example, should be smoothed more than pressures. If a pressure field is to be analyzed, 12 - 14 observations should be used to determine the 10 constants of the polynomial; in case of the East-West component of wind, no less than 20 observations are required.

The problem of determining n coefficients from m observations (m>n) is usually handled by the method of least squares. This would yield the most probable polynomial if it is assumed that eddies and observational errors are distributed normally about the smoothed field. Even if the distribution is not normal, the least square solution represents a probable state on which errors and eddies have been superimposed.

Generally, the condition of least squares for a cubic polynomial in x and y leads to ten normal equations with the ten values of a_{ij} as unknowns. Considerable algebra is involved in computing the coefficients in these equations and in their solution. The time needed to do these computations with standard computing machines is long compared to a forecast period; however, the computing time necessary for the projected electronic computer is not expected to be extravagant. The considerable computations necessary in the objective analysis to be described here were made by the Computation Laboratory of the Bureau of Standars at New York City, headed by Arnold Lowen, under the immediate supervision of Jack Laderman.

Analysis of a Wind Field

A situation was chosen where the wind coverage in the Eastern third of the United States was nearly complete up to 10,000. The number of wind reports varied from 54 to 3,000' to 36 at 9,000'. Cartesian coordinates x and y were defined as follows: The y axis was the 95th Meridian, the x axis at right angles to it and passing through the point: 30°N, 95°W. The coordinates are defined on a Lambert conformal projection.

The u component (in the x direction) and the v component of wind (in the y direction) were fitted by independent cubic polynomials, at 3,000', 5,000', 7,000' and 9,000'.

Figure 1 shows the observed winds (above) and the wind computed from the cubic (below) at 3,000'. The numbers indicate speed in miles per hour. Apparently the computed winds show little, if any, systematic difference from the observed winds. They are, of course, a great deal less erratic. It may thus be stated that the cubics fit the observations satisfactorily.

Figure 2 shows the divergence of the same field. On the left, it is shown as computed from the cubics. Lines of constant divergence are therefore conic sections. The maps in the center and to the right show the fields of divergence determined by two analysts, A and B, by conventional methods. Apparantly, the two sets of subjective measurement agree with each other no better than with the objective determination. In general features, all three maps are similar.

Figure 3 compares fields of vertical velocities for the same period. On the map on the left, the vertical velocities have been computed objectively by adding the properly weighted values of divergence at 3,000', 5,000', 7,000' and 9,000'. Again, lines of constant vertical velocity are conic sections. Also these vertical velocities are instantaneous. The vertical velocities on the right were computed completely independently by the adiabatic method. This method yields 12 hour average vertical velocities. The agreement is as good as can be expected.

Combination of Separate Areas

The objective analysis as described so far leads to independent polynomial expressions in separate areas of limited size. At the boundaries, the analyzed field is discontinuous. This discontinuity has no physical reality since it depended on the arbitrary choice of the area. Therefore a method must be divised which smoothes out these discontinuities.

It is now postulated that the coefficients of the individual polynomials only apply at the center of gravity of the areas, or perhaps only at a centroid line. Between those points, the coefficients are assumed to vary by a simple law. If only 2 areas are analyzed, the coefficients can be taken constant along certain directions and made to vary linearly in another; if more areas are to be connected, the coefficients of the individual polynomials are again polynomials in x and y, the order depending on the number of areas.

Instead of thus piecing together of separate areas it may appear that spherical harmonics lead to better results when objective analysis is to be applied to a hemisphere or the whole earth.

The process of piecing together was studied on a field of resultant vectors. Horizontal components of resultant vectors are defined by;

$$R_x = \int_0^h u \, dz \text{ and } R_y = \int_0^h v \, dz$$

These can be obtained directly from pilot balloon runs and have the property that their divergence is proportional to vertical velocities, which can be determined independently.

Accidental errors should have a considerably smaller effect on wind resultants than on winds at individual levels. Also eddies located in particular layers will influence the resultants to a smaller extent. Therefore, the ratio of coefficients in the polynomials to the number of measurements from which these coefficients are to be determined would be greater.

The observations for December 8, 1945, 22 GCT were chosen, since resultant wind vectors R_{10} could be obtained at unusually many observing stations East of 100°W longitude, and since vertical velocities were available for that period. The number of resultants available in the Midwestern and Eastern part of the United States was 57.

The total area was divided into two sections, C and D, by a straight line. The criteria for choosing this line were: 1) that the number of observing stations in each area was approximately the same and 2) that the distribution of observing stations in each area did not show great variation of density; a preliminary study had shown that observations in the section containing few data influence the final coefficient in the polynomial to such an extent that a small error in the observations will influence the values of the coefficients in the polynomials significantly.

First, separate third order polynomials were fitted to the x and y components of the resultant vectors in both areas, and the divergences computed. Figure 4 shows the distribution of vertical velocities: W = -1.15 div R. As was to be expected, the field is discontinuous along the boundary AA' between the areas C and D.

The two third order equations in the regions C and D were combined into a single fourth degree equation by the assumption that the coefficients in the original cubics vary linearly at right angles to the line separating areas C and D. If the x' direction is defined as a direction at right angles to this dividing line, the coefficients a_{ij} are then given by an equation of the form:

$$a_{ij} = m_{ij} x' + n_{ij}$$

The m's and n's were computed under the assumption that the values of the a's were equal to the values determined for the individual cubics at the position of the centers of gravity of areas C and D.

Figure 5 shows the distribution of "objective" vertical velocities obtained from the divergence of the fourth degree equation. Apparently the smoothing process had a pronounced effect on the distribution of vertical velocities.

Influence of Observational Errors on Objectively Determined Vertical Velocity.

Previous experience with subjective methods of measuring divergences of winds or resultant winds indicated that observational errors would have a large effect on the measured value. With objective techniques this effect can be estimated quantitatively.

The observations in region D were subjected to random errors estimated to be of the same magnitude as the errors of observation. The frequency of errors of given sizes are summarized in the following table:

Error:	-8	-7	-6	-5	-4	-3	-2	-1	-0	1	2	3	4	5	6	7	8
Frequency:	1	1	1	2	2	2	3	3	3	3	3	2	2	2	1	1	1

The unit of the errors is $(100\ m)^2\ min^{-1}$. Each possible

possible error was written on one or more cards (depending on the frequency in the above table) and the cards were shuffled. A card was drawn at random and the error on the card was added algebraically to the first observation. The card was put back into the deck, the deck was shuffled again and a new card was drawn. Its error was added to the second observation and so forth, for all observations. The table below shows the vertical velocities computed from the altered observations at certain points compared with the original computations:

STATION	ORIGINAL VERTICAL VELOCITY	WITH ADDED ERROR
Big Springs, Texas	0 cm sec^{-1}	.3 cm sec^{-1}
Kansas City, Missouri	-2.4 "	-2.1 "
St. Louis, Missouri	-1.7 "	-1.0 "
Memphis, Tennessee	.0 "	.8 "
Tallahassee, Florida	-.6 "	-1.6 "
Jacksonville, Florida	-.5 "	-1.4 "

This sample seems to indicate that the errors did not influence sign significantly, but that the magnitudes are affected considerably.

In Figure 4 the dashed lines indicate the position of the zero lines as evaluated from the altered observations. Apparently the position of the zero lines is not very greatly affected, although the field of divergence, which was previously elliptic is now hyperbolic.

Analysis of a Contour Field.

Contour lines are rarely analyzed on the basis of reported elevations alone. Winds, usually aid the analyst. The majority of forecasters consider wind direction only and attempt to make the ~~wind~~ contour lines as nearly parallel to the wind as possible. Usually the speed is used to indicate the reliability of the direction. A minority of meteorologists make use of the wind speed to adjust the spacing of contour lines, using the geostrophic wind scale.

Both of these procedures can be incorporated into objective analysis. The quantity $\vec{V}.$ grad h (V horizontal wind, h elevation) is zero if wind and contour lines are parallel. Hence the condition: $\Sigma\,(\vec{V}.\ \mathrm{grad}\ h)^2 = \min$ describes mathematically the subjective procedure of adjusting contour lines to fit the winds. Since the analysis has to fit winds and elevations simultaneously, the polynomial for h should be subjected to the condition:

$$a\Sigma_{s'}(h-h_o)^2 + \Sigma_{s'}\,(\vec{V}_o\ \mathrm{grad}\ h)^2 = \min$$

s denotes summation over radiosonde station, s' over pilot balloon station. Subscript o denotes observed, a is a constant which depends on the relative weight of winds and elevations. This constant has dimensions and depends therefore on the units used for elevation and wind.

If wind speed is to be used to space the contour lines in addition to wind direction and elevations, this condition may be used:

$$b\sum_{s}(h-h_o)^2 + \sum_{s'}(u-u_{gs})^2 + \sum(v-v_{gs})^2 = \min$$

Subscript gs stands for geostrophic. b is a constant different from a.

Figure 6 shows the data and subjective analysis. There are 8 elevations and 14 winds, judged about sufficient to determine the 10 constants of the elevation polynomial.

Figures 7 and 8 show the objective analysis by the two techniques.

Both objective techniques give results very much alike; they differ less from each other than from the subjective analysis. Both objective techniques differ from the subjective analysis by a more even spacing and simpler pattern. It may be argued that the objective analyses are actually preferable in this case since the aim was to eliminate details superimposed on the broad field. The deviations of the observations from the analyzed field are small enough to be accounted for by errors and eddies.

A change of the weight b by a factor 3 did not change the analysis significantly.

Horizontal Gradients of the field of h and its Laplacian were also computed. The agreement in the gradients was tolerable; that in the Laplacian not good. This indicates that meteorological data at present are insufficient to yield reliable values of second derivatives.

This conclusion was substantiated by an evaluation of the effect of observational errors on the derivatives of the elevation field.

The effect of the observational errors is illustrated below:

	$\frac{\partial\varphi}{\partial x}$		$\frac{\partial\varphi}{\partial y}$		$\frac{\partial^2\varphi}{\partial x^2} + \frac{\partial^2\varphi}{\partial y^2}$	
	obs.	with error	obs.	with error	obs.	with error
Brunswick, Ga.	0	- .9	-26.1	-24.8	+25.5	+18.8
New Orleans	- 8.6	- 6.8	-12.4	-14.8	-23.5	- 4.9
Nashville	-14.4	-12.1	-24.6	-29.1	+13.0	-11.7
Atlanta	- 8.6	- 7.3	-25.2	-25.3	+19.3	+ 3.6
Salisbury, N.C.	- 2.5	- 2.6	-25.6	-29.7	+40.6	+ 7.8

These values are based on the following distribution of errors:

For u and v (m sec^{-1})

u or v	-20	-15	-10	-5	0	5	10	15	20
weight	1	1	2	3	3	3	2	1	1

For φ (in megergs gm^{-1})

φ	-2	-1.5	-1.0	-0.5	0	0.5	1.0	1.5	2.0
weight	1	1	2	3	3	3	2	1	1

The assumed errors in the wind components are possibly too large, hence the changes produced on the polynomials too great. Even so, it appears that computation of the Laplacian of pressure (or height) is not reliable, and should be avoided.

The deviation from geostrophic wind

The fundamental equations of Meteorology can be transformed into a complete system of equations in which partial derivatives with respect to time can be expressed in terms of the meteorological variable and their gradients. This system of equations can be integrated numerically, provided that the quantities on the right side of the equation are known with sufficient accuracy.

Two of the equations of this complete system are the two horizontal components of the vector equation of motion, solved for the time derivatives:

$$\frac{\partial u}{\partial t} = - u\frac{\partial u}{\partial x} - v\frac{\partial u}{\partial y} - w\frac{\partial u}{\partial z} + f(v-v_{gs}) + F_x \qquad (1)$$

$$\frac{\partial v}{\partial t} = - u\frac{\partial v}{\partial x} - v\frac{\partial v}{\partial y} - w\frac{\partial v}{\partial z} - f(u-u_{gs}) + F_y \qquad (2)$$

The notation is standard. u, v and w are the three wind components in the directions x, y and z, f is the Coriolis parameter, t time. The subscript gs stands for geostrophic. F is the component of the force of friction in the direction specified by the subscript.

The terms $u - u_{gs}$ and $v - v_{gs}$ which appear prominently on the right side of equations 1 and 2 are the components of "nongeostrophic wind vector". The purpose of this report is the discussion of the measurement of this vector and its components.

Many attempts have been made in the past to measure the deviations from geostrophic wind. The easiest method consists in drawing smooth isobars, computing the geostrophic wind from the distance of the isobars and subtract it from the reported wind observation. This method leads to erroneous results for two important reasons:

1) The isobars are usually not drawn independently of the wind

2) The geostrophic wind is an average over the space between the isobars. Moreover, the isobars are smoothed and drawn from relatively distant observations. The winds, on the other hand, are local and not smoothed

These difficulties can be overcome by

1) Independent analysis of wind and pressure (or contour) fields

2) Consistent smoothing of the two fields.

Two situations were treated in this manner:

A) The wind and contour field at 700mb, Mar. 25, 1947, 1500Z and

B) The wind and pressure field at 10,000', Dec. 2, 1944, 1600Z

The observations are given in figures 9 and 10. In situation A, even the subjective analysis indicates a blowing of the winds across the contour lines, toward lower elevations; also, in the Eastern part of the area, the winds are far below geostrophic.

In situation B, deviations from geostrophic winds appear to be less regular.

If pressure (or elevation) are represented by a cubic in x and y, other geostrophic wind components are quadritics. Hence it was decided to fit quadratics to the observed wind component also.

The areas were chosen in such a way that they contained 12 observations of pressure (or elevation). In both cases, about 25 wind reports were available in the same areas.

On both figures 9 and 10, rectangles were drawn to indicate the area analyzed in detail. They were chosen so as to be completely surrounded by both pressure (or elevation) and wind observations.

Figures 11 and 12 show the deviation vectors from geostrophic wind in the areas outlined in figures 9 and 10, computed from the polynomials of p, or h, u and v. Isobars (or contour lines) are also given.

In situation A, the geostrophic deviations point toward lower elevations, and are directed against the main flow in the Eastern part, as expected. In situation B the geostrophic deviations form an anticyclonic wheel, centered slightly East of the wedge line.

The data used in this study were modified by random "errors", of a magnitude consistent with observational errors. The following table gives the errors and their weights:

for u and v (in m sec^{-1})

u or v	-12	-8	-4	-2	0	2	4	8	12
weight	1	1	2	3	3	3	2	1	1

for p(in mb) and geopotential (megergs gm^{-1})

φ or p	-1.8	-1.2	-.8	-.4	0	.4	.8	1.2	1.8
weight	1	1	2	3	3	3	2	1	1

Figures 13 and 14 show the geostrophic deviations with errors applied. Apparently, the effect of the errors on the final result is much greater in situation B than in A. Indeed, it becomes likely that the geostrophic deviations of situation B (fig. 12) are extremely uncertain.

It is desirable to test the reliability of the computed nongeostrophic wind vectors shown in figures 11 and 12 by independent measurements.

Let $\vec{V}_n$ represent the nongeostrophic wind vector. Then the horizontal component of the equation of motion may be written:

$$\vec{V}_n = \frac{1}{f}\left(\vec{F} - \frac{d\vec{V}}{dt}\right) \times \vec{k} \qquad (3)$$

where F is the horizontal component of the force of friction and k is a unit vertical vector.

In order to estimate the nongeostrophic wind vector to be expected it is first assumed that $\vec{F}$ is small compared to $d\vec{V}/dt$ at 700 mb. The term $d\vec{V}/dt$ indicates the change of the horizontal motion following a parcel in three dimensions. It can be computed most easily from isentropic charts if individual temperature changes can be assumed adiabatic.

In the case of situation A, isentropic charts were not available, and change of speed along an isobaric trajectory was substituted for that along an isentropic trajectory. The geostrophic deviations expected from these accelerations are indicated in figure 15. The agreement between this and the observed geostrophic deviations (figure 11) is tolerable; both the direction toward lower elevation, and the direction against the flow in the Eastern part of the area agree. Better agreement is not likely to be expected because a) observed geostrophic deviations were almost instantaneous, accelerations were 12 hour average changes of velocity up to the period analyzed (later winds were not available). b) friction and vertical motion were neglected. However an attempt to improve the agreement by including the effect of these two factors failed, perhaps due to the inadequency of our knowledge of these quentities. In the case of situation B isentropic trajectories could be constructed and 12 hour accelerations could be computed centered at the period of analysis. The nongeostrophic winds based on these measurements, are entered as broken arrows in figure 12. The agreement with the observed nongeostrophic winds is poor.

The doubtful reality of the geostrophic deviation field in situation B (figure 12) can be demonstrated in another way. Apparently these vectors have a vorticity of ~~lake~~ magnitude curl $V_n \sim -10^{-5}$ sec^{-1}. Now, taking the divergence of (3):

$$\text{div}\ \frac{d\vec{V}}{dt} = \lambda\ \text{curl}\ V_n \qquad (4)$$

The left side of (4), the divergence of acceleration, is related to the acceleration of divergence. Assuming these two quantities the same, this would mean a decrease of divergence along the trajectory of about 10^{-5} sec^{-1} in 3 hours. A decrease of divergence, however, was not observed, in fact, along most of the trajectories though the area analyzed, the divergence increased.

From all these results we may conclude that the "observed" field of geostrophic deviations gives a fairly correct picture in situation A, but not in B. Even in A, the exact magnitude is not everywhere reliable, especially in the northeastern section where nongeostrophic speeds of the order of 30 m sec^{-1} appear.

Abstract and Conclusion

For the purpose of "analysis" of the pressure and wind field, polynomials have been fitted to the observations. This process is objective, eliminates the necessity of plotting the data, but takes a large amount of time. This is of relatively small importance if the technique is to be applied to forecasting with the aid of an electronic computer.

It is shown that measurements and computations made from subjectively analyzed wind and pressure fields can be made equally well from these polynomials. Isobars and stream lines evaluated from the polynomials agree well with those obtained subjectively.

The following conclusions can be reached concerning data at 10,000':

a) Pressure gradients can be measured with fair accuracy.

b) The Laplacian of the pressure field is very poorly determined by the observations

c) Deviations from geostrophic winds can be obtained from observations occasionally, but not with sufficient accuracy for use in the numerical computation process.

A Study of the General Circulation in March

Project Report 1947 - 48

Introduction

The term "General Circulation" is used to describe many kinds of mean motions in the atmosphere. At one extreme, it is applied to the general flow aloft on an individual map, at the other it refers to the mean motion over a period of many years, averaged also over all longitudes.

The definition of "general circulation" used here is close to the second extreme, denoting a long period mean as well as a mean over longitude. Thus, essentially, the study refers to mean motion as function of latitude and height only.

This meaning of "General Circulation" is thus similar to that given by Rossby in his "Scientific Basis of Meteorolcgy"; however, due to the wealth of data required in the type of analysis to be described here, this study will be limited to the "General Circulation" in a single month. March was chosen since the circulation in that month is similar to the annual circulation.

Rossby's largely <u>qualitative</u> theory postulated the existence of three cells; the present study was intended to utilize existing observation to arrive at a <u>quantitative</u> description of the circulation averaged over longitude and time.

It is clear at the outset that the averaging process will reduce magnitudes of the meridional and vertical components of the motion. However, these very small average motions still have a pronounced effect on the meridional heat and moisture transfer. It is proposed to compare the magnitudes of the heat and moisture transfer by these slow average motions with that produced by the large scale eddies superimposed on this mean motion. It is clear that if the "General Circulation" represents an average over both longitude and time, all pressure systems, both travelling and semistationary, must be regarded as eddies.

Ideally, the "General Circulation" and the mean pressure distribution should be inferred from the energy balance, the equation of continuity and the equations of motion. In that case the pressure, the only meteorological element which has been observed thoroughly over most of the earth and to high elevations would not be used directly. Here, the pressure will be assumed known, and it will be attempted to determine a field of motion consistent with the pressure field. It will be seen that even with the knowledge of pressure, the field of motion is subject to considerable uncertainty, due to our lack of knowledge of the effect of momentum exchange by eddies of all sizes in the free atmosphere.

Method of Integration

With the pressure given and the density inferred from the hydrostatic equation, the problem reduces to ~~determine~~ a determination of the three components of mean motion from the first two equations of motion and the equation of continuity. The independent variables are φ, latitude, and r, the distance from the earth's center. The dependent variables U, V and W are the mean zonal, meridional and vertical wind components. As used in this report, a mean expressed by a capital letter, is defined by

$$\overline{X} = \frac{\sum\limits_{\text{years}} \int_t \int_0^{2\pi} x \, d\lambda \, dt}{\text{No. of years} \times \text{No. of seconds in month} \times 2\pi} \qquad (1)$$

~~No. of years x No. of seconds in month x 2π~~

where ~~T~~ t is time, λ longitude. The symbol "integral over time" extends over the month studied. The number of years considered here is of the order of forty. In practice, the integrals have to be approximated by sums. Also in this manner, P denotes the mean pressure, Q the mean density. The first two equations of motion and the equation of continuity can now be written:

~~ρV = ρW(∂V/∂r + U/r) + ρ(∂Q/∂t − F) / (f − ∂U/r∂φ + U/r tan φ)~~

$$\rho V = \frac{\rho W\left(\frac{\partial U}{\partial r} + \frac{U}{r}\right) + \rho\left(\frac{\partial U}{\partial t} - F_\lambda\right)}{f - \frac{1}{r}\frac{\partial U}{\partial \phi} + \frac{U}{r}\tan\phi} \qquad (2)$$

$$U = - \frac{1}{fQr} \frac{\partial P}{\partial \varphi} \tag{3}$$

$$\frac{\partial (QW)}{\partial r} = - \frac{1}{r}\left[\frac{\partial (QV)}{\partial \varphi} - QV \tan \varphi\right] \tag{4}$$

where f is the Coriolis parameter and F_λ is the zonal frictional force, to be discussed later at some length. Equation (3) can be used to compute U as a function of φ and r; then given W at a low level, V is computed from (2), hence W at a higher level from (4), then V at that level from (2) etc.

The approximations involved in these equations have been discussed in detail earlier. Equation (3), which expresses geostrophic balance along the meridians, is applicable above the "Friction layer" only; it also becomes of questionable value in the tropics where both pressure gradient and Coriolis force become small and friction, even in the free atmosphere, is presumably of a similar order of magnitude. Thus the integration will be carried out above the friction layer and North of 20° latitude.

Three problems have to be discussed, before the actual integration is started;

1) the northern and southern boundary condition,
2) evaluation of W at H, the top of the friction layer,
3) estimate of F.

The Extreme Latitudes

Due to lack of data the calculations are terminated at 20° and 65°N. The quantity: $\frac{\partial \rho V}{\partial \varphi}$ at 20° and 65°N is evaluated with the aid of the assumption that there is no flow across the pole and the equator. This means that ρV is assumed to vary linearly, from 0° to 25° and 60° to 90° latitude.

The Lower Boundary

The determination of vertical velocity at the top of the friction layer is treated as a problem. From the equation of continuity it might be expected that the vertical velocity would be correlated with surface divergence. Therefore, divergence charts were constructed from resultant winds over long periods. Since the divergence is a linear function of the wind, the divergence of the mean wind is the mean divergence of the wind which can be measured from mean maps*. Initially, sufficient resultant mean wind data were available only over the oceans, the continental United States and sections of India. Measurements of divergence in these regions indicated that areas of maximum divergence exist 5° South and 10° East of High Pressure centers. Divergence was plotted against pressure, taken off 5° North and 10° West of the point where the divergence had been measured. This graph is shown as figure 1. The scatter is

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*In the appendix a theoretical study of mean divergence, carried out in connection with this project, is summarized.

very large, part of which is due to random errors in measurement of divergence, another part due to the questionable nature of a relationship between divergence and pressure. A straight line is drawn on Figure 1. It was drawn by eye so as to give the best relation between divergence and pressure. One condition for this line was that the divergence at 1015.0, the mean pressure of the Northern Hemisphere in March, should be zero.

The straight line in Figure 1 permits an estimate of surface divergence, averaged over all longitudes, from the mean pressures. The result of this estimate is given below:

latitude	20°	25°	30°	35°	40°	45°	50°	55°	60°	65°	70°	75°
divergence in 10^{-6} sec^{-1}	+1	1.5	1.5	1.2	.7	.2	-.2	-.4	0	+.3	+.5	+.6

The relation between vertical velocity at 5000' (which was taken as the top of the friction layer) and surface divergence could be studied only from United States data where vertical velocities were available. Figure 2 shows the relation of vertical velocity and ~~with~~ surface divergence.

The figure shows that a good estimate of vertical velocity could be obtained from: $W = 1.25 \times 10^5 \text{ div } V$. If this relation is used, the following table results:

φ	20	25	30	35	40	45	50	55	60	65	70	75
W	.08	-.12	-.12	-.10	-.06	-.01	+.01	+.03	0	-.03	-.04	-.05

Frictions

For convenience, the friction term F can be divided into two parts:

1) the effect of eddies so small that the eddies are smoothed in ordinary weather map analysis. This will be called: Austausch Friction.

2) the effect of the large scale eddies which appear on weather maps as cyclones or anticyclones, but act as eddies on the average circulation. This will be called: Gross Austausch Friction.

Austausch Friction

Austausch friction is usually separated into horizontal and vertical components:

$$F^A_\lambda = \frac{1}{\rho}\left[\mu_v \frac{\partial^2 U}{\partial r^2} + \frac{\mu_n}{r^2}\frac{\partial^2 U}{\partial \varphi^2}\right] \qquad (5)$$

where μ_v and μ_n are the vertical and horizontal coefficients of eddy viscosity, the horizontal coefficient being assumed the same in all directions. Little is known about the variation or even the magnitude of either coefficient, above the friction layer. If both coefficients are similar in magnitude, horizontal momentum transport by eddies can be neglected; however, this assumption does not agree with studies of isentropic diffusion.

Gross Austausch Friction

The force of friction due to large scale exchange of momentum (Gross Austausch) may be written:

$$F_{\lambda}^{G} = -\frac{1}{\rho}\left[\frac{\partial \overline{\rho u'v'}}{r\partial\varphi} + \frac{\partial \overline{\rho u'w'}}{\partial r} - \frac{\overline{\rho u'v'}}{r}\tan\varphi\right] \qquad (6)$$

here a' denotes deviation from the mean, ρ the individual density. The variation of density is neglected compared to the variation of the wind components.

The quantities $-\overline{\rho u'v'}$ and $-\overline{\rho u'w'}$ will be called "stresses" and denoted τ_{xy} and τ_{xz}, respectively.

Only τ_{xy} can be evaluated with any generality. τ_{xz} can be estimated if the velocity is a linear function of meridional velocity everywhere, as it seems to be in the United States. In that case, we can put

$$\tau_{xz} = K\,\tau_{xy}$$

where K may be a function of height.

Observations of vertical velocities in the United States lead to these values of K:

Height	0	3km	6km	10km	13km	16km
K	0	1.8×10^{-3}	2.5×10^{-3}	2.0×10^{-3}	1.2×10^{-3}	$.8\times10^{-3}$

Thus the problem is reduced to evaluation of $\tau_{xy} = -\overline{\rho u'v'}$.

The "Gross Austausch" stress can be broken up into two parts, the "Longitude Average" (LA) stress and the "Time Average" (TA) stress. The former is computed by combining the deviations of time mean wind components from the time-longitude mean; in order to find the TA stresses, deviations from time means at individual locations have to be combined and averaged over all longitudes. Thus the LA stresses can be obtained from mean maps, whereas the TA stresses have to be found from numerous individual charts. Essentially TA stresses measure the momentum transfer by travelling pressure systems, LA stresses by semistationary systems.

So far, LA stresses, based on geostrophic winds, have been computed from U.S. Weather Bureau normal charts at 0, 3, 6, 10, 13 and 16 km. The results are shown graphically in figure 3. Apparently, the order of magnitude of these stresses is 10 dyne cm^{-2}, a little larger than normal surface stresses.

The Time Average Stresses

Time average stresses would be relatively easy to compute if the patterns were consistent year after year. To test this, stresses were first computed at 10,000' in the U.S. for March 1939, 1945 and 1947. In each case, $\overline{u'v'}$ was computed from $\overline{u_{gs}v_{gs}}$ - $\bar{u}_{gs}\bar{v}_{gs}$ (subscript gs means geostrophic); the averages were computed from observations 24 hours apart. As figures 4, 5, 6 show, there are considerable differences in the stress patterns of these 3 years, perhaps because a single month is not a sufficiently long period*. Perhaps similar stress patterns are not to be expected year after year, because many properties of the flow change completely from a given month of a year to the same month of another year. Incidentally, stresses were computed for 04 GMT and 16 GMT independently. The differences between the stress fields at the two times are small and not sufficiently consistent to lead to any definite concept of diurnal variation of stresses.

Even if the stress patterns in given regions repeat themselves year after year the average over all longitudes would be uncertain, since sufficient data for computation of these stresses are not available in large parts of the Northern hemisphere especially above 3 km.

Only if the stress patterns are related to the patterns of a variable easily evaluated, is there any hope ~~to compute~~ of computing the mean over all longitudes. Comparison of the stress patterns in the United States with the pressure patterns showed a possible relation of sign between the stresses and the East-West pressure

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*According to V Starr's theory Journ. Meteor. 5, 39, 1948, τ should generally be negative. This does not seem to be born out.

gradient. Hence the North-South geostrophic wind, V_{gs}, averaged over all longitudes within the United States, was plotted against $-\overline{\rho u'v'}$, averaged over the same longitudes. The result is shown in figure 7. In all cases, negative stresses occur with Southerly mean flow and vice versa. If a linear relation between stress and geostrophic meridional flow could be assumed, the total TA stress averaged over all longitudes would vanish.

Stresses $-\overline{\rho u'v'}$ were also computed at 10,000' over the ocean for 1945. In this case, there was a correlation between stress and meridional flow just opposite to that observed over land. Hence, the question of relation of TA stress to other variables needs considerably more study.

Circulation Model Ia

In spite of all uncertainties it was decided to compute a General Circulation model based on equations 2 - 4.

Model Ia started from the vertical velocities at 1.5 km given above, proceeding from these in vertical steps of 1.5 km. All horizontal gradients were computed over 5° intervals. At 65° and 20°, however, $\frac{\partial(\rho V)}{r\partial\varphi}$ had to be computed from finite differences over 25° or 30° degrees, since the boundary conditions give ρV at 0° and 90°, respectively.

The friction term was simplified greatly. Of the Austausch terms, only $\frac{\mu_v}{\rho} \frac{\partial^2 U}{\partial z^2}$ was computed; μ was assumed 50 gm cm^{-1} sec^{-1} up to 10 km and zero above that level.

Of the Gross Austausch components of friction, only the first term of (6), $\frac{1}{\rho r} \frac{\partial \overline{\rho u'v'}}{\partial \varphi}$, was used, and of this only the LA component, which shows only the contribution due to the semi-stationary pressure centers. Admittedly, some of the other terms contributing to friction may be equally large. Additional terms will be introduced in later models, as their computation progresses.

Streamlines of model Ia are shown in figure 8 in a vertical cross section. Apparently the sign of the vertical velocity throughout the model is determined essentially by the sign of the vertical velocity at 1.5 km. In other words, the change of QW with height is small compared with QW itself. This shows the importance of having correct vertical velocities at 1.5 km. It also shows that at a given latitude, the vertical velocoty is not likely to change sign up to a very high level.

The absolute magnitude of the vertical velocities is of order 1 cm sec^{-1}, that of the meridional velocities, 3 cm sec^{-1}. The latter is large enough to carry northward a considerable fraction of the computed radiation excess at the equator and convey it to the pole.

A more detailed study of the heat transfer is planned for 1948-1949.

According to figure 8, momentum is carried downward over most of the latitudes between 20° and 65°N. However, from the equation of continuity we know that, if no air is to flow across the equator,

$$\int_{o}^{\pi/2} QW \cos \varphi \, d\varphi = 0$$

This makes extremely plausible the assumption, that in low latitudes momentum must be carried upward. It is unlikely that sufficient upward flow would occur North of 65°, partially because the cosine factor becomes small there. Also, synoptic experience in the equatorial regions makes upward flow there likely.

Thus, figure (1) shows two large clockwise cells, and two smaller counterclockwise "eddies" between them. It would be misleading to call the two large cells "direct", since they reach into the stratosphere, where the horizontal temperature gradient is opposed to that of the troposphere.

It was pointed out previously that the surface divergence is of the order 10^{-6} sec^{-1}. However, in model Ia, it drops a magnitude already at 1.5 km, and stays at about 10^{-7} sec^{-1} above that. On daily maps, on the other hand, the divergence is of the same order of magnitude at all levels. In order to arrive at a vertical velocity at 1.5 km, we assumed a relation between surface divergence and vertical velocity at this level. This relation was based on measurements on daily maps. However, since the divergence maps seems to decrease with height more rapidly on mean maps than on daily maps, the vertical velocity computed from daily maps may be systematically too large. For this reason, model Ib was constructed. The integration was repeated, after the vertical velocities at 1.5 km had been reduced by a factor 4. The result is given in figure 9. The change of the lower boundary condition apparently did not affect the general circulation pattern very much. Especially at high latitudes where the vertical velocities were small to begin with, the changes are small; at low latitudes, however, the streamlines are generally more horizontal than they had been before.

A rather distressing property may be noted in both models; according to considerable evidence, in middle latitude, northerly flow is generally accompanied by downward motion and vice versa. The correlation is good enough so that a similar effect might have been expected in the mean, in the midtroposphere. The figures do not bear out any such relationship

SUMMARY

The equations of mean motion were integrated numerically for March, with the pressure assumed known. Only the effect of vertical Austausch and the horizontal component of the Gross-austausch due to semi-stationary pressure systems were included. The result shows two large "direct" cells in tropical and north temperature regions and two small "indirect" eddies between them. The term "direct" being defined in accordance with the solenoids normally observed in the troposphere. The cells in question here, however, extend far into the stratosphere.

Appendix

Analysis of divergence over the North Atlantic

The values of mean divergence computed from wind resultant maps were analyzed theoretically.

If the vertical component of the curl of the mean equations of motion is computed, the following equation results:

$$\mathrm{div}_2 \vec{V}_2 = \frac{1}{f} \operatorname{curl} F_2' + \frac{1}{f} \operatorname{curl} F_2'' - \frac{U}{R} \operatorname{cotan} \varphi - \frac{1}{f} \operatorname{curl} \frac{dV_2}{dt}$$

where subscript 2 means two dimensional. F_2' is the friction force due to Austausch, F_2'' that due to Gross austausch.

The term curl F_2' has been discussed by Rossby and Montgomery and has been applied by the latter to the determination of convergence of ocean currents. It can be evaluated, although not altogether accurately, from mean maps.

The last two terms of the equation above also can be estimated from mean maps, but appear to be a magnitude smaller than either $\mathrm{div}_2\vec{V}_2$ or $\frac{1}{f}$ curl F_2'. The second term on the right, $\frac{1}{f}$ curl F_2'' can be evaluated only if a sufficient number of individual maps were analyzed. However, a sufficient number of North Atlantic charts were not available.

Charts of $\frac{1}{f}$ curl F_2' were compared with charts of $\mathrm{div}_2 V_2$. The agreement was quite poor in some regions, for example of

Newfoundland. Such disagreements can be due either to errors of observation or to $\frac{1}{f}$ curl F_2'' which was not computed. As a matter of fact, the charts of $\frac{1}{f}$ curl F_2' agreed more with the mean pattern of divergence expected by synoptic Meteorologists than did the pattern of $div_2 V_2$ itself. It is thus quite possible that the principal reason for the discrepancy is the large experimental error of $div_2 V_2$.