

JULE GREGORY CHARNEY
MC 184 BOX 14, F. 446

PETTERSSSEN, SVERRE, 1949-1963

THE UNIVERSITY OF CHICAGO
CHICAGO 37 • ILLINOIS
DEPARTMENT OF THE GEOPHYSICAL SCIENCES

February 11, 1963

Dear Jule:

To make a long story short, I'm enclosing herewith a copy of my letter to Mr. Ratcliffe from which you will see the scope of our conferences on May 1st and 2nd. On May 1st (under John Simpson's part of the program) there will be addresses by Urey, Lederberg, and Sir Bernard Lovell, and there will be table speeches by Wiesner and Seitz (or equivalent).

On May 2nd, I plan two high-level sessions on atmospheric sciences. The first will deal with ionospheric research (see enclosed letter) and the second one should deal with some aspects in which meteorological satellites are involved. I wonder if you would be good enough to give a major address (which we could publish if you like) on a field such as prediction, the dynamics of macro-states, or any such title of your own choosing. The address should involve the grand aspects of the Atmospheric Surveillance Program. After this we could have 2 or 3 shorter addresses, which deal more specifically with the Surveillance Program. Lally might be one of the speakers.

May 3rd is reserved for biology in relation to space, and I believe Dean Bennett is arranging for a program on which Lederberg, Calvin, Pittendrigh and Urey will appear. A number of other dignitaries will be involved.

I sincerely hope that you will accept the invitation, provide the lead to this important session, and advise me on other speakers who you would like to contribute to your session. I plan to have a well attended luncheon on May 2nd and I shall be looking for a speaker. One possibility is Waterman; have you any suggestions?

You are, of course, cordially invited to also attend the space program, May 1st, and the biology program on May 3rd. We will pay your travel expenses and I hope to produce money for an honorarium. I shall be grateful for an early reply for I do not wish to make further arrangements until I know your wishes.

Sincerely,

hure

Sverre Petterssen

*Why don't you
Both come.*

THE UNIVERSITY OF CHICAGO

CHICAGO 37 • ILLINOIS

DEPARTMENT OF THE GEOPHYSICAL SCIENCES

January 30, 1963

*Ask Platzman
if this is worth
while*

Dear Mr. Ratcliffe,

As the scientific focal point of a "Space Month" that is to be held in Chicago this coming April-May, a two-day conference of upper-atmospheric and space scientists is now being planned under the sponsorship of the National Aeronautics and Space Administration, the University of Chicago, and other interested universities in this area. It is our intention to keep the scientific conferences on the very highest level, with speakers chosen from amongst outstanding leaders in the various fields. It is our hope that the speakers will prefer to contribute with forward-looking address-type presentations, rather than with detailed papers.

It has fallen to me to make arrangements for the atmospheric sciences conference, and it is my great pleasure to inform you that I have been authorized to invite you to contribute to the atmospheric sciences program with an address on a subject of your own choosing. If it were possible for you to accept this invitation, I would welcome your suggestions for two or three other speakers, who could contribute to the same session, with shorter contributions on subjects related to your field. The session that I have in mind could be held either in the morning or afternoon of May 2nd. On the assumption that your address will deal with ionospheric studies, I would try to arrange the other session on May 2nd on some other area of high-atmosphere research. Although our plans are not yet firm, I hope to arrange for a third session, in the morning of May 3rd, on the atmospheres of other planets.

Our plans for other science conferences include a program on May 1, dealing with solar physics, interplanetary studies, chemistry, etc. Furthermore, the atmospheric sciences conference will be followed on May 3rd by a conference on biology in relation to space research. It is our hope that you would be interested in attending all three conferences and we should, of course, be very pleased if you felt inclined to contribute to the discussions.

I realize that it might be difficult for you to spare time for a visit to the United States, but I wish to assure you that your participation in the program would greatly strengthen the high-atmospheric research that is being conducted in this and other universities.

Mr. J. A. Ratcliffe

-2-

January 30, 1963

I should add that arrangements have been made to cover your traveling and living expenses in connection with the conferences, and an honorarium will also be provided. I sincerely hope that we shall have the pleasure of making your personal acquaintance and hearing you at the Conferences. If there is any other place in the United States which you would like to visit please let me know; I'm sure that a number of research centers here would like to take advantage of your presence in this country.

I shall be looking forward to hearing from you as soon as convenient. It will be particularly useful to know the title of your address so that we may proceed with other arrangements for the conferences.

Yours sincerely,



Sverre Petterssen

Mr. J. A. Ratcliffe, F.R.S.,
Director, D.S.I.R. Radio
Research Station,
Ditton Park,
Slough, Bucks,
England

July 19, 1962

Dr. S. Petterssen
Dept. of Geophysical Sciences
University of Chicago
5727 S. University
Chicago 37, Illinois

Dear Dr. Petterssen:

Prof. Charney is attending a Scientific Oceanographic Cruise in the Indian Ocean for the month of August so he will not be able to attend the CIPASH meeting in Washington, D.C. August 3-4, 1962.

Sincerely,

Berit K. Larsen
Secretary to Prof. Charney

b1

November 29, 1961

Professor Sverre Petterssen
Department of the Geophysical Sciences
University of Chicago
Chicago 37, Illinois

Dear Sverre:

I have finally decided that I must decline your very interesting and challenging offer to join the department at Chicago. This is the first time I have received an offer from an institution where the scientific milieu is as meaningful and exciting to me as is that where I now work; the decision was therefore not one to be made quickly.

Thank you again for your consideration and patience.

Sincerely yours,

Norman A. Phillips

17 August 1961

Dr. Sverre Petterssen
Department of the Geophysical Sciences
The University of Chicago
Chicago 37, Illinois

Dear Sverre:

Thank you very much for your invitation to participate in a panel discussion on National Goals in Meteorology at the forthcoming Annual Meeting of the Meteorological Society. The list of participants you have chosen, I am sure, ensures a lively and worthwhile discussion. However, I must personally beg off because I have already promised to deliver a long address on January 26 to the Joint Session with the Fluid Dynamics Division of the American Physical Society. This is to be a survey lecture requiring much time in preparation and thus effectively preventing participation in other programs.

Sincerely yours,

Jule G. Charney

THE UNIVERSITY OF CHICAGO
CHICAGO 37 • ILLINOIS
DEPARTMENT OF THE GEOPHYSICAL SCIENCES

August 8, 1961

Dr. Jule Charney
Department of Meteorology
Massachusetts Institute of Technology
Cambridge, Massachusetts

Dear Dr. Charney: *Jule*

Dr. Spar, who is Program Chairman for the forthcoming Annual Meeting of the American Meteorological Society in New York, January 1962, has asked me to serve as chairman of a session on Our National Meteorological Research Policy. Wisely, or unwisely, I have yielded to his request and I have been left free to decide whether the session should consist in presentation of formal papers or panel discussions, or both. Also, I have been asked to invite speakers.

The purpose of this letter is to ask you to help by contributing to the session which I am sure will stir up much interest.

Since I have recently served on the Task Force that prepared a 10-year plan for atmospheric research, Dr. Spar thought that I could be most useful by presenting at the session a summary of the national goals that resulted from the planning. I expect that I can do this in about 45 minutes. Thereafter, there should be time for one or two short papers of central interest. After the papers have been presented there should be at least one hour for a lively panel discussion, and those who prepare short papers might wish to use them in the discussion rather than to present them in advance. Toward the end of the session I plan to arrange for about 30 minutes for questions from the floor and about 15 minutes for summarization.

I shall be very grateful if you would let me know at your early convenience whether I can reckon on your help to make this session a success. In addition to yourself, I have asked Dr. H. G. Booker, of Cornell University, Hans Panofsky, of Penn State University; Harry Wexler, of the U. S. Weather Bureau; E. Wendell Hewson, of the University of Michigan; and John A. Simpson, of the University of Chicago to participate. I shall be grateful if you would let me have suggestions for additional members of the team.

More specifically, would you kindly let me know whether you will be able to join the panel, and if the answer is "yes" (which I sincerely hope it will be), please let me know whether you wish to present a formal paper or just prepare for the discussion. Finally, please let me have your suggestions for other participants.

With kindest regards,

Yours sincerely,

Sverre
Sverre Petterssen

SP:mb

U. S. NAVAL POSTGRADUATE SCHOOL
MONTEREY, CALIFORNIA

IN REPLY REFER TO:

13 August 1959

Dr. Norman Phillips
Massachusetts Institute of Technology
Department of Meteorology
Cambridge 39, Massachusetts

Dear Norman:

Thank you for your letter. For some reason, your report was never forwarded to me so I would appreciate a copy sent to my present address.

I was reading through your paper on "A Map Projection" and wish to ask you if there is not a sign error in the first equation (21). I worked through the transformation and arrived at a plus sign for the second term in the square brackets.

The reason why I have been working through this is that I am in the process of introducing the coordinates of the Polar Stereographic Projection into the equations we have to work with.

Wish you a good vacation in Maine.

Snorre

SNORRE

*copy sent to Monterey
8/19/59*

very

Dear Snorre:

Here is the report. Sorry you never got it.

Eqn (21) in "A Map . . ." is correct (Note that the kinetic energy equation is not correct if your + sign is introduced.)

Had a truly delightful week at Appel-Rauw. Plan on 2 weeks next year if we are here.

Terry

17 February 1958

Professor Sverre Petterssen
Department of Meteorology
University of Chicago
Chicago 37, Illinois

Dear Sverre:

I have discussed with Charney and Phillips the question of the latter's participation in the joint meeting in Bergen next July. Phillips is very much interested and Charney and I see no reason why we cannot finance his trip out of contract funds. If you and the A.M.S. committee for the meeting are favorable to Phillip's participation I see no reason why it cannot be considered settled. Phillips will of course expect to receive some kind of an invitation and necessary details from you or the appropriate person. For my part I would be very pleased to have Phillips as our representative.

With best regards,

Sincerely yours,

Henry G. Houghton

HGH/jms

THE UNIVERSITY OF CHICAGO
CHICAGO 37 • ILLINOIS
DEPARTMENT OF METEOROLOGY

June 4, 1956

Dr. Jule Charney
The Institute for Advanced Study
Electronic Computer Project
Princeton, New Jersey

Dear Jule:

Under separate cover, I am sending reprints of the
material you requested in your letter of May 29. Some other *(four)*
storms were studied but the reports on them are not available.

Sincerely,



Sverre Petterssen

SP:bw

November 2, 1955

Dear Sverre:

Please forgive my delay in returning your typescript. I received it just as our study group on the general circulation convened at the Institute and consequently had no time to read it until this week. Having read it, I have very little to suggest. It is an excellent expository job. The following are a few trivial comments:

Page	Line	Comment
3.2.1	5	One might add that the "noise" consists of sound and inertio-gravitational oscillations in general, and only the latter if the hydrostatic approximation is made.
3.3.1	8	The various terms you name are neglected for reasons of consistency. Thus the neglection is justified by the same scale reasoning that says .
3.4.2	15	I now think the mean level of non-divergence is between 500 and 400 mb. (See Charney, Fjortoft, Von Neumann, 1950).
3.6.7	24	It is usually possible to locate fronts in the numerical forecast.
3.8.2	6	I have now made barotropic forecasts with both the "primitive" and the "balance" equations. The results are a definite improvement on the quasi-geostrophic method.

With best regards,

Sincerely,

Jule Charney

THE UNIVERSITY OF CHICAGO
CHICAGO 37 • ILLINOIS
DEPARTMENT OF METEOROLOGY

October 20, 1955

Dr. J. G. Charney
Electronic Computer Project
Institute for Advanced Study
Princeton, N. J.

Dear Jule:

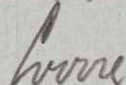
I am availing myself of your kind offer to read through the chapter on numerical forecasting which I am writing for the forthcoming supplement to the Compendium. I am enclosing herewith Chapter 3 on which I would be very glad to have your comments. The abrupt start is due to the circumstance that the problems have been touched upon to some extent in Chapters 1 and 2. I am therefore enclosing carbon copies of these two chapters so that you may see the context.

As I told you when I saw you in Washington last week, Miller is pestering me for the manuscript and I feel that I have to send him something within a few days. I shall be very grateful therefore if you would let me have your comments as soon as at all possible.

The remainder of my contribution consists of a short account of Fjörtoft's graphical integrations, a very brief account of results obtained by computations, using Rossby's formula, and similar limited approaches. There will be a short section on time series analysis (Malone's work) and also on parameter techniques (what is commonly called objective forecasting). In addition, I have to write something about analysis and observation. This, however, will be very brief except for automatic analysis. I don't see that there has been much progress in conventional analysis since the Compendium was published.

To conclude, let me thank you for your offer to read Chapter 3. The more comments you have, the more pleased I shall be.

Sincerely yours,



Sverre Petterssen

SP:bw
Encl.

THE UNIVERSITY OF CHICAGO
CHICAGO 37 • ILLINOIS
DEPARTMENT OF METEOROLOGY

February 24, 1955

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

Dear Jule:

I understand that Arnt will get in touch with you as soon as he arrives in New York. I have written to him today c/o you and my letter contains some reinforcement which so many foreigners need when they arrive. I expect that he will phone you from New York and I shall be very grateful if you will tell him that my letter is waiting for him.

With kindest regards.

Yours sincerely,



Sverre Petterssen

SP:bw

THE INSTITUTE FOR ADVANCED STUDY
ELECTRONIC COMPUTER PROJECT
PRINCETON, NEW JERSEY

February 21, 1955

Dr. Sverre Petterssen
c/o Joint Numerical Weather
Prediction Unit
U. S. Weather Bureau
Washington 25, D. C.

remitt

Dear Sverre:

Please forgive me for this belated reply to your letter of January 25.

You are certainly welcome to use any of our numerical predictions as illustrations for the section of your book on numerical weather prediction. In reading over your draft of the section containing the results I was a bit puzzled by the dates. I know that we made predictions for the November 24-26, 1950 storm, but I have forgotten whether we made predictions for the November 25-26, 1952 storm. Either one, however, would be quite suitable as illustrations.

I would be glad to have you pay us a visit during your stay in Suitland. We are expecting Arnt sometime during March, but I do not know exactly when he will be here. Perhaps the best thing would be to drop you a note when he arrives.

With best regards,

Yours sincerely,

Jule Charney

JC/cb

THE UNIVERSITY OF CHICAGO
CHICAGO 37 • ILLINOIS
DEPARTMENT OF METEOROLOGY

January 25, 1955

Dr. Jule Charney
Electronic Computer Project
The Institute for Advanced Study
Princeton, N. J.

Dear Jule:

I believe I told you sometime ago that Arnt undertook to write a chapter on numerical forecasting for the new edition of my book. He has done this extraordinarily well and the chapter makes very pleasant reading. The last section was concerned with the results of numerical integrations and I thought that the material he had been able to dig out from the literature was not quite representative. Furthermore, since the theories and techniques are in a state of rapid development, I was afraid that whatever results we quote now will be out of date probably before the book is printed. I suggested to Arnt that I write the last section and use the computations which you and Gilchrist made concerning the November 1952 storm. I am enclosing a draft of this section and asking your permission to reproduce the values given in the table. Considering that this was a storm with an almost explosive development, I think the results are appealing and might be representative of what we have good reason to expect for the future. I realize that the second storm which was computed gave less satisfactory results. But this storm has turned out to be a monstrosity in every respect. It was one of very small dimensions and it is our opinion that the orography played an important part. Dr. Estoque finds that the turning of the horizontal vorticity vector into the vertical contributed very much to the development as the storm slid down the slope of the Rockies. I am not prepared to use this storm as a typical case.

I shall be at Suitland for four weeks beginning February 21st. Our storm investigation program is now more or less completed and I would very much like to spend a day or two at Princeton to show you the results. Perhaps I could come when Arnt arrives.

With kindest regards.

Yours sincerely,



Sverre Petterssen

SP:bw

18.8. Results. - A number of numerical forecasts have been made with a view to testing the various models,¹⁾ and from these

1) See references 7-10.

tests it is possible to form an opinion of how such forecasts will work out in practice. It should be noted, however, that experiments with more refined models are continuing, and that improvements are likely to result.

Most thoroughly tested is the barotropic (i. e., the one-level) model. In general, it is found the correlation coefficients between the observed and the computed 24-hour height changes at the 500 mb level vary about 0.75, showing that about 55 per cent of the variations are accounted for. In effect, this means that the simple one-level model is clearly recognizable as a first approximation. The accuracy of this model has been found to vary considerably with the synoptic situation. The best results are obtained when the height changes are mainly due to movement of pressure systems, while poorer results are found when appreciable intensifications are involved.

Some interesting examples of predictions based upon the two-level model have been described by Charney and Phillips [7], and a few attempts have been made to predict major storm developments

by the aid of the three-level model. One of these storms was discussed in detail in Section 17.1. Fig. 18.8.1 shows, as an example, two successive forecasts for the 400 mb level. Of these, the one on the right covers the period of very rapid development. It will be seen that the salient features of the development are well indicated.

In Table 18.8.1 are reproduced the correlation coefficients of all numerical forecasts pertaining to the aforementioned storm. By comparison with Fig. 17.1.1 and 17.1.2, it will be seen that the correlations increased noticeably as the storm moved away from the Rocky Mountains. Since the computations were performed on the assumption that $\omega = 0$ at 1000 mb (see Section 18.5), one would expect more accurate results after the storm had moved well away from the mountain barrier.

The tests of the numerical prediction methods have shown that the quality of the 24-hour prognoses are comparable with that of forecasts produced by conventional methods which, to a large extent, are based upon experience. The advantage of the numerical forecasts is that they are entirely objective and provide the forecaster with an approximation to which he may add improvements derived either from experience of past cases or from such extrapolations as were described in Chapter 10.

It should be noted that the theories and techniques of numer-

Table 18.8.1. - Correlation between Observed and Computed
24-Hour Height Changes by the Aid of the
Three-Level Model.¹⁾

Forecast for (GCT)	Level (mb)			Remarks
	900	700	400	
Nov. 25. 03	0.81	0.75	0.87	Forward march of upper trough; little change at sea level.
Nov. 25. 15	0.85	0.81	0.85	Rapid development set in shortly after 03 GCT.
Nov. 26. 03	0.84	0.77	0.87	Maximum intensity reached shortly after 03 GCT.
Nov. 26. 15	0.94	0.92	0.94	Rapid development followed by decay.

1) Computations were made by Drs. J. Charney and B. Gilchrist,
Institute for Advanced Study, Princeton, N. J.

18.8.4

ical prediction are in a state of rapid development, and it is to
be expected that the performance will improve.

February 5, 1954

Professor Sverre Petterssen
The University of Chicago
Department of Meteorology
Chicago 37, Illinois

Dear Sverre:

Thank you very much for your letter of January 15. I was glad to hear that you will be able to come to Princeton after your Washington meeting on February 13. This will enable us to conclude arrangements for the use of your analyses.

It would appear that you have chosen some situations in the central part of the United States. We have in the past avoided such situations because of the complicating factor of topography, but we have now put in topography in two of our models and should therefore be able to handle them.

With regard to the integration of your equations in Tellus Vol. 5, No. 3, we can integrate (15) straight off and compare with (11). We can also do (18) for a two-level model in which D is evaluated from the energy equation essentially. Thus

$$D = - \frac{\partial \omega}{\partial p}$$

$$\omega = - \frac{1}{\sigma \frac{\partial \theta}{\partial p}} \left(\frac{\partial}{\partial t} + \mathbf{v} \cdot \nabla \right) \frac{\partial \theta}{\partial p}$$

With best regards,

Sincerely,

Jule Charney

JC/cb

THE UNIVERSITY OF CHICAGO

CHICAGO 37 • ILLINOIS

DEPARTMENT OF METEOROLOGY

January 25, 1954

Dr. Jule Charney
The Institute for Advanced Study
Electronic Computer Project
Princeton, New Jersey

Dear Jule:

The following will supplement my letter of January 11th.

We are at present working on four major storms and we have two additional cases which we intend to examine. The purpose of this investigation is to try to discover how to forecast the sudden development which is so typical of cyclogenesis.

The storm that I have been working on is the Thanksgiving Storm 1952. I have analyzed the 1000, 850, 700, 500 and 300 mb. surfaces at 12-hour intervals. The period covered is November 23rd through November 27th. In addition to the pressure contour charts, I have isentropic analyses for the 303 potential temperatures. The area covered is broadly from 25 to 75° N. from western Alaska to central Greenland in the north and from 60° to 140° W. in the south. We can add area if you so desire.

The analyses of the three remaining storms will be finished within two or three weeks. All storms are within the United States and the area is about the same as indicated above.

In our analyses we have introduced fronts and the kinks in the contours tend to mess up the vorticity when a standard grid is used. I suppose that you will wish us to eliminate the fronts by smoothing the contour field.

We have now computed the geostrophic vorticity for the November Storm 1952 using a grid of 400 km. The other storms will be computed in the same fashion.

In addition to the storms mentioned above, the US Weather Bureau has asked us to look into the storm that occurred during the first week of November last year. Most of the charts for this storm have been plotted but it will be some time before we get down to the analyses.

With reference to the second paragraph of your letter, the only type of computation that would be of interest to my group at the present time is to try to predict the pressure field by the aid of Equation 18 in my paper in the last issue of Tellus, and to compare this

Dr. Jule Charney, Jan. 25, 1954 - p. 2

prediction with what comes out of the equation if the geostrophic vorticity is used. We have hand-computed one case without the divergence term but the results were inconclusive. It is obvious, however, that there cannot be much difference unless the divergence term is included. For the time being I think it will be enough to run off both computations on the Thanksgiving Storm 1952. If the one prediction equation is better than the other it certainly should show up in connection with this storm. At the time of maximum development I find divergence between 2 and 3×10^{-5} .

I shall be in Washington on February 13th to attend a meeting in the National Science Foundation. If it is convenient to you I might come to Princeton for a day or two and we could then make firm plans for collaboration.

With kindest regards,

Yours sincerely,



Sverre Petterssen

SP/srl

January 6, 1954

Professor Sverre Petterssen
Department of Meteorology
University of Chicago
Chicago 37, Illinois

Dear Sverre:

Gilchrist told me that your group is preparing a series of three-dimensional analyses of cyclogenetical situations and that you would be willing to let us use these analyses in our numerical studies. If you would, it would be a godsend to us. We now have no facilities for producing analyses of our own, and it is obviously too sanguine to rely on the first results from our objective analysis studies. Cressman is here and we plan together to conduct a combined numerical-synoptic investigation of special systems -- cyclones, anti-cyclones, jets, etc. I feel that this work would also tie in well with your own investigations.

To get things started, may we ask you to send us a description of the situations you are analysing, the levels analyzed, and the extent of the area covered. From this information we could tell you just what grid values we would like read off. If there is any special type of numerical treatment of the data you, yourself, would like to have, we shall certainly be glad to cooperate. To insure proper coordination it may be necessary for one of our group to visit you, or vice versa.

We now have models that are capable of dealing with the data at one, two, three, or five levels.

Please forgive me for not replying sooner to your circular letter of October 12. I had a draft on my desk and have just discovered it buried under a pile of papers. The enclosed copy of a letter I have just sent off to the secretary of the Royal Meteorological Society will, I think, clear the record.

Hoping to hear from you soon, and meanwhile wishing you and Grace a Happy New Year!

Sincerely yours,

Jule Charney

JC/cb

Enclosure (letter to Secretary (Sec. R.M.S.))

THE UNIVERSITY OF CHICAGO
CHICAGO 37 • ILLINOIS
DEPARTMENT OF METEOROLOGY

January 11, 1954

Dr. Jule Charney
The Institute for Advanced Study
Electronic Computer Project
Princeton, New Jersey

Dear Jule:

Thank you very much for your letter of January 6th. I got very many nice presents for Christmas but this is the best of them all. We have selected six major storms for detailed investigation and our primary purpose is to find out something about the mechanism of their genesis. We are of course also trying to find all we can about their later behavior and how to forecast. The prototype storm is the Thanksgiving Storm 1952. The analysis is now complete and so are the vorticity computations. We are now trying to evaluate the divergence field. Prior to the cyclogenesis the level of non-divergence was well above the 300 mb. surface. As the 300 mb. trough (with strong divergence ahead of it) approached the frontal zone the level of non-divergence seemed to shift suddenly to somewhere near the 600 mb. surface and most of the high-level divergence obtained compensation in convergence at low levels. The intensification of the storm was unusually rapid. Since my return from Toronto I have watched closely every major cyclogenesis that has taken place in and near the United States and it appears that every one of them, so far, has followed the pattern set by the Thanksgiving Storm, although of course there are some differences in degrees. I have built some sort of a hypothesis around what I have seen and, naturally, I am most anxious to arrange for some sort of an objective test. It has therefore been my hope that your group would be interested in using our analysis in testing your models so that we can compare our conclusions with something which is objective.

The Thanksgiving Storm has been analyzed at the conventional levels (1000, 850, 700, 500 and 300 mb.). We have also analyzed one isentropic surface. If need be, we can provide additional analyses.

I am about to leave for a meeting in Seattle and will return early next week. Not knowing precisely what data you would need from us, I think it would be excellent if one of your group could spend a few days here so that we can sort out the problems of mutual interest. If this cannot be done I shall be quite happy to take all the charts and data to Princeton although this would interrupt some of the work that is going on here.

*area
roughly
from 140°W
to 60°E
25° - 70°N*

Dr. Jule Charney, 11 January 1954 - p. 2.

I am not certain that I have answered your letter adequately, but on account of my trip to the west coast the remainder will have to be left until I return.

With kindest regards,

Yours sincerely,



Sverre Petterssen

SP/srl

P.S. I had hoped to be able to invite Ragnar here for a week or so but my contract will be over-expended for some months to come. I shall try to find some money somewhere and I will write about this too when I return.

January 6, 1954

Mr. M. E. Shoenberg
Assistant Secretary
Royal Meteorological Society
49 Cromwell Road
London, S. W. 7, England

Dear Mr. Shoenberg:

When I spoke at the Toronto Meteorological Conference I was not aware that the papers were to be published in the Quarterly Journal. I had then intended to submit the paper to the Proceedings of the National Academy of Science of the United States and have now done so. The article will appear this month. Since the Proceedings of the National Academy reach an essentially non-meteorological audience I would have no objection to having the paper published in the Quarterly Journal as well. If it is not contrary to your policy to publish articles that have already appeared in print, and I understand that several of the other papers presented at the Conference were of this character, I could submit my manuscript immediately upon receiving notification from you.

Yours sincerely,

Jule Charney

JC/cb

CC: Dr. S. Petterssen

ROYAL METEOROLOGICAL SOCIETY
49 Cromwell Road
London, S.W.7.

MES/AD/TOR

24 December 1953

Dr. J. Charney,
The Institute for Advanced Study,
Princeton,
New Jersey,
U.S.A.

Dear Sir,

On the Prediction of Cyclogenesis

It is hoped to publish your paper in the Proceedings of the Toronto Meteorological Conference 1953 and as the material is already well in hand for printing, the Editor has asked me to draw your attention to the fact that the MS of your paper has not yet been received.

I have been asked to say that if the MS is not received here by, say, 15 January, it may not be possible to include it for publication.

It is of course possible that the paper has already been sent to the chairman of the symposium, and has not been registered here yet. I should be grateful if you would advise me whether we can expect the MS, so that our records may be marked accordingly.

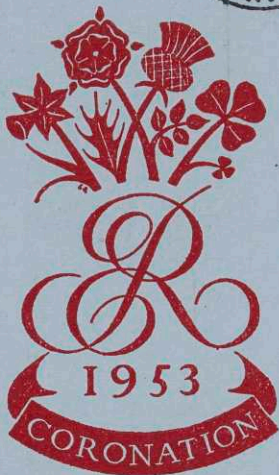
Yours faithfully

M. E. Shoenberg

M. E. Shoenberg
Assistant Secretary.

to open cut here
↓

BY AIR MAIL
PAR AVION
AIR LETTER
AEROGRAMME
SOUTHAMPTON
30 DEC
1953
S.W.7.



Dr. J. Charney,
The Institute for Advanced
Study,
Princeton,
New Jersey,
U.S.A.

First fold here

Second fold here

Sender's name and address: Royal Meteorological Society
49 Cromwell Road,
London, S.W.7.

IF ANYTHING IS ENCLOSED THIS LETTER
MAY BE SENT BY ORDINARY MAIL

THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF METEOROLOGY

October 12, 1953

Prof. J. Bjerknes
Prof. Jerome Spar
Prof. Herbert Riehl
Dr. Jules Charney
Mr. Jerome Namias

Gentlemen:

On the request of Dr. Andrew Thompson, Chairman of the Canadian Planning Committee, I am enclosing herewith a pamphlet on preparation of papers for the Quarterly Journal, which might be of interest to you in preparing the manuscript of the paper that you presented at the symposium on Modern Cyclone Theory.

I would like to take this opportunity to thank you for your most valuable contribution to the symposium.

Yours sincerely,

S.P

Sverre Petterssen
Chairman of the Symposium
on Modern Cyclone Theory

SP/srl

May 14, 1953

Dr. Sverre Petterssen
Department of Meteorology
University of Chicago
Chicago 37, Illinois

Dear Dr. Petterssen:

I am sending herewith the title and abstract of my paper to be presented at the Symposium on Modern Cyclone Theory at the Joint Meeting with the Royal Meteorological Society.

Thank you very much for your letter of May 8 concerning the activities of the Committee on Encouragement of Research of the AMS and the National Science Foundation Earth Sciences Program. The items which you have listed for consideration by the Panel of Consultants for the Earth Sciences seems to me to be equally germane to the activities of the Committee and I hope to be able to comment on them in another letter.

I am grateful that you will remain in close touch with our Committee and I shall rely heavily upon your advise and counsel.

With best regards,

Jule Charney

JC/cb

Enclosure

ON THE PREDICTION OF CYCLOGENESIS

(Abstract)

The results of a series of numerical integrations of the quasi-geostrophic equations of motion for a cyclogenetical situation are discussed in relation to existing theories of cyclogenesis.

THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF METEOROLOGY

May 8, 1953

Dr. Jule Charney
Electronic Computer Project
Institute of Advanced Studies
Princeton, N. J.

Dear Jule:

Thank you for your letter of April 30th. I was very glad to hear that you have been elected Chairman of the Committee on Encouragement of Research.

There is no business pending before the outgoing committee, so you have the advantage of starting with a clear slate.

In the past the Committee has considered the following major cases:

1. A report to the Council recommending general measures to stimulate meteorological research, and
2. A recommendation to the Council in regard to the use of donations from the Munitalp Foundation.

It is not clear to me precisely what action the Council took in these matters. If you are interested in following-up these past events, I am sure the Executive Secretary can give you all information.

As chairman of this Committee, I have found it difficult to do much about actual stimulation of research. In the first place, it has been impossible to get the Committee together more than once, and to get things done by correspondence has proved quite cumbersome. Secondly, there does not seem to be much one can do in regard to the activities in the actual research centers. Thirdly, most research are sponsored by government agencies and these act on their individual requirements rather than on the basis of a long-term national policy.

As you may know the National Science Foundation has recently activated their effort in Earth Sciences, and this may be an opportunity for your Committee to assist in developing sound and progressive policies. I have recently accepted Dr. Waterman's invitation to act as meteorological consultant to the Foundation. The Panel of Consultants for Earth Sciences met yesterday for the first time, and we shall meet again on November 6th.

At our next meeting we shall, inter alia, consider the following items:

- a. Determination of areas where research support is needed.
- b. Symposia and Conferences.
- c. Summertime research programs.
- d. Identification and training of promising young research Scientists.
- e. The fellowship program.

As consultant to the Foundation it will be my ambition to do all I can for the advancement of meteorological research and to remain in close touch with your Committee. Needless to say that I shall be very grateful for any advice and suggestion that you and your Committee may wish to offer. In particular, it would be very helpful if your Committee (without reference to the Foundation's program) could come out with independent recommendations concerning the above-mentioned points.

Although I have not given much thought to these items yet, my present thinking is along the following lines:

The all-important problem is to attract new talent to the meteorological profession, and to place emphasis on certain basic fields which are likely to enhance basic understanding of atmospheric processes, and, at the same time will serve to attract new talent.

As to "a": Amongst such areas I could think of the following: (i) Large-scale atmospheric motion (including numerical forecasting) and energy transformations; (ii) atmospheric physics (including cloud physics); (iii) micrometeorology; and (iv) theoretical climatology.

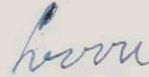
As to "b": We should certainly support the idea that specialized symposia and conferences are very desirable.

As to "c": I understand that these programs are intended for Junior Scientists (have you anything to suggest?).

As to "d" and "e": These items are important. What we must strive for is some plan which is suitable for meteorology. If we stay on the general level and come under some general category (such as: physical sciences, or earth sciences) we may be severely handicapped.

I shall be very grateful if your Committee could find an opportunity to consider the points that I have outlined above. And, should it not be possible for your Committee to formulate recommendations at an early date, I shall be very grateful to have your views.

With kind regards,
Yours sincerely,



SVERRE PETERSSSEN

Copies for: Prof. H. R. Byers
Mr. K. Spengler

March 27, 1953

Dr. Sverre Petterssen
Department of Meteorology
University of Chicago
Chicago 37, Illinois

Dear Dr. Petterssen:

First, I should like to thank you again for participating in the Symposium and Panel Discussion on Numerical Forecasting at Atlantic City. Your presence helped greatly in making the meeting a success.

In return it would not be becoming to refuse your kind invitation to join the symposium on MODERN CYCLONE THEORY in Toronto. I am certainly disposed to accept, for I am interested in the cyclone problem and, in fact, am working on one phase of this problem now. But the difficulty is that I cannot as yet tell whether anything worth-while will come of it. I should hate to commit myself and then have nothing to talk about. Would it be possible to postpone my decision until later this spring? I should know by then whether I will have anything to say.

With best regards,

Sincerely,

JC/cb

THE UNIVERSITY OF CHICAGO
CHICAGO 37 • ILLINOIS
DEPARTMENT OF METEOROLOGY

February 12, 1953

Dr. J. Charney
Electronic Computer Project
Institute of Advanced Studies
Princeton, N. J.

Dear Charney:

The President of the American Meteorological Society has asked me to serve as chairman of a Symposium on MODERN CYCLONE THEORY to be held as part of the program of the Joint Meeting of the Royal Meteorological Society and the American Meteorological Society in Toronto, Canada. It is probable that this symposium will be scheduled on Friday, September 11, 1953.


It has been explained that my duty as chairman includes the organization of the program and procurement of contributions to the symposium, and the purpose of this letter is to approach leading authorities on the subject with invitation to contribute to the symposium.

As I understand it, tropical as well as extratropical cyclones can be treated under the symposium. Although the word "theory" has been included in the title, I believe that it has not been the intention to include reports on empirical investigations if they throw some light upon the mechanisms involved. Furthermore, the papers to be presented could deal either with the results of original research or with a review of past researches.

I wonder if you would be good enough to prepare a paper on the theory of extratropical cyclones for presentation at the meeting. Any contribution that you might be willing to make will be highly appreciated.

An early response to this letter would assist me in organizing the symposium.

Yours sincerely,


SVERRE PETERSSIN

THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF METEOROLOGY

May 4, 1953

My dear Colleague:

The Executive Secretary of the American Meteorological Society has informed me that the Program for the forthcoming Joint Meeting with the Royal Meteorological Society will have to be printed and circulated at an early date. He has asked me to request the speakers on the Symposium on Modern Cyclone Theory to send him, preferably by 15th May, the precise title of these contributions together with an abstract of the paper to be presented.

Would you kindly send the Executive Secretary the requested information and send me information copies.

Thank you very much for your kindness in contributing to this symposium which, I am sure, will attract wide attention.

Yours sincerely,



SVERRE PETERSEN

I am planning with 30 minutes for you

April 30, 1953

Professor Sverre Petterssen
Department of Meteorology
University of Chicago
Chicago 37, Illinois

Dear Professor Petterssen:

I am glad to be able to tell you that I can now make definite my acceptance of your invitation to lecture at the Toronto meetings in September. We have finally succeeded in predicting cyclogenesis, and I think that what I will have to say may be of some interest.

It appears that I have been appointed Chairman of the Committee on Encouragement of Meteorological Research of the AMS. I feel somewhat at a loss because except for the conversations I have had with you I know nothing about the operations of the committee. I would be very obliged therefore if you could in some way bring me up to date on the committee's activities under your chairmanship. It would be very helpful, for example, if you would let me have copies of your official correspondence. If you wish to keep them for your own records, I would be glad to have them ozalided here and then returned to you. Also I do expect to be in Chicago in July and perhaps there will be an opportunity to discuss matters then.

George Platzman is going to try to make a barotropic forecast using as initial data vorticities which your group will have computed. I think this is an excellent idea and will be very interested to see the outcome.

With best regards, I am

Yours sincerely,

Jule Charney

JC/cb

THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF METEOROLOGY

March 30, 1953

Dr. Jule Charney
Electronic Computer Project
The Institute of Advanced Studies
Princeton, N. J.

Dear Charney:

Thank you for your letter. I am glad to think that there is still some hope that you will contribute to the Toronto Symposium.

What I hoped I could persuade you to do was to give an historical account of various theories of cyclone formation leading up to and including your present thoughts and recent results (if any). This, I am sure would be extremely interesting even if you should not have any spectacular results from the work you are now engaged in.

The word "theory" above is meant to be theory and not a sort of semi-theory which is essentially descriptive. There will be others who will cover the descriptive side.

If this does not appeal to you, I shall be glad to accept any contribution you might wish to give. The main point is that your name is on the program.

I tried to do some diplomatic work in Washington on my way back from Princeton. It looks as if the U. S. Weather Bureau will take an initiative and develop a program for operational tests and use of numerical techniques and at an early date firm up arrangements with the AWS and the Navy. I do not think that we need worry about any back-door approach.

With kind regards and many thanks for the enjoyable evening in Princeton,

Yours sincerely,


SVERRE PETERSEN

SP:edm

Hq Air Weather Service
Andrews Air Force Base
Washington 25, D. C.
19 October 1950

Dr. Jule Charney
Electronic Computer Project
The Institute for Advanced Study
Princeton, New Jersey

Dear Charney:

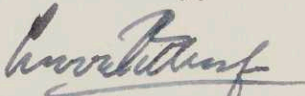
Thank you for your letter of October 11. I would have liked very much to come to Princeton to hear Professor McVittie's lecture, but unfortunately I have to be in Alaska next week.

I appreciate your courtesy in inviting me, and I hope that I may have the privilege of attending similar lectures in the future.

We enjoyed our visit to Princeton very much, both from a professional and social point of view. The results were truly astounding and I wish you and Ragnar the best of luck in the next experiment.

With best regards,

Yours sincerely,


Sverre Petterssen

October 11, 1950

Dr. S. Petterssen
Directorate of Scientific Services
Headquarters Air Weather Service
Andrews Air Force Base
Maryland

Dear Dr. Petterssen:

We are pleased to invite you and any other interested members of your group to attend a lecture by G. C. McVittie, Professor of Mathematics, University of London, on "Coordinate Systems in Dynamical Meteorology". The lecture will be held in the Institute for Advanced Study lecture hall Monday, October 23 at 4:00 p.m.

Tea is served in the lounge at 3:30 p.m.

Sincerely yours,

Jule Charney

JC/cp

HEADQUARTERS
AIR WEATHER SERVICE
ANDREWS AIR FORCE BASE
WASHINGTON 25, D. C.

IN REPLY REFER TO:

20 December 1949

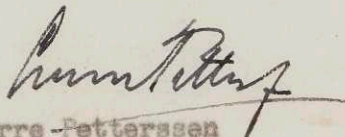
Dr. A. F. Spilhaus *Instrumentation*
Dr. Harry Wexler *Operational Meteorology*
Dr. E. Wendel Hewson *Boundary Layer*
Dr. J. G. Cherney ✓ *Dynamical Meteorology*
Dr. E. Palmén *Synoptic Meteorology*

My dear Colleague:

With reference to earlier correspondence concerning the Symposium on Progress in Meteorological Research to be held at the forthcoming anniversary meeting of the American Meteorological Society, I am pleased to inform you that your contribution to the symposium is scheduled for the morning session on January 6, 1950.

I have informed Father Macelwane that about forty minutes will be required for each contribution to this symposium.

Yours sincerely,



Sverre Pettersen
Chairman of Committee on
Encouragement of Meteorological Research