

11 Old Stage Court  
Rockville, MD  
13 September, 1992

Dear Dr, Persson,

You may be correct about my not seeing the first part of your questions.

I have heard the same thing about Rossby's being "blacklisted" by the 1925 U.S. Weather Bureau. The situation changed completely after Reichelderfer became Weather Bureau Chief. One must recall that Reichelderfer also was a bother to the Bureau of 1925 or so with his notions about fronts and other ideas that he picked up during a visit to Scandinavia. Early informal papers by Rossby and Reichelderfer from that era can be found today in the NOAA library, a short distance from my house. I knew both of them rather well. Reichelderfer became the recipient of scorn from some circles during the early post-WWII years. I think it was because the Weather Bureau was not well supported financially by several administrations and did not have the funds to satisfy the wishes of some would-be contractors on the outside of government. Rossby and Reichelderfer had a very good respect for each other. I became well acquainted with both, after the war and can assure you of this.

Why did Rossby leave the U.S.A.? I can only speculate on this. In the immediate post war years he maintained his European contacts, including at least part of a summer in Innsbruck with Ertel. (Did he not also make an earlier visit with Ertel, or do I have the time wrong?) Rossby was essentially a restless person. I think that if he thought he was going as far as he could in a given line of development of his thought, he would seek stimulation by going elsewhere. I am sure that in the early 1950's he was thinking of starting a meteorology group at the American University in Beirut. It seemed to me that Chicago was not one of the best places to live in the U.S.A. and that was part of the reason I left there as soon as I received my degree from Rossby. (Beirut turned out to be much worse!)

Perhaps Rossby feared a lack of continuing financial support. Horace Byers is still living in California, and might be able to help you on that question. You can get his address through the American Meteorological Society. Don't delay. Horace is quite old.

Why did the Americans linger in 1949-53? I don't think we did. You remember the piece by Charney, Fjortoft and von Neumann in 1950 (Tellus). They did a barotropic forecast for a small area with a mesh length of 763 km on the U.S. Army Eniac. Such an enormous mesh length wouldn't be interesting at all now, and would hardly have produced anything of value to a forecaster. We had to wait until von Neumann's "Johnniac" was ready. Charney published his first forecasts on that in 1954, as soon as the Johnniac was available. If you say that the Americans lingered, I would say we didn't linger, but rather, were diverted by the possibilities of

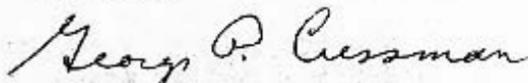
baroclinic forecasting without having thoroughly explored barotropic forecasting. We worked much too hard to be thought of as lingering.

What were Rossby's visions about the future? In his last year at Chicago, he often mentioned the atmosphere as a milieu. Wasn't this a very advanced notion in 1950?

You mentioned hydrodynamics vs. numerical methods in the integration of the non-linear differential equations. We learned that neither could be neglected. I never heard Rossby talk of an automatic weather service.

Rossby had several Chinese students. The ones I knew were Hsieh Yiping and Ye DuJeng. There was also a young lady, whose name I don't remember. Ye and Hsieh both returned to China and suffered much during the cultural revolution, especially Hsieh. I don't know what happened to the young lady. Afterward, Ye rose to a very high position in their Academy of Sciences and Hsieh was Chairman of the Meteorology Department at Beijing University. They are both alive. I visited them on a number of occasions during two stays in China, and was visited in my home in Maryland by Ye a little ~~more~~ *more than a* year ago.

Sincerely,



George P. Cressman

11 Old Stage Court  
Rockville, MD, 20852  
28 Sept., 1992

Dear Mr. Persson,

Your letter of September 8 is not so easy to answer as clearly as I should like. Recently I attempted to answer another inquiry on Rossby's views on another subject. At this distance in time it is too easy to confuse my earlier views with Rossby's because he had such a profound influence on me. In any case I will do the best I can.

My full time contact with Rossby in the years after WW II at the University of Chicago was limited to the years 1945 - 1949. Rossby hired me to be responsible for a current weather synoptic laboratory, not so much as a part of the Department's teaching program, but as a part of the research program. He made it clear that he expected most of the Department staff to show up for daily map discussions. His idea was to temper theory with reality.

In those years at Chicago there was little discussion of P. E. modelling vs modelling with filtered equations. Rossby seemed to me to be more concerned with developing a broad understanding of atmospheric phenomena rather than with a specific predictive approach and was very interested in filtering as a means to better understanding. However, I believe it safe to say that his approach was centered on understanding many of the atmospheric phenomena starting from a barotropic point of view. At times, this seemed to me to be a somewhat deliberate provocation to Palmen and others, who were more interested in baroclinic processes. That was characteristic of the way Rossby worked to challenge other people to greater efforts. As we know, in the range of a few days, many of the large scale changes are largely barotropic.

The question of whether or not Rossby was a good synoptician is wrongly formulated. He worked as one in his younger years at Bergen, but was not a "pencil and eraser" type. He preferred to think rather than to draw. He paid me to draw.

I was not aware of any shyness he may have had for the British, although I can't remember any British visitors at Chicago for extended periods while I was there. I can't believe he was intimidated. Perhaps he just had better personal contacts elsewhere. Regarding funding, Rossby was not particularly interested in accounting. He hired Horace Byers to take care of administrative matters.

I hope this is of some help to you. With best wishes,

George P. Cressman

11 Old Stage Court  
Rockville, MD, 20852  
November 19, 1992

Dear Dr. Persson,

In reply to your letter of September 10, I have no exact memory of when Rossby may have mentioned the possibility of a barotropic model. It seemed clear to me that Rossby was thinking barotropically when he so clearly encouraged the use of his long wave propagation equation and constant absolute vorticity trajectories in forecasting. By contrast, Palmen was more interested in baroclinic events. This difference in outlook added more spice to our daily map discussions in Chicago. As you may know, Namias made some use of these concepts in five day forecasting starting in the late 1930's, although he had to have used some empirical factors to bring the answers into reality, since he used the 700 mb (or 10,000 ft.) charts for his data.

Now, I will try to answer your numbered questions.

1. I heard that Rossby was "blacklisted" by the Weather Bureau in his early days in the U.S. Reichelderfer enjoyed a similar, if lesser, distinction before he was made Chief. Both of them were looked on as "reformers", bringing disturbing new ideas from Europe. Then, Reichelderfer was appointed Bureau Chief and the situation changed completely. Reichelderfer and Rossby respected each other.
2. I don't know why Rossby left Chicago when he did. He tended to become restless after several years in one place and liked to change his location and agenda from time to time. (I didn't like to live in Chicago at that time, being on a small salary and not able to afford the living conditions I thought my small family ought to have). The Rossbys had a pleasant apartment very near the University. I had the impression that Rossby left partly to get a change in agenda and also to return to Sweden. He had talked about the possibility of moving to the American University in Beirut and might have been bored and in need of a change in interests.
3. I don't know enough about the politics of starting NWP in Sweden to be of any help to you.
4. We Americans were on our own schedule and were not influenced in operational decisions by events in other countries. The initial experiment on the ENIAC was just that, an experiment. The experiment, being successful, could be viewed as a feasibility study. The ENIAC was not adequate for an operational start and was not available except for an early experiment. The subsequent American experiments on the "Johnniac", at the I.A.S. at Princeton had to wait until the machine was ready. I was there at the time.

5. I think that I absorbed from Rossby a desire to be able to envision the atmospheric events in terms of processes; barotropic ones that could be relatively simply described and baroclinic processes that interrupted the barotropic events. In my opinion, we still have a long way to go in this respect. You might recall some similar remarks of Fjortoft in Taba's book of interviews.

6. I am not aware of Rossby's opinions on the subject of "automatic weather services."

7. and 8. Rossby, as you know, had some very good students from China. Y. P. Hsieh and T.C. Yeh were at the U. of Chicago when I was there. They returned to China and did very well until the Cultural Revolution happened. It was a disaster for both, especially for Hsieh. It also retarded Chinese meteorology for at least a decade. Both of them are rehabilitated and retired now.

9. See paragraph 2.

10. You have a good description of Rossby. I don't know of that causing friction with the "establishment".

I have written a history of forecasting, with emphasis on the U.S. scene. At the present there are no definite plans to publish it. I did it at the request of the A.M.S. for presentation at the 1992 National Meeting. It is rather lengthy and expensive to duplicate. The A.M.S. might publish a book on meteorological history in 1993. If so, my piece might be in it.

Sincerely,

*George P. Cressman*  
George P. Cressman

11 Old Stage Court  
Rockville, MD, 20852

November 23, 1992

Dear Dr. Persson

Your recent letter concerned the topic of using a forecast for the first guess for a new analysis. At least, in the U.S., the origin of this goes back many years. In doing analyses by hand, a long standing practice was to place the plotted, unanalyzed chart on a light table, superimposed on the previous analyzed chart. This was usually practiced in a weather station responsible for analyses over sparse data areas, such as oceans. I don't know when this started, but cannot remember seeing a light table in a weather office before World War II. An obvious benefit of this practice was to maintain better continuity of successive analyses.

When numerical weather prediction and objective analyses came along, it seemed quite natural to start with a first guess from a previous forecast for the same time. It did not seem like an innovation, but rather as a sort of continuation of a well known practice. There was a trap to be avoided in this procedure. If there was a systematic error in the forecast, such as retrogression of very long waves, the results could be very bad indeed, since the error would continue to grow through successive analysis-forecast cycles. Ad hoc methods were devised to deal with this matter. At the beginning, bogus data were inserted to control systematic errors. Gradually, improvements in the data and the forecast methods greatly reduced the necessity for bogus reports. I don't know whether or not bogus data are still used except to correct obviously deficient data..

Sincerely,

  
George P. Cressman

11 Old Stage Court  
Rockville, MD, 20852  
April 12, 1993

Dear Dr. Persson

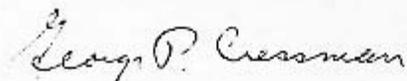
Your letter of July 4 brought back memories of interesting times. In comment on the question of predictability, I remember clearly a conversation I had with my father (an educator) in about 1939. At the time I was a student in physics at Penn State College, now a university. I told him about a course in meteorology I had taken under professor H. Landsberg. I went on to say I thought I would like to make meteorology my life's work. He then asked me whether meteorology was a suitable kind of scientific work for a trained physicist. I replied that the equations that controlled atmospheric processes were basically known, but not solved in any predictive manner, and that meteorology ought to be solvable in time. He raised no further objections.

Wiener's views on the weather prediction problem as expressed in 1956 came after Charney's "Numerical Prediction of Cyclogenesis "(1954) was published, to which he did not refer. Do you suppose he didn't know about it?. Wiener's views had some influence here in the early 1950's. The evidence was that a vocal group with MIT connections proposed a new meteorological system (to be known as project 433-L).For prediction, statistical methods were to be developed and employed. This seemed to me to be the bold launching of competition that would surely draw funds and support from the continued dynamic prediction efforts that were almost continually gaining in usefulness. Well, now we know how it all worked out.

I am sorry you feel you read despair into my 1958 paper. I can assure you that was not the case. Personally, I felt the problem of very long wave regression was reduced to unimportant dimensions for the time being. Actually, it was characteristic of all the filtered equation models we used, being a problem of proper representation of the mutual adjustment process between the motion and mass fields. It disappeared only when the primitive equation models were developed and used.

In addition to all that, could you really think that a physicist would capitulate and yield to statistics at such a point?

With best wishes,

  
George P. Cressman

11 Old Stage Court  
Rockville, MD, 20852  
May 23, 1993

Dear Dr. Persson

Your letter of April 18 brought back many memories. However, I think you may be over emphasizing the possibility of loss of support for operational NWP in the years 1957-58. Neither Fred Shuman nor I remember this as a danger posed by higher levels, which would have to have been Dr.Reichelderfer or Generals Moorman or Peterson, especially. They were not the sort of people to capitulate easily. Reichelderfer was himself an innovator in his earlier days and had had problems in being ignored by the establishment. Dan Rex, whom I knew well, was the key to Navy support, and surprises from that quarter seemed unlikely. Forget Irving P. Krick. He was irrelevant in this matter, as in many others.

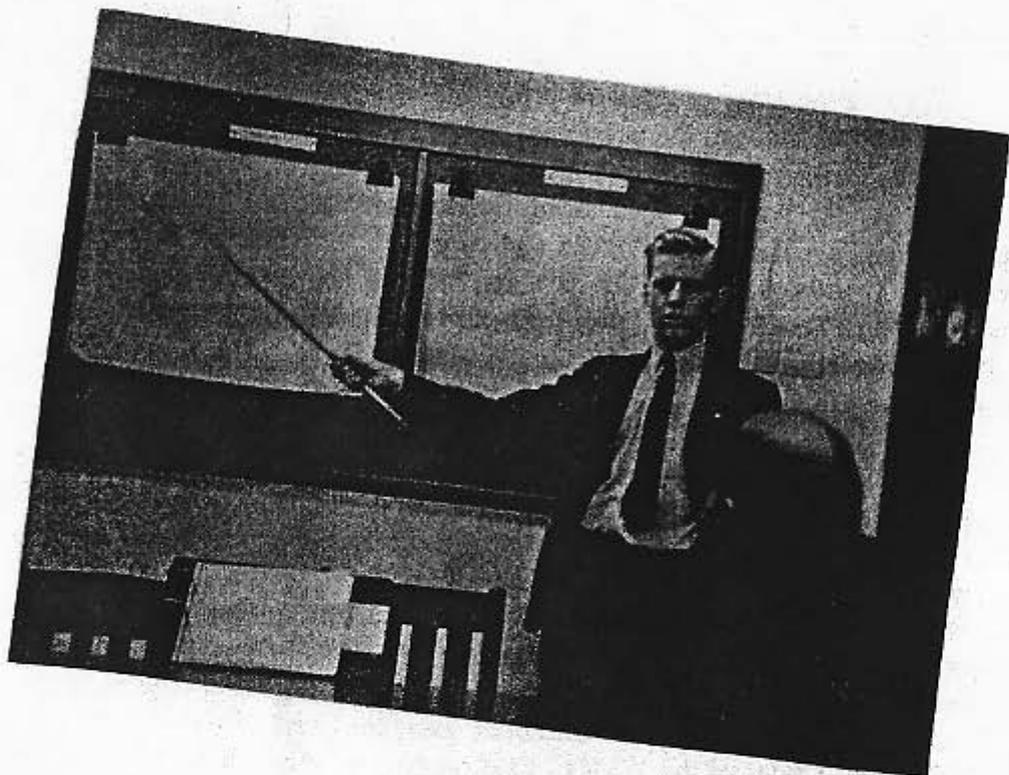
One rather difficult matter you have not mentioned yet was the problem of automatic data decoding and entry. This was a real mess, due to the deplorable coding practices and lack of discipline on the meteorological circuits. I recommend for your reading the paper in MWR of October '57; "An Experiment in Automatic Data Processing" I wrote with Art Bedient. The idea of this paper was to embarrass the meteorological community so as to obtain some remedial action. However, the meteorological community was at least partly embarrass-proof. Such matters were not considered to be worth the attention of professional people. To accelerate progress I had myself placed on the CSM Working Group of Codes of the WMO. That group turned out to be flexible and became interested in the problem. The names Duverge (France) and Ryshkov (USSR) remain in my memory. We ended up with a substantial overhaul of the coding practices which eliminate much grief from our daily operational work

Returning to the matter of Norbert Wiener, would you say that the practice of ensemble forecasting vindicates his early views? Perhaps in principle, although I know of no way we could have got to ensemble forecasting if he had been more convincing at the time. Fred Shuman, who was an M.I.T. graduate, remembers M.I.T. as not being supportive of N.W.P.intellectually despite the fact that Charney was from M.I.T. Perhaps I mentioned this to you in an earlier letter. The views of Wiener must have been instrumental in generating the early competition to the dynamic approach by a group with M.I.T. background who affiliated themselves with Travelers Insurance Co. and proposed a large R and D effort in statistical forecasting. This effort paid off when they moved out of competition with dynamic forecasting methods and invented Model Output Statistics.

Now we can celebrate the success of all our work by savoring the forecast of the big blizzard of 1993 on the U.S. East Coast. This had many similarities to the storm of November 1950, which was the one successfully forecast (post hoc) by Charney on the Johnniac computer at Princeton. *Two days in advance a blizzard forecast was issued for to the public for the right time and the right place for the 1993 storm, while the 1950 storm was a complete surprise even as it happened.* Actually, even the

four day forecast hit the 1993 storm beautifully. See the NMC Office Note 392 by Tracton and Kalnay. No doubt ECMWF was equally successful.

*George Cressman*  
George Cressman



11 Old Stage Court  
Rockville, MD, 20852  
November 28, 1993

Dear Dr. Persson,

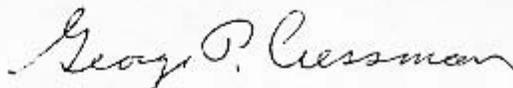
In your letter of a few months ago, you mentioned the map discussions in Chicago during the time Rossby was Chairman of the meteorology department. As I probably mentioned in a previous letter, I was hired by Rossby at the end of the war, specifically to run a synoptic laboratory and to conduct a daily weather program of analyses, forecasts and map discussions as part of the academic program.

In your last letter to me you mentioned map discussions in Swedish, which would have been impossible for me, since I knew practically no Swedish. On occasions, the participation by staff members included some sentences in Swedish when somebody like Bergeron was visiting, but that was all. Perhaps you would be interested in the enclosed copy of a photograph showing me presenting the map discussion and Rossby listening. In the photo, Rossby looks asleep. I can assure you that he wasn't. Horace Byers used to say that Rossby's secret of success was to look detached or asleep when he was actually listening very carefully. I enclose another photograph of myself, of recent vintage. Unfortunately, the resemblance to the other photograph is not very good.

A few months ago I found a Rossby reprint, which I can't find now. I believe it was from his 1939 piece in the Journal of Marine Research. In this paper he seemed to see too many difficulties in the way of numerical prediction. The reasons he gave related to the lack of sufficient data for the initial analysis. (When Rossby asked me to come to Chicago at the end of the war, he said that he wanted me to extend the geographical extent of daily weather analysis as far as could be done reliably.)

Later, Wiener used a similar argument based on inadequate initial data, but rather differently, as you know. I think that the seeming coincidence of their views on this matter was more apparent than real. The data base in 1939 was quite inadequate for a three dimensional analysis of large areas, lacking fast and reliable communications, lacking any credible base of commercial aircraft reports, surface based or satellite soundings over sparse data areas, and lacking a credible approach to the analysis problem. Rossby knew this. Wiener wasn't very familiar with such matters, as far as I can tell from his papers.

With best wishes,



George P. Cressman

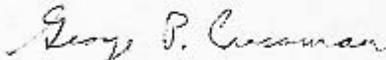
11 Old Stage Court  
Rockville, MD, 20852  
November 6, 1994

Dear Dr. Persson,

Thanks for your letter of October 10, 1994. It gives me a few problems to send you a completely frank answer since I knew all the parties involved and Sutcliffe and I were friends, although I didn't see him after about 1970. E. Knighting visited us in Washington at the JNWP Unit for about a year and a half in 1958 and 59.

It seems indisputable that the British lagged in some aspects of numerical weather prediction. For one thing, if you have only a small computer available, you can think of numerical prediction only in terms of barotropic forecasts if you need to treat a sufficiently large area. If then one is inclined to think that a two layer quasi-geostrophic model has some ability at baroclinic forecasting, which they apparently did, one may tend to ignore the possibilities of barotropic forecasting. If I recall correctly (my reprint library is not as complete as it used to be) the British were mainly interested in a two layer quasi-geostrophic model. In common with all such models theirs had a term in which the term ~~development~~ appeared. It was often referred to as a baroclinic term, but in fact had nothing to do with development, being simply an artifact of the mathematics employed.

The stubborn aspect of the British character is trying at times, but was an admirable quality when it came to important matters such as the threat to civilization from the continent.

  
George P. Cressman

11 Old Stage Court  
Rockville, MD, 20852  
3 March, 1995

PERS.WPS

Dear Dr. Persson,

Your letter came at a good time for me, as I have finished my draft for inclusion in a book to be published by the American Meteorological Society. I don't know when it will appear, although the editor thinks it may be within a few months. I am not quite so optimistic. The book will be a compilation of several historical papers. In any case, I am enclosing a copy of a few pages which may interest you.

Your latest letter, touching on the concept of group velocity, is of considerable interest to me, as it relates to a matter I have been thinking of for a long time. The latest piece in BAMS, v75,n7, relating to the U.S. East Coast storm of March 10, 1993, brings this to my interest again. The BAMS piece would be much more interesting if it had gone more into the setting of the stage for the storm. It used to be known that some major baroclinic upper air events are partly a consequence of previous barotropic events, such as a down stream increase of long wave amplitudes, proceeding downstream with the speed of the group velocity. Once this event is under way, a large conversion of existing available potential energy to kinetic energy may take over subsequent developments. Perhaps the interest and excitement of a large baroclinic event tends to overshadow the barotropic processes that also are involved, both before and after the large scale energy conversion. The latest BAMS piece didn't go into this type of consideration as far as I could have wished.

Perhaps, with the remarkable success now being achieved in prediction efforts, one is inclined to forget such matters. I don't know whether anybody thinks about barotropic processes any more. Personally, I think the barotropic *mise en scene* is an important process that often precedes a baroclinic event and certainly shapes the event, followed by new barotropic consequences farther downstream of the energy conversion.

One could argue that this is of no importance, as the successful forecast is a natural consequence of the equations used in a modern prediction model. One could also drive a car without having any idea of how it works. Or, as Rossby once replied to the question "What good is it?"---"It isn't any good, but isn't it interesting?"

Yes, you may use the photograph of Rossby and me. No problems.

Sincerely,

George P. Cressman

11 Old Stage Court  
Rockville, MD, 20852  
March 27, 1995

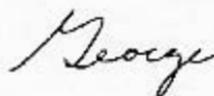
Dear Anders,

Thank you for your two recent letters. I received the letter you didn't send as well as the letter you sent. In reply to your inquiry, I was in Stockholm, at Rossby's request, for a three week period in 1953. I don't remember the details, but do remember presenting a series of lectures at the University.

According to some notes I have, the NWP group at Princeton consisted of Charney and Eliassen at that time, and in late 1953 they produced the famous numerical forecast of the east coast storm of November 24, 1950. In December, 1953, I moved to Princeton to join the pre-operational NWP group already forming.

Mrs. Cressman and I are going to leave in late April to spend about a month and a half in France, and I will not be able to answer correspondence for a while.

With best wishes,



11 Old Stage Court  
Rockville, MD, 20852  
August 20, 1995

Dear Anders,

Thank you very much for your essay on downstream development, which brought back many pleasant memories. I think you have covered the matter very well and have no further comments at this time. No doubt you have seen the recent papers on the Super-storm and blizzard of 1994 on the U.S, east coast. The interplay between barotropic and baroclinic processes was of great importance for that development. From the standpoint of a resident on the U.S. east coast, the forecast was of unprecedented accuracy and value. I recently had a letter from a formerly highly regarded meteorologist living in the Rocky mountain area, who still thinks that forecasting is not showing any improvements. Perhaps I persuaded him a little.

I think you would enjoy reading "*A History of the United States Weather Bureau*" by Donald R. Whitnah(1961) University of Illinois Press, Urbana, for his account of the problems and efforts to reform the U.S. Weather Bureau which took place in the 1920's and 30's as a consequence of many aviation disasters as the Weather Bureau tried to cope with the problems of aviation forecasting. This eventually resulted in the gradual adoption of the Norwegian frontal analysis here, as well as in eventually bringing Rossby and Sverre Pettersen to this country. Perhaps the ECMWF library could find a copy if it doesn't already have one.

With best regards,

